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Contents

Foreword	5
Thórhallur Eythórsson. Three Daughters and a Funeral: Re-reading the Tune Inscription	7
Martin Findell. The Germanic Diphthongs in the Continental Runic Inscriptions	45
Michael P. Barnes. The Manx Runes and the Supposed Jæren Connection .	59
K. Jonas Nordby. <i>Rád þat</i> , If You Can!	81
Rikke Steenholt Olesen. Runes about a Snow-White Woman: The Lund Gaming-Piece Revisited	89

Short Notice

James E. Knirk. N 244 Helland II: A Topsy-turvy Runestone	105
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Debate

Bernard Mees. Early Runic Metrics: A Linguistic Approach	111
Edith Marold. Entgegnung zu Bernard Mees: „Early Runic Metrics: A Linguistic Approach“	119
Svante Fischer. Finsta i Skederid (U ATA3916/47)	125
Magnus Källström. Finstastenen i Skederid — ett genmäle	135

Review Article

Nancy L. Wicker and Henrik Williams. Bracteates and Runes	151
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Reviews

Solveig Möllenberg. <i>Tradition und Transfer in spätgermanischer Zeit: Süd-deutsches, englisches und skandinavisches Fundgut des 6. Jahrhunderts.</i> Reviewed by Martin Hannes Graf	215
Lena Peterson. “ <i>En brisi vas lina sunn, en lini vas unar sunn ... En þa barlaf ...</i> ”: <i>Etymologiska studier över fyra personnamn på Malsta- och Sunnåstenarna i Hälsingland.</i> Reviewed by Staffan Fridell	219
Contributors	223

Foreword

This third volume of *Futhark*, unfortunately again somewhat delayed, contains a new section entitled “Debate”, which was heralded in the “Foreword” to the previous issue. There are two debates, each encompassing a pair of articles. The first of these polemics was sparked off by an article in volume 2 of the journal, whereas the second one takes its point of departure in a topic raised elsewhere some years ago. Further contributions to this section, as well as to the section “Short Notices”, are encouraged, in addition to regular articles, of course.

In this volume there is again also a review article, this time resulting from the publication of the final, evaluative tome of the series *Die Goldbrakteaten der Völkerwanderungszeit: Ikonographischer Katalog*, where all the Migration Period bracteates have been published. This publication, especially the final volume, is of great importance also to runic studies, which will be obvious not least in the length of the review article.

The editors would again like to take this opportunity to thank the peer reviewers for their invaluable contributions to the quality of articles in the periodical. Obviously, many of them have spent days laboring intensely on this task; their pointed comments and questions have led to great improvements in the final products. One peer reviewer for volume 2 of *Futhark* even queried why an article had been published that was different from the one he had received to review — so much had it changed in the rewriting and editing process.

In particular, the editors would like to express their heartfelt gratitude to Michael Barnes for his untiring efforts to disambiguate and otherwise improve the English language and style in contributions to the journal. He is now the English language consultant for the periodical and, in addition to his suggestions for refinements of expression, contributes actively in the editorial process concerning the contents of the pieces.

Purchase of the printed version of *Futhark* is encouraged, especially by institutions. Back copies of the first two issues are also still available at a very reasonable price.

James E. Knirk

Henrik Williams

Three Daughters and a Funeral: Re-reading the Tune Inscription

Thórhallur Eythórsson

Abstract

This paper contains a new analysis of the runic inscription on the Tune stone, made on the basis of autopsies and various earlier proposals. While I agree with the view that there is a word missing at the top of side A (contra Grønvik 1981 and others), probably **r⟨unoz⟩**, I depart from the current *communis opinio* in proposing that side B consists of two independent subject-initial clauses. I argue that the first word in B1 is likely to be a personal name ending in *-z* and the subject of a verb meaning something like ‘erect’, of which **staina** ‘stone’ is the object. Moreover, I reject the analysis of **dalidun** in B2 as ‘made (nice), prepared’ (Seip 1929), presenting arguments supporting the emendation **da⟨i⟩lidun** (Bugge 1891, in *NlæR*), thus giving **prijoz dohtriz da⟨i⟩lidun arbija** ‘three daughters shared the inheritance’. Finally, I resuscitate the old idea of Läßler (1892, 1896a, 1896b) concerning **sijostez**, taking it at face value and considering the phrase **sijostez arbijano** to reflect an archaic legal term meaning ‘the closest family heirs’. Following Läßler I assume that the form is derived from a reflexive (rather than a root meaning ‘bind’, Bjorvand 2008), an analysis supported by a parallel in archaic Latin (*suus heres* ‘family heir, self-successor’). I conclude that the three daughters of Wöduridaz shared the inheritance as the closest family heirs, while some other person (perhaps Wīwaz) erected the stone.

Keywords: inheritance, pre-Viking women’s rights, legal language, runic epigraphy, older runic inscriptions, Old Germanic, Indo-European

Introduction

Much controversy has surrounded the reading and interpretation of the best known of the older runic inscriptions from Norway, the one on the stone from Tune in Østfold, southeast of Oslo, traditionally dated to

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A.D. c. 400.¹ According to a recent study based on the typology of rune forms and the archeological dates of comparative material, a dating from A.D. c. 375/400 to 520/530 is possible (Imer 2011, 205). The stone was built into the graveyard wall by Tune church until 1850 when it was moved just outside the wall. In 1857 the stone was sent to Oslo (then Christiania), where it stood in the university yard for over eighty years. After that it was kept in a storage room for half a century, but in 1991 it was put on exhibition in the Historical Museum in Oslo. The stone, of reddish granite, is over two meters high and quite impressive. The runes are carved on two sides, called A and B. The Tune inscription has been studied and discussed intensively ever since P. A. Munch (1857) was able to read the first words. The major contributions to the study of this runic document include Bugge (1891, in *NIæR*), Marstrander (1930), Krause (1966, 1971), Antonsen (1975) and Grønvik (1981). In addition, various aspects of it have been discussed in a number of minor studies, some of which have yielded important insights. The history of the reading and interpretation of the Tune inscription is an interesting subject in itself, and is documented in detail in Grønvik's 1981 monograph (to which Grønvik 1987, 1994 and 1998 are supplementary). The whole debate confirms the First Law of Runo-dynamics, attributed to David M. Wilson (Page 1987, 10), that "for every inscription there shall be as many interpretations as there are scholars working on it". Even so, it is clear that some readings and interpretations can be shown to be more plausible than others, and this is the justification for the analysis advanced in the present paper. My analysis was made on the basis both of autopsy of the stone itself on a number of occasions between 1996 and 2009, and of proposals by earlier scholars, in particular Bugge, Löffler, Noreen and Jónsson.²

The paper begins with a discussion of side A, the reading of which is rather uncontroversial. The core of the paper, however, contains a detailed investigation of the text on side B, on which most of the controversy has focused. Here I first address the question whether the sequence **dalidun arbija** (B2–3) should be taken to mean 'prepared the funeral feast', as is now the standard view, or be emended to **da(i)lidun arbija** 'shared the inheritance', as earlier scholarship would have it. There follows an analysis

¹ This article was originally delivered as a paper at the Fifth International Symposium on Runes and Runic Inscriptions in Jelling, Denmark, in 2000, but was too voluminous for publication in the proceedings (Stoklund et al. 2006). I would like to thank James Knirk and two anonymous reviewers for their extensive and invaluable critical comments in connection with publication in *Futhark*. Needless to say, I alone am responsible for all remaining errors.

² The existing literature on, and interpretations of, the Tune inscription are conveniently cataloged on the website of the Kiel Rune Project (<http://www.runenprojekt.uni-kiel.de>).

of the much discussed sequence **sijostez** (variously emended) in B3. Finally, I attempt to provide answers to the central questions concerning the content of side B: Who shared the inheritance? Who erected the stone? The conclusion contains a new overall interpretation of the entire Tune inscription.

Side A

The text of the inscription on side A begins at the top, going down from left to right in A1 and then up again from right to left in A2, according to the boustrophedon mode of writing. The standard reading of this side, codified in the handbooks of Krause (1966, no. 72; cf. 1971, 169), is as follows:³

A1: **ekwiwazafter-woduri** →
 A2: **dewitadahalaiban:worahto-ŕ⟨...⟩** ←

This reading is due to Bugge (1891, 6–21), who proposed that the trace of the rune at the end of A2 was **r**, the missing word being *rūnōz* ‘runes’. Divided into words, the inscription on side A can be rendered as follows:

ek wiwaz after woduride witadahalaiban worahto ŕ⟨unoz⟩

‘I Wīwaz wrought the r(unes) in memory of Wōdurīdaz, the breadward.’

The text contains a clause beginning with a complex subject noun phrase, an *ek*-formula consisting of the pronoun **ek** ‘I’ and the personal name **wiwaz** (*Wīwaz*), a masculine *a*-stem in the nominative case, cf. **wiz** (ON⁴ **Vír*) on the Eikeland clasp (Krause 1966, no. 17a; Antonsen 1975, no. 53). The predicate of this clause is **worahto**, first singular past tense, to PGmc. **wurkijana*ⁿ (ON *yrkja*, past tense *orta*), whose basic meaning is ‘make’. This form represents **worhtō*, with an epenthetic vowel **a** breaking up the cluster *-rht-*. The direct object of this verb is the missing word at the end of A1, apparently beginning in **r**, which suggests that it is *rūnōz* (for different views, see below). The phrase ‘make runes’ has a parallel in the inscription on the Tjurkö bracteate: **wurte runoz** ‘wrought runes’ (Krause 1966, no. 136; Antonsen 1975, no. 109).

The form **woduride** is generally taken to be dative of a masculine *a*-stem *Wōdurīdaz*, and I will take that for granted here. This name is not attested

³ In this article, **z** is used as the transliteration for **ȝ** (z for the normalization), and for the sake of consistency here and elsewhere **z** is tacitly substituted for the transliteration **r** employed by many other scholars. Bind-runes are not indicated.

⁴ Old Norse is used for Old West Nordic, i.e. Old Norwegian and Old Icelandic; see the bibliography for abbreviations of languages.

elsewhere, but had it survived into Old Norse it would have appeared as **Óðriðr*. The name occurs with the epithet **witadahalaiban**, dative of a masculine *n*-stem. It is to be read *witandahlaiban*, according to a rule that the nasal dental *n* may be omitted before the dentals *þ* and *d* in runic spelling (Krause 1971, 35). The standard analysis of this word is due to von Friesen (1900). According to this analysis, the form is a compound whose first member is a present participle to PGmc. **witana*ⁿ, evidenced in Goth. *witan* ‘keep watch over, make secure’, ON *vita* in *vitaðr* ‘allotted’ (cf. *sá er þeim vøllr of vitaðr* ‘that field is marked out for them’, *Vafþrúðnismál* 18). The second member is a weak (*n*-stem) noun to PGmc. **hlaibaz* (Goth. *hlaifs*, ON *hleifr* etc.) ‘bread’; the *n*-stem form is conditioned by its occurrence in this particular type of compound. In Old Norse a corresponding form to the one in the Tune inscription would be **vitandhleifi*. Following this analysis the meaning of *witandahlaiba* is ‘the one watching the loaf, breadward’ (comparable to OE *hlāford* ‘[literally] breadward, i.e. lord’), or maybe rather ‘the distributor of bread’. Poetic compounds with a present participle as the first element are attested in the archaic Old Norse compound type *sløngvandbaugi* ‘ring-slinger’ (Cleasby and Vigfusson 1957, 570), which is parallel to Indo-Iranian forms like *vidádvasu* ‘gaining houses’ (Krause 1971, 48).⁵ The prepositional phrase **after woduride witadahalaiban** is an adjunct in the clause. The dative noun phrase is governed by the preposition **after**. Whereas the corresponding preposition in Old Norse (*eftir*, *eptir*) in the meaning ‘in memory of’ is only attested with the accusative, the dative occurs with Old English *æfter* in this meaning (Bosworth and Toller 1898, 10f.; Page 1958, 149–52). It is likely that the dative with this preposition is an archaic feature, while the accusative is an innovation within Old Norse (Grønvik 1981, 146; Syrett 1994, 85). There are word separators before **woduride**, and before and after **worahto**. On the other hand, as in other instances of the *ek*-formula, there is no word separator splitting up the complex noun phrase **ekwiwaz**. Note, finally, the alliteration exhibited by the words **wiwaz – woduride – witadahalaiban – worahto**, and the placement of the finite verb in non-second position (see Eythórsson 2001, 44–46; 2012, 38–40).

As mentioned above, the question whether there is a word missing at the end of A2, and if so what it may be, has been debated in the literature on the Tune inscription. While the reading **r<unoz>** is now generally accepted,

⁵ A different analysis has been proposed by Dishington (2010), according to which **witadahalaiban** means ‘whose estate is planned and certain’. In my view the arguments on which this analysis is based cannot be accepted.

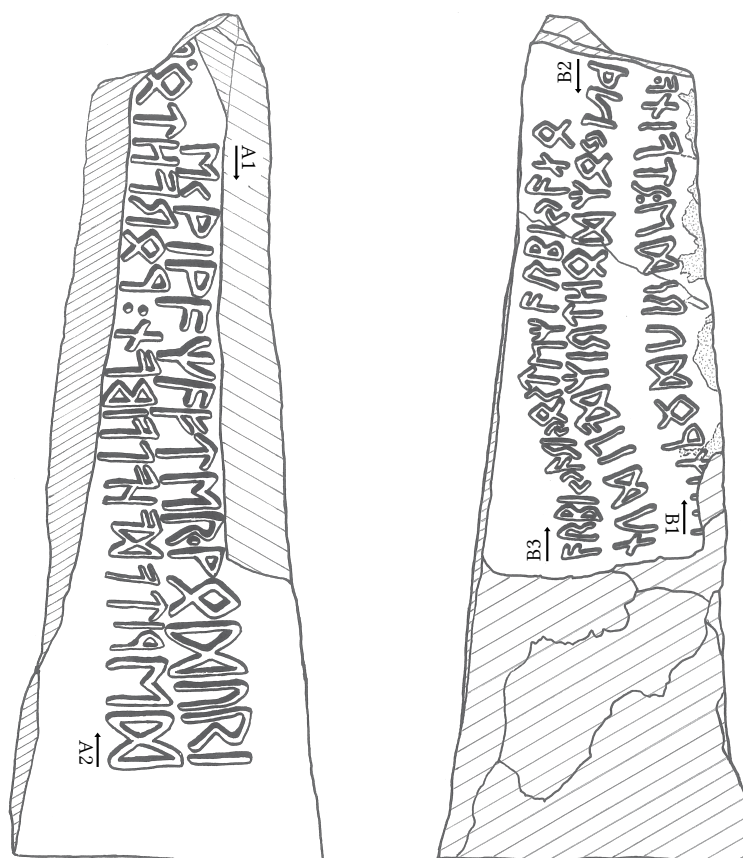


Fig. 1. The Tune stone, sides A and B. Drawing by James E. Knirk.

it may be mentioned, for the sake of completeness, that a different reading was proposed by Marstrander (1930, 325–27). Pointing out that the rune in question could just as well be **w**, Marstrander suggested that the missing word might be **w**<**aru**> ‘grave’ (cf. ON *vǫrr*). Side A would then contain a statement by *Wiwaz* that he ‘made the grave’ (*wor^ahtō waru*) for *Wōdurīdaz* the breadward. This reading, however, is less satisfying than the traditional one because of the lack of parallels to such an expression in the runic corpus, as well as in other Germanic languages. Neither the preposition *after* (or similar forms) nor reflexes of the Proto-Germanic verb **wurkijana*ⁿ are attested in a comparable context elsewhere.

A radically different view was taken by Antonsen (1975, no. 27) and Grønvik (1981, 125–27, 148–52), according to which there is no rune at all

at the end of line A2, hence no addition is required here. Arguing at length against the traditional assumption that there is a missing piece at the top, contemptuously dubbed the “top-piece hypothesis” (*toppstykkehypotesen*), Grønvik claimed that the verb *wor^ahtō* is here used absolutely as ON *yrkja* can be used, and that the Tune stone then is not a “runological torso”.

The epigraphic facts, however, do not support the assumption that the inscription has been preserved intact. Based on personal inspection of the stone on several occasions (as mentioned), I can confirm, along with most other runologists, that there is unquestionably a trace of a rune at the end of line A2. In view of parallels in other inscriptions it seems most likely that the missing word begins in **r**, and therefore Bugge’s 1891 reading **worah^{to} r<unoz>** should be upheld. In fact, Grønvik himself later changed his mind about the reading of side A (1984, 54; 1994, 45–50), conceding that there is a trace of a rune after the form **worah^{to}** and that there would have been room for the word *rūnōz* on the lost top-piece if it were about a foot long (“og det kan det såvidt ha vært plass til på det tapte toppstykket, hvis dette har hatt en lengde på 30–40 cm (sml. Knirk 1991, 106)”; Grønvik 1994, 47 f.).

In conclusion, side A contains a statement by Wīwaz that he made (“wrought”) the runic inscription in memory of Wōdurīdaz, whose epithet is *witandahlaiba* ‘breadward’. While this conclusion is rather uncontroversial, it is necessary to re-iterate it here as the correct assessment of the text on side A will be of importance for the discussion of side B. Most of the controversy surrounding the Tune inscription involves the reading and interpretation of side B, to which I turn next.

Side B

The text in line B1 begins at the bottom, going up to the top, which, as is now generally agreed, is broken off. The word **staina**, followed by a word separator, stands immediately before an even ledge at the top. This is also at the level where the first word, **prijoz**, in line B2 begins. The direction of the text here is first downward, and then boustrophedon up again in B3 (see Grønvik 1981, 127–37, concerning the various boustrophedon patterns):

B1:	<...>zwoduride:staina-<?>	←
B2:	prijozdohtrizdalidun	←
B3:	arbijasijostezarbijano	→

The main problems concerning side B can be summarized as follows. Initially, how much of the inscription is lost? There is clearly a lacuna at the beginning of B1, where there are traces of four (possibly five) staves

before a rune which is generally read as **z**. I will return to this matter below. The question whether there is also a lacuna at the end of B1 is more difficult to answer. It was assumed in earlier scholarship that a word is missing there, but this assumption has been disfavored ever since Marstrander's (1930) seminal paper on the Tune inscription. The issue remains open, however, given that there is now almost universal agreement that the top is broken off (containing the word *rūnōz* 'runes' at end of A). The answer depends to some extent on whether it can be determined that the even ledge at the top of side B was made before or after the runes where carved. If the ledge was already there when the runes were carved, then it would seem less likely — although not inconceivable — that the text would have continued after **staina**. In addition, the analysis of two forms is of crucial importance for the interpretation of the inscription. In the one case, the question is whether the form **dalidun** (B2) is to be taken at face value, as has been the standard view since Seip (1929), or to be emended to **da(i)lidun**, as Bugge proposed (1891). In the other, it must be determined whether the sequence given above as **sijostez** actually contains the runes **si**, as most runologists would maintain, or a retrograde **r**, as Krause (1934, 1937, 1966, 1971) proposed, reading **arjostez** 'the noblest' or **ar(bi)jostez** 'die zum Erbe Nächstberechtigten' (cf. also Antonsen's 1975 reading **arbiarjostez** 'the most legitimate to inherit'). Finally, it must be established whether there are graphic indications that some lines belong more closely together. In this respect, it is important to note the use of word separators in B1 and not in B2–3. This may suggest that B1 belongs more closely with A1–2 than with B2–3 (Grønvik 1981, 137, and elsewhere). This point leads to a further question, namely whether it is possible to determine the order in which the lines are to be read. It is usually taken for granted that the order is A1–2 and B1–2–3. There are reasons to believe, however, that lines B2–3 may just as well be read before B1, as discussed further below (Jónsson 1931, Sanness Johnsen 1969, Moltke 1984). On the other hand, it is unlikely that B1 is a continuation of A2 (Bugge 1903 in *NIæR*, Grønvik 1981, 1984, 1994). Additionally, it has repeatedly been observed that there is a noticeable difference in the shapes of the runes on the two sides of the inscription. The runes on side A are clearly more carefully carved than the ones on side B. The question therefore arises whether the different shapes of the runes in A and B point to two carvers. Despite many attempts, this matter seems to be indeterminable. The undeniable difference in the ductus of the writing appears not to be so significant that it is necessary to assume two carvers; there is also variation within A2.

Sharing the inheritance or preparing the funeral feast?

There is only one finite verb form preserved on side B. This is **dalidun** (B2), evidently third person plural past tense of a weak verb, implying a plural subject. This form has been taken either at face value as *dālidun* ‘made (nice), prepared’, or emended to **da(i)lidun** ‘shared’. The former is unattested elsewhere in Germanic; the latter corresponds to Goth. *dailjan*, ON *deila*, OE *dælan*, OS *dēlian*, OFris. *dēla*, OHG *teilen* (‘divide, separate, distribute, share’), reflecting a verb PGmc. **dailijana* derived from a noun **daili-*, **daila-* ‘part’ (the former seen in Goth. *dails*, and the latter in ON *deill*, OE *dæ̅l*, OS, OFris. *dēl*, OHG *teil*). Depending on the interpretation, its object would be either **staina** or **arbija**, or both. The form **arbija**, reflecting a neuter *ija*-stem **arbija*, is ambiguous in that it may mean either ‘inheritance’ or ‘funeral feast’. The meaning ‘inheritance’ of the word **arbija* has parallels in related words in other Germanic and Indo-European languages: Goth. *arbi*, OHG *erbi*, OE *ierfe*, OFris. *erve*, ODutch *er(e)ve*, OSwed. and ODan. *ærfwe*, Norw. (Bergen dialect) *erve* ‘inheritance’. In Old Norse this word occurs only in the compounds *erfivorðr*, *erfinyti* ‘heir, son’, cf. OS *erbiuuard*, OE *yrfeward* ‘heir’. Outside Germanic a corresponding form is found in Old Irish *orb(b)e* ‘inheritance’ (cf. Grønvik 1982, 10 f.). The meaning ‘funeral feast’, on the other hand, is only attested in ON *erfi* and not in any other language (Grønvik 1981, 177–80, 1982, 5 f.; Antonsen 1986, 329 f.).

Bugge proposed the reading **da(i)lidun arbija**, meaning ‘shared the inheritance, got a portion of the inheritance’ (1891, 27 f., 33). The reading **da(i)lidun** presupposes that the diphthong **ai** is spelled **a**, presumably by mistake on the part of the carver. This has parallels in other inscriptions, in older runes **hateka** for *haiteka* (cf. ON *heiti-k*) ‘I am called’ (Lindholmen, Krause 1966, no. 29; Antonsen 1975, no. 17).

As already noted, the form **arbija** is ambiguous. It could correspond to forms meaning ‘inheritance’, or to forms meaning ‘funeral feast’. It was von Friesen (1918, 14) who first proposed that **arbija** must have the latter meaning on the Tune inscription. In accordance with this analysis, von Friesen argued, the verb **da(i)lidun** must mean ‘shared among themselves’ (cf. ON *deila e-u á milli sín*). So the phrase **dalidun arbija** was taken to mean ‘shared (the expenses for) the funeral feast [verteilt unter sich (die Kosten für) den Erbschmaus]’.

This proposal was considered by Noreen (1923, 390) as an alternative to the one by Bugge; he translates ‘teilten das erbe [oder vielleicht eher die kosten für den erbschmaus]’. Moreover, evaluating von Friesen’s translation, Krause (1926, 235 f.) accepted the analysis of **arbija** as ‘funeral feast’, but

took **dalidun** to mean ‘distribute, hold [austeilen, ausrichten]’, comparing it to ON *deila mat, dogurð* ‘distribute food, breakfast’ (Fritzner 1883–96, 1: 241 s.v. *deila*). Taking the subject to be ‘three daughters’, Krause translated **prijoz dohtriz da(i)lidun arbija** as ‘three daughters held the funeral feast [Drei Töchter richteten das Erbmahl aus].’

A crucial step in the development of the alternative account was taken by Seip (1929). Accepting the validity of von Friesens’s suggestion of **arbija** as ‘funeral feast’ rather than ‘inheritance’, Seip was nevertheless uncomfortable with the notion of ‘sharing the expenses for the funeral feast’, as he seems to have believed that ON *deila* was not attested in the sense ‘share [dele mellem sig]’. He rejected Krause’s translation ‘ausrichten’ on the grounds that it seemed “artificial [kunstig]”, surmising that the context of a funeral feast required the meaning ‘make, perform, hold’ (cf. ON *gera erfi* ‘hold a funeral feast’). Accordingly, Seip proposed that **dalidun** was not to be emended, but was rather the past tense of an unattested verb PGmc. **dēlijanaⁿ* (as if ON **dæla*, 3 pl. past tense **dældu*) ‘do, make’, from which the Old Norse adjective *dæll* ‘gentle, familiar, forbearing’ was derived. He further suggested that these forms were related to Old Church Slavic *dělo* ‘work’, *dělajō* ‘I work (on)’. Accordingly, the translation of **prijoz dohtriz dalidun arbija** was: ‘Three daughters did (i.e. prepared, held) a funeral feast [tre døtre gjorde arveøl].’

Seip’s proposal was embraced by Neckel (1929), who, however, suggested that the putative verb **dēlijanaⁿ* was formed to the adjective attested as ON *dæll*, rather than the other way around, interpreting the relevant passage thus: ‘The three daughters prepared the funeral feast so that people ate and celebrated; they made it *dælt* for relatives and guests [die drei Töchter machten das Erbmahl zurecht, so daß es verspeist und gefeiert wurde, sie machten es *dælt* für Gesippen und Gäste].’

In his classic paper on the Tune inscription, Marstrander (1930, 308–10) adopted Seip’s basic idea. Similarly to Neckel, Marstrander assumed that **dēlijanaⁿ* was derived within North Germanic from an unattested noun **dālaⁿ* (related to the adj. *dæll*). In other respects, he departed radically from previous interpretations, proposing that the object of **dalidun** was not only **arbija** ‘funeral feast’, but also **staina** ‘stone’. This part of the inscription was taken to consist of two asyndetic clauses, the finite verb being omitted in the second one. Moreover, the daughters were considered to be the subject of the first clause only whereas the subject of the second clause would be the (male) heirs (read by Marstrander as **(a)sijostez arbijano**). Further details of this interpretation will be given shortly, but the gist of it is this: ‘The daughters “made” (i.e. erected) the stone but the male heirs “made” (i.e. held)

the funeral feast [(dóttrene) reiste denne sten ..., men arveølet holdt de mannlike arvinger ...]’ (p. 342).

Marstrander’s reading and interpretation of the Tune inscription have become very influential, recurring (with some modifications) in the handbooks of Krause (1966, 1971) (‘... bereiteten den Stein drei Töchter, das Erbmal (aber) die ... Erben’). Antonsen (1975, no. 27) and Grønvik (1981, 180) also accept the interpretation of **dalidun** as ‘make, prepare’. Antonsen, however, takes the object of this verb to be **staina** ‘stone’ only (‘three daughters ... prepared the stone’). Grønvik, on the other hand, assumes that the object is only **arbija** ‘funeral feast’ (‘three daughters made the funeral feast nice [tre døtre gjorde gravølet hyggelig]’), while **staina** is the object of a different verb (see below).

There are several problems concerning the alternative view taking **dalidun** to mean ‘make, prepare’, as was pointed out already by Western (1930). Aside from the fact that there is no evidence for a verb PGmc. **dēlijana* ‘do, make’ in Old Norse or in any other Germanic language, there is the more general question, also raised by Western, whether the successful hosting of a funeral feast is of such importance that it would deserve to be immortalized in a runic inscription. Apart from the fact that no parallels for such an activity are found in the runic material, the answer to this question must in the end depend more on one’s common sense than on strict scientific “proof”.

It should be borne in mind that Seip’s premise was that if **arbija** means ‘funeral feast’, the context requires that the form **dalidun** has the sense ‘make, prepare, hold’. What seems to have been lost sight of in the ensuing discussion is the simple fact that **arbija** can just as well mean ‘inheritance’, as in Bugge’s reading. Given the problems with the alternative view, the consequences of the original idea should be explored again. In this case, the verb obviously cannot have a meaning ‘make (nice), prepare’, whereas ‘divide, share’ would make perfect sense. Thus we must go back to square one, accepting the validity of Bugge’s proposal that **dalidun** is to be emended to **da(i)lidun** ‘shared’. The phrase **da(i)lidun arbija** would mean ‘shared the inheritance, got a portion of the inheritance’.

The two main objections which have been raised against the emendation **da(i)lidun** ‘share’ are, first, that it requires the assumption of a misspelling of **a** for **ai** in an otherwise carefully carved inscription (Marstrander 1930, 307; Grønvik 1981, 92), and second, that the Old Norse verb *deila* with an object in the accusative does not normally have the meaning ‘share’ but rather ‘divide up, distribute’ (Seip 1929, 22; Grønvik 1981, 88, 209 n. 68). Neither of these objections carries much weight, however. On the one hand, as mentioned above, the writing of **a** for **ai** has parallels in other

inscriptions; the omission of the **i** is probably just a spelling error (cf. Krause 1966, 70).⁶ On the other hand, there is evidence that the verb **ðailijana* originally governed accusative rather than dative case. In fact, in Old Norse *deila* is attested with the accusative (as well as with the dative), as in the following passage from “Tryggðamál” in the law-book *Grágás* (*Grágás*, 1: 206.; cf. Fritzner 1883–96, 1: 241 s.v. *deila*, Cleasby and Vigfusson 1957, 98):

Pit scolot deíla kníf oc kiotstycke. oc alla lute yckar imille sem frændr en eigi sem fiandr.

‘You (two) shall share a knife and a piece of meat and all things among yourselves as friends and not as foes.’

In this passage *alla lute* ‘all things’ can only be accusative. The occurrence with accusative can be assumed to be the older usage with *deila* ‘share’. Already in early Old Norse, however, accusative gives way to dative with this verb in accordance with a more general diachronic tendency in the language to expand the domain of dative as the case of the direct object of verbs (Heusler 1932, 115). Moreover, phrases combining reflexes of **ðailijana* ‘divide, share’ and **arbija* ‘inheritance’ are widely attested in old Germanic languages. The following examples are from Old English: *næfre Ismael wið Isāce, wið mīn āgen bearn, yrfe dæleð* ‘never shall Ishmael share the inheritance with Isaac, with my own child’ (Caedmon 2781f.); *ierfe-gedāl* ‘share of inheritance’. Even in Old Norse there is indirect evidence for this combination, although *deila* here has the meaning ‘share in, get a portion of’. Thus, for example, in the *Poetic Edda* the verb occurs with the object *fé* ‘property, money’: *deila fé fǫður* ‘get a portion of the property of one’s father’ (*Skírnismál*, st. 22).

It appears then that problems concerning the emendation of **dalidun** to **da(i)lidun** are only apparent. It is, in fact, perfectly reasonable to assume that on the Tune inscription the verb means ‘shared’. In this case, **arbija** cannot mean ‘funeral feast’, for needless to say ‘to share a funeral feast’ does not make any sense. After almost a century of scholarly debate we have now come back to Bugge’s original proposal to read **da(i)lidun arbija** ‘shared the inheritance’. With this interpretation the word **staina** ‘stone’ is unlikely to be the object of the verb **da(i)lidun**. On the other hand, it would be plausible to assume that **staina** is the object of a missing verb meaning ‘erect (*vel sim.*)’.

⁶ The possibility that the writing of **a** for **ai** is due to a sporadic monophthongization cannot be ruled out entirely, although it is not very likely in view of the occurrence of the form **staina** in B1.

A likely candidate is the verb PGmc. **satjana* ‘erect’, suggested already by Löffler (1892, 5 n. 1), Noreen (1903, 345; 1923, 390), and Bugge (1903, 521–23).⁷ This verb occurs with ‘stone’ in the inscription from Rö, Bohuslän (Krause 1966, no. 73; Antonsen 1975, no. 26): **satido** < s > tain < a > ‘I erected the stone’. Several other possibilities have been entertained, in particular by Grønvik (1981, 1994), as will be discussed further below. An analysis along these lines entails that side B consists of two clauses, each containing a finite verb. In view of this conclusion, the following two questions arise: First, who shared the inheritance? And second, who erected the stone? In order to be able to answer these questions we must try to find out what the subject of the verb **da** < i > lidun ‘shared’ is. But before trying to answer these questions, it is in order to shed some light on the murky sequence **sijostezarbijano** in line B3.

The sequence **sijostezarbijano**: related, divine or lovely heirs?

The sequence **sijostezarbijano** (B3), following **da** < i > lidun **arbija**, is evidently a phrase consisting of two words. The first is **sijostez**, which has been read in various ways: **si** < b > jostez, **(a)sijostez**, **(a)rjostez**, **(a)r** < b > jostez (see Syrett 1994, 89, for some discussion). The second is **arbijano**, genitive plural of a masculine *n*-stem noun **arbijan-* ‘heir’ (cf. Goth. *arbja*, OHG *erbo*, OE *ierfe*; ON *arfi* < **arþan-*, Grønvik 1982, 5).

The form **sijostez** is undoubtedly the most mysterious word in this inscription, besides the ones that are missing. It appears to be an adjective in the superlative with the masculine plural ending **-ēz*. To be sure, it is likely that the phrase **sijostez arbijano** denotes ‘the closest heirs’ or something similar, and therefore the approximate meaning of **sijostez** can be deduced from the context. But as the analysis of this form is not immediately obvious, most scholars assume that an emendation is needed.

Reading **si** < b > jostez ‘the ones most closely related, the next of kin [de nærmest beslægtede]’, Bugge (1891, 34) suggested that the rune **b** had been left out by mistake. This form would be derived from PGmc. **siþja-* ‘related’ (cf. ON *sif*, pl. *sifjar* ‘affinity, connection, by marriage’, Cleasby and Vigfusson 1957, 526). Accordingly, the meaning of the phrase would be ‘the next of kin of the heirs’, referring to the daughters, which of course is quite

⁷ To be sure, Löffler assumed that the form was third plural (**satidun**), presumably referring to the heirs, but Noreen and Bugge posit a singular form. See the discussion further below.

natural in the context.⁸ Marstrander (1930, 310), however, rejected Bugge's reading, mainly on the grounds that an omission of **b** in an otherwise carefully carved inscription was unlikely. Instead, he proposed (pp. 320 f.) that the initial vowel of this form coincided with the final vowel of the preceding word, **arbija**, and that it should therefore be read **(a)sijostez**. This was supposed to be the superlative to an adjective **ansija-* (**āsija-*) derived from **ansu-* (**āsu-*) 'god' (ON *áss*), meaning 'the ones closest to the god [som stod Åsen nærmest]' (p. 342).

While the emendation to **si(b)jostez** may be doubtful, although not demonstrably false, various problems are inherent in Marstrander's own proposal, as illustrated by Grønvik (1976, 159–163; 1981, 111; 1994, 46 f.). In particular, the semantics of the alleged form is forced. In fact, such a form would be more likely to mean 'the most divine'. This, however, would be quite peculiar in this context, given that the superlative would imply that the heirs mentioned in the inscription were in some sense "more divine" than some other party of heirs, not mentioned in the document. Moreover, the word formation would be unusual in Germanic, where such secondary adjective formations are rare (cf. Grønvik 1981, 111; Krahe and Meid 1969, § 74.4). In view of these difficulties, Grønvik (1981, 182 f.) rejects Marstrander's analysis of the form, but retains the reading **(a)sijostez**, claiming that it is the superlative to an otherwise unattested verbal adjective **āsija-* 'lovely [elskelig]', made to the root **ans-* (**ās-*), cf. ON *unna* (1 sg. *ann*) 'love', *ást* (fem.) 'love' (< **ansti-*, cf. Goth. *ansts* 'grace'). Accordingly, the meaning of **(a)sijostez** would be 'the loveliest [de mest elskelige]'.

While Grønvik's criticism of Marstrander's analysis is justified, his own proposal fares no better. The main problem has to do with the formation of an adjective **āsija-* 'lovely [elskelig]' to the root **ans-* 'love', for which there is no other evidence.

More generally, the idea that the text contains information about three daughters, "the loveliest of the heirs", hosting a "nice" funeral feast, suggests a sentimental atmosphere of coziness (*hyggelighet*) which may be appropriate in the setting of a modern Scandinavian welfare state, but seems peculiarly out of place in a pre-Viking runic inscription. In fact, Grønvik

⁸ Grønvik (1981, 118–21, with further references) discusses PGmc. **sibja-* at some length, arguing that it originally only applied to those who are married into the family, not to blood relations, and therefore it could not refer to the daughters (p. 120). This appears to be valid for Old Norse *sifjar*, but in the earliest West Germanic sources the cognate forms are attested with a reference to blood relations (pp. 119 f.). Despite Grønvik's conclusion to the contrary, it seems possible that the West Germanic meaning is the original one and that there has been a semantic narrowing in Old Norse.

(2010, 120–25) himself abandoned his earlier proposal, reverting instead to Marstrander’s earlier assumption that the form **āsija-* is to be related to **ansu-* ‘god’, conjecturing that it could mean ‘connected/associated to (a) god; devoted to (a) god’. This new conjecture is prone to the same objections as Marstrander’s analysis, discussed above.⁹

A further problem with the alleged form **(a)sijostez**—whatever its meaning—has to do with the assumption that it shares the initial vowel with the immediately preceding form, **arbija**. As admitted by Grønvik (1981, 182), there are no certain cases of a double vowel written single in the runic inscriptions, although no counterexamples either. In short, the form **(a)sijostez** appears to be a mirage.¹⁰

The ‘most Aryan’ of the heirs?

A drastic new reading was proposed by Krause (1934). Like Marstrander, Krause assumed a double value for the final **a** of **arbija**, but he interpreted the sequence read by others as **si** as a retrograde **r**. Krause’s reading of B3 is as follows:

B3: **arbijarjostezarbijano** →

The form, as read by Krause, was **(a)rjostez**, a superlative of the word reflected in *Aryan* (German *Arier*), Sanskrit *aryá-* ‘lord’, and allegedly also Old Irish *aire* ‘free man, prince’ (on which see below). The supposed meaning here is ‘the noblest [die vornehmsten]’. Krause surmised that in the Tune inscription it had a “racial flavor” (1934, 218):

... so mag das altnorwegische *arjostez* des Tunesteins auch einen rassistischen Beigeschmack haben.

Jedenfalls beweist der Tunestein, daß das Wort, auf dem unser „Arier“ beruht, nicht nur indo-iranisch und keltisch, sondern auch altgermanisch ist.

This was “political correctness” in the Third Reich. Interestingly, this passage does not occur in Krause’s handbook of 1937, nor is it found in the later, standard edition (Krause 1966), or in his book on the language of

⁹ This is also emphasized by Bjorvand (2008), who provides further criticism of the proposed word formation of **āsija-*, to be discussed in another connection below.

¹⁰ The reviewers assert that there are in fact instances of double vowels (Rö, Wremen) written single. However, in my view, the evidence is not entirely clear, and the same goes for other such cases. In fact, despite common claims to the contrary (e.g. Grønvik 1981, 182), there are also few if any certain instances of double consonants in word-final and word-initial position written single either (cf. Antonsen 1975, 242; 1986, 330).

the runic inscriptions (Krause 1971), but the reading and the interpretation are retained. Krause's reading has been criticized by several scholars, in particular Norwegian runologists, who have examined the inscription carefully, with the means of modern technology (cf. Grønvik 1981, 114–16; Knirk 2006, 334). The results of this examination show that the reading **arjostez** is unfounded, and that it can be considered certain that the text shows the sequence **si** and not a retrograde **r** (which would be the only one in the inscription). Independently of the epigraphic facts, the etymology and linguistic distribution of the word *Aryan* have been revised in subsequent scholarship. It is now mostly assumed that the root of *Aryan* is confined to Indo-Iranian (Mayrhofer 1992–2001, 1: 111f. s.v. *arí-*, 174f. s.v. *árya-*). Accordingly, the Celtic word *aire* 'free man, prince', mentioned by Krause in the passage quoted above, must have a different etymology (to Old Irish *air* 'before', cf. OHG *furi*, *fora* 'before').¹¹ For a well-balanced discussion, see Grønvik (1981, 116).

It is clear that neither the epigraphic nor the linguistic evidence supports Krause's proposal. In view of the fact that it recurs in recent scholarly literature (e.g. Heidermanns 1993, 103; Boutkan 1995, 101; Nielsen 1998, 546f.; Bammesberger 1999; Imer 2011, 205), it must be emphasized that it is most implausible and should be put *ad acta* as an unfortunate example of scholarship misguided by opportunistic political considerations. Interestingly, Krause himself seems to have become somewhat doubtful about the existence of the proposed word. In the second edition of his handbook (Krause 1966, 166), he suggested an alternative emendation **(a)r<bi>jostez** 'die zum Erbe Nächstberechtigten'. This was probably meant to be formed to an alleged adj. **arbija-* 'entitled to inheritance', possibly attested in OSw. *iamnærfti* 'equally entitled to inheritance [lika arvsberättigad]' (cf. Grønvik 1981, 116). Finally, Antonsen's (1975, no. 27) proposal **arbijarjostez** 'the most legitimate-to-inherit', a form claimed to be a compound made to **arbija*ⁿ 'inheritance' and **arjöstēz*, not only retains Krause's unsubstantiated reading, but would also be unparalleled in Germanic, both in regard to its word formation and its semantics.

Indo-European legal language in the Tune inscription

None of the proposals to emend the form **sijostez** discussed above can be considered plausible. Fortunately, however, there is a further possibility,

¹¹ Note, however, that the connection between Indo-Iranian and Celtic is upheld by Delamarre (2003, s.v. *arios*).

which is simply to take **sijostez** at face value. As such it can be analyzed as the superlative to an adjective **sī-ja-* based on the stem of the reflexive **se-* ‘self, own’ (seen in the reflexive pronoun ON *sik* ‘self’ etc.). This analysis was, in fact, suggested more than a century ago by Löffler (1892, 2–4), translating the form **sijostez** as ‘the ones most closely related [de mäst eller närmast besläktade]’. Löffler suggested that this adjective was also contained in the Old Frisian masculine noun *sia* (*sīa*), which means ‘descendant [Sprosse, Nachkomme, Grad der Verwandtschaft]’. Plausible as it is, this idea has been virtually ignored in the discussion of the Tune inscription ever since it was subjected to a critical evaluation, and dismissed, by Marstrand (1930, 310–15; see also Syrett 1994, 89). Recently, Bjorvand (2008) has reopened the discussion; while accepting the reading **sijostez** and the meaning ‘the ones most closely related’, he rejects the association with the reflexive **se-*, connecting it instead to PGmc. **sī-* (PIE **siH-* ‘bind’, **sī-man-* (masc.) found in OE *sīma*, OS *sīmo*, OFris. *sīm*, ON *síma* ‘rope, cord’ (and also Modern Icelandic *sími* ‘telephone’); in fact, for Bjorvand the Old Frisian form *sīa* also belongs here. Although possible, this proposal has the drawback that such an adjective is hard to motivate on independent grounds since forms derived from PIE **siH-* appear not to figure in kinship terms in Indo-European.

Despite the objections by Marstrand and Bjorvand, I believe that Löffler’s analysis can be maintained. The form **sijostez** can plausibly be taken as the superlative of an archaic form of the reflexive possessive adjective **sī-ja-z*, reflecting pre-Gmc. **sei-yo-s*.¹² This is a **-yo-*-derivation which would have co-existed alongside **sei-no-s*, formed with the suffix **-no-*, giving the Proto-Germanic form **sī-na-z*. In other Indo-European languages possessive adjectives are formed in a similar way as in Germanic, but generally with a different suffix, notably **-yo-* in Old Church Slavic (*svoju* ‘self’s, own’ < **swo-yo-*). The possessive adjectives are derived from the locative case formed to the Proto-Indo-European pronominal stems **me-*, **te-*, **se-* with the addition of various suffixes, which in Germanic is regularly **-no-*. This gives the pre-Germanic forms **mei-no-s*, **tei-no-s*, **sei-no-s*, yielding PGmc. **mī-na-z*, **pī-na-z*, **sī-na-z* (ON *mínn*, *þínn*, *sínn*, etc.). The semantic development of a locative construction (‘with me’) to a possessive (‘belongs to me’) is straightforward and has typological parallels in various languages, including Celtic, Slavic, and Hungarian. A superlative to a pronominal adjective, comparable to the form **sīyōstēz* posited on the basis of the Tune inscription, is found, for example, in Latin *ipsissumus*

¹² Löffler (1892, 3) suggested that the form was derived from the reflexive stem PIE **se-* (as **se-yo-*), or alternatively a locative stem PIE **sei-* (as **sei-o-*; Löffler 1892, 226).

‘his very own self’ (Plautus, *Trin.* 4, 2, 14), presumably a sociolinguistically conditioned nonce formation to *ipse* ‘self, himself’.

A further issue is addressed by Bjorvand (2008), according to whom the superlative suffix **-ōsta-* was confined to *a*-stems in the earliest Germanic, whereas the suffix **-ista-* occurred elsewhere, including *ja/ija*-stems. Thus, for example, with an *a*-stem adjective like PGmc. *leuba-* ‘dear’ we find ON *ljúfr*, *ljúfari*, *ljúfastr* and comparable forms in other old Germanic languages (with **-ōsta-*), but with a *ija*-stem like PGmc. **rīkija-* ‘mighty’ we get early ON *rīkstr* (with **-ista-*). Later on, however, the suffix **-ōsta-* was gradually expanded beyond its original domain, as in ON *rīkastr* for *rīkstr*. In this light, Bjorvand claims, a form such as *sijōstēz* (or *sijōstēz*, with a short *-i-*, with Läßler’s alternative reading, or *āsijōstēz*, for that matter) could in any case not be considered as an inherited, non-analogical form in Germanic. The expected form in the Tune inscription would in fact be **siistēz* (or **siistēz* or **āsistēz*). As mentioned above, Bjorvand assumes the reading *sijōstēz* ‘next of kin’, to the root **sī-* ‘cord’, which is hard to substantiate. In order to account for the *-j-* in the form, Bjorvand proposes that it is a glide in a hiatus after the long *-ī-*, rather than part of the stem.

There are two main objections to this proposal. First, although the handbooks of Germanic historical linguistics present the case as it is referred by Bjorvand, the evidence for the distribution of the two superlative suffixes in the earliest Germanic is tenuous, and it is not certain that the distribution of the variants was as rigid as claimed. Thus, it is conceivable that even already at an early stage PGmc. **-ōsta-* could, at least occasionally, occur with *ja/ija*-stems, as it did later. Second, the assumption of a glide is not compelling for the language of the early runic inscriptions, and certainly not for Old Norse, as Bjorvand himself remarks. To be sure, he suggests, as a parallel, that there is a glide in the form *þrijōz* ‘three (fem.)’ in the Tune inscription itself as against ON *þrjár* (from **þrīar*) which does not contain a glide. The *-j-*, however, can just as well be taken to be part of the stem, with *þrijōz* reflecting PGmc. **þre-jō-z*, and it is possible that ON *þrjár* (**þrīar*) and OS *thriā* do so as well (cf. also OHG *drīu*, OE *ðrēo* which point to **þre-jō-*; see e.g. Antonsen 1975, 45). To conclude, despite Bjorvand’s objections, the form **sijōstēz* can probably be taken to be a superlative to a PGmc. *ja*-stem **sī-ja-*.

An additional problem with the above analysis for the form **sijostez** in the Tune inscription is the apparent lack of supporting evidence in Germanic for a possessive adjective with the suffix **-yo-* rather than **-no-*. As was pointed out by Läßler (1892, 3), however, Old Frisian *sīa* ‘descendant’ may reflect an *n*-stem noun **sījan-* (masc.) ‘relative’, to an adjective **sīja-* ‘own, related’ which would be a **-yo-*-derivative from the reflexive stem **se-*.

It must be kept in mind, however, that Old Frisian is not an archaic dialect within Germanic; in fact, it has been called “an Eldorado for etymologists” (Marstrander 1930, 312). Discussing Löffler’s account, Bugge (1903, in *NIæR*, 2: 515) rejected it on the grounds that OFris. *siā*, which can mean ‘companion [Gefährte, Genosse]’ (as well as ‘descendant’), was to be derived from **sīpa-* (PGmc. **sinpa-*) ‘trip, way’. In his study of the Tune inscription, however, Marstrander (1930, 310–12) examined the evidence in great detail, concluding against Bugge that OFris. *siā* ‘descendant’ was unlikely to have lost an intervocalic **-b-*, and was thus not derived from **sīpa-*. The reason is that the form only occurs in Old East Frisian documents, where loss of intervocalic **-b-* is not attested. Therefore, the Old East Frisian form *siā* ‘descendant’ must be etymologically different from **sinpa*. As to Löffler’s suggestion, Marstrander (p. 312) admitted that “Seldom in the history of runology has a combination been proposed that objectively and formally holds up better [der sjelden i runologiens historie har vært fremlagt en kombinasjon som saklig og formelt står bedre]”. Nevertheless, Marstrander was unable to accept its validity, mainly because it was based on evidence outside North Germanic, concluding that **sijostez** was a *vox nihili*. Instead, Marstrander (p. 320) suggested the reading **(a)sijostez**, discussed and dismissed above.

Shortly after Löffler published his analysis, Kauffmann in a review (1894) proposed a different etymology for the forms **sijostez** on the Tune stone and OFris. *siā*, comparing them to Lat. *sequi* ‘follow’ and *socius* ‘companion’ (reflecting **sehwo-*, to the PIE root **sek^we/o-*). Kauffman’s hypothesis recurs in Holthausen (1925, 92) and Lloyd and Springer (1988, 518 s.v. OHG *beinsegga* [fem. *n*-stem] ‘pedi-sequa, Dienerin’), although its shortcomings were demonstrated already by Löffler (1896a, 98; 1896b, 215). According to established sound laws, a Proto-Indo-European form **sek^w-i-o-* would have given PGmc. **sigja-* or **siwja-*, not **sija-*.¹³ This etymology was justly dismissed by Marstrander (1930, 313–15), whose own proposal, however, was completely unmotivated, connecting *siā* ‘descendant’ with ON *sýja* (< **siujōn-*) ‘round of planking of a ship’s side [omfaret i bordkledningen, bordgangen]’. Bjorvand’s recent proposal (2008) has been discussed above.

It should go without saying that none of the alternative accounts is any more convincing than that of Löffler. In conclusion, OFris. *siā* can be derived from the stem of the reflexive, and so can the form **sijostez** on the

¹³ A reviewer states that it is unnecessary to posit *-i-* in the preform of OFris. *siā* ‘descendant’, given the derivation of OFris. *siā* ‘see’ from PIE **sek^we/o-*. The reviewer further suggests a connection between the Old Frisian words—“so the descendant is the one the ancestor can see”. Given the lack of parallels, I find this suggestion implausible.

Tune stone. The *-yo-form of the possessive adjective could have existed in the earliest Germanic alongside the *-no-formation which subsequently replaced it.

The original meaning of the reflexive stem *se- was ‘own, belonging to the family’. PIE *swo- is the reflexive and possessive pronoun equally applicable to all persons, as in Russian *svoj* ‘(my, your, his, our, their) own’. This original meaning is also seen in Goth. *swes* ‘own (adj.); property (noun)’, ON *sváss* ‘dear, beloved’, OFris. *swēs* ‘related’, OE *swǣs*, OS, OHG *swās* ‘dear, one’s own’, OHG *gi-swāso* ‘familiar, secret’, Middle Dutch *swāselinc* ‘related by marriage’ (cf. Benveniste 1973 [1969], 269; Szemerényi 1996, 220 f.; Lehmann 1986, 182 s.v. *swes*). Therefore, the phrase **sijaz arbijan-*, reconstructed for Germanic on the basis of the Tune inscription, would mean ‘his own heir, family heir’.

In fact, a close parallel to this expression occurs in archaic Latin. This is the phrase *suus heres* ‘his own heir, family heir, self-successor’, which is found in the *Law of the Twelve Tables* (*Leg. xii tab.*, V 4; Warmington 1979, 448 f.):

Si intestato moritur, cui suus heres nec escit, adgnatus proximus familiam habeto.

‘If person dies intestate, and has no self-successor, nearest agnate male kinsman shall have possession of deceased’s household.’

In early Roman times a *suus heres* was an heir who had been in paternal power of the testator until his death, and was regarded as having held the father’s property in common with him by dormant right which became active automatically upon the father’s death (Warmington 1979, 444, 448). Later the term was used of first successors to an intestate, as stated by Justinian: *Intestatorum ... hereditates ex Lege XII Tabularum primum ad suos heredes pertinent* ‘Inheritances of persons who die intestate fall first, by the Law of the Twelve Tables, to self-successors’ (*Inst.*, III,1,1). Moreover, Justinian makes clear that both men and women could be *sui heredes*: *Utraque persona in hominum procreatione similiter naturae officio fungitur, et Lege antiqua XII Tabularum omnes similiter ad successiones ab intestato vocabantur* ‘Both sexes perform equally the function of nature in perpetuating mankind, and by the ancient Law of the Twelve Tables all were called equally to succession by an ancestor intestate on decease’ (*Inst.*, II,13,5). If, however, there were no “self-successors” or blood-relations (*consanguinei*), the inheritance would belong to the nearest agnate relations (*adgnati proximi*), that is, male kinsmen by blood who traced their descent through males of the same family (Warmington 1979, 448).

The Latin legal term *suus heres* was discussed by Benveniste (1973 [1969], 272) in the context of Indo-European. He notes that the occurrence of the reflexive in this expression is clearly an archaism which is only comprehensible in view of the original meaning of the reflexive stem, PIE **se-*, **s(e)wo-* ‘own, belonging to the family’. On the other hand, if *suus* had only possessive sense in this phrase, it would not be necessary. “A *heres* who is a *suus*,” Benveniste concludes, “this is what the provision intends: there is no transmission of property outside the *sui*, that is to say the closed group of immediate descendants; it remains within the group of collaterals”.

Together, the archaic Latin phrase *suus heres* and **sījaz arbījan-*, conjectured for Germanic on the basis of the Tune inscription, reflect a legal term which has its origins at a linguistic stage ancestral to the Germanic and Italic branches, that is to say Western Indo-European, if not Proto-Indo-European itself. To be sure, in Germanic the lexical item **arbījan-* occurs instead of **ġ^heh₁-ro-*, the word for ‘heir’ manifested in Lat. *heres* (a compound of **ġ^heh₁-ro-* plus the substantivizing suffix **-e/od-*) and Greek *k^hērōstēs* ‘collateral heir’ (see also *k^hēros* ‘deprived of parent’, *k^hēra* ‘widow’). But the specific combination with the reflexive in the phrase ‘his own heir, family heir, self-successor’ is a *détail singulier* in the sense of Meillet, conclusively suggesting the survival of a formulaic construction which belongs to Indo-European legal language into the early Germanic period.¹⁴

In summary, as argued by Löffler (1892), the form **sijostez** in the Tune inscription can be taken at face value and analyzed as the superlative to a possessive adjective derived from the reflexive stem **se-*. The phrase **sijostez arbijano** ‘the closest family heirs’ is an ancient legal term which has a parallel in Archaic Latin, and arguably has its roots in Indo-European.

Who shared the inheritance? Who erected the stone?

We have seen above that there is a good case for emending **dalidun** to **da(i)lidun** ‘shared’, whose object must be **arbija** ‘inheritance’, and that therefore the form **staina** ‘stone (acc. sg.)’ is likely to be the object of a different verb (‘erect’ or something similar). On the basis of the results established so far, the following questions must now be re-iterated concerning the role of the three daughters (**prijoz dohtriz**) and the closest family heirs (**sijostez arbijano**). First, who shared the inheritance—the closest family

¹⁴ Watkins (1995) contains detailed case studies addressing the problem of lexical substitution and the reconstruction of formulaic expressions with specific semantics for Proto-Indo-European.

heirs excluding the daughters, or the daughters who are the closest family heirs? Second, who erected the stone — the daughters or somebody else?

The answer to the first question depends on the syntactic function of the phrase **sijostez arbijano** ‘the closest family heirs’. There are two possibilities. On the one hand, the masculine form of the adjective may indicate male heirs, the subject of **da(i)lidun arbija**: ‘The closest male family heirs shared the inheritance.’ If this is the case, this phrase is not modifying **prijoz dohriz**, which would be the subject of a different clause (presumably involving the raising of the stone). On the other hand, **sijostez arbijano** may be in apposition to **prijoz dohriz**, the subject of **da(i)lidun arbija**: ‘Three daughters shared the inheritance, the closest family heirs.’ This involves gender agreement in the appositional phrase between the nominalized adjective **sijostez** and the masculine partitive genitive **arbijano** (as if Icelandic *dætur, nánastir erfingja* ‘daughters, the closest heirs’; cf. Jónsson 1931, 149). If this latter possibility is the correct one, B1 must contain a separate clause from B2–3.

The answer to the question who erected the stone, in turn, depends on the answer to the first one. If it can be established that the family heirs are different from the daughters, then the former must have shared the inheritance, while the latter did something else, i.e. erected the stone. If, on the other hand, the three daughters shared the inheritance as the closest heirs, then either the daughters also erected the stone, or somebody else did, whose identity would remain to be established. The three possible interpretations can be summarized as follows:

1. Three daughters erected the stone for Wōdurīdaz, but his closest family heirs shared the inheritance.
2. Three daughters erected the stone for Wōdurīdaz and shared the inheritance, as the closest family heirs.
3. NN erected the stone for Wōdurīdaz. Three daughters shared the inheritance, as the closest family heirs.

In order to establish which of the three possible interpretations is to be preferred, we must try to locate the position of the missing verb (‘erect’ *vel sim.*), of which **staina** is the object. Here there are only two possibilities: the verb in question must have been either at the beginning or the end of line B1. In the following section I seek to determine which of these two slots is more plausible as a position for the missing verb.

What is missing in line B1?

A relatively short word of four (possibly five) staves ending in **z** appears to be missing at the beginning of line B1. It seems impossible to narrow down the runic possibilities for the staff immediately preceding the first visible rune. Various proposals have been made concerning this missing word, some of which will be evaluated in the following.

Moreover, there is possibly a lacuna after **staina** (which is followed by a word separator) at the end of B1, despite claims to the contrary by many leading scholars, including Bugge (1903, 520), Marstrander (1930, 298–300, 309 n. 1), Krause (1966), Antonsen (1975), and Grønvik (1981, 117, 141, 168). As stated at the outset, at the end of line A2 there is a trace of a rune, in all probability **r**, indicating that the word *rūnōz* ‘runes’ is missing at the top of the stone, which is broken off. Therefore, a word could in fact be missing at the top on the other side as well, at the end of line B1.

As argued above, it seems a reasonable conjecture that **staina** is the object of a verb such as PGmc. **satjana* ‘erect’ and not of **da(i)lidun**. This verb could be either at beginning or the end of B1, although both assumptions involve some epigraphic difficulties.

The counterarguments to the assumption that the text continues after **staina** (and the word separator following it) mainly involve the presence of an even ledge at the top of side B. The main points can be summarized as follows. First, if the ledge had been made after the runes were carved, it would be a remarkable coincidence that **staina** ends immediately before the ledge, and **prijoz** begins immediately after it. Rather, it would seem as though it was already there before the runes were carved; so the runemaster would not have gone any further than the ledge with line B1, and this is also where he began with B2. Second, if there was a verb after **staina**, why did the runemaster not begin the next line (B2) at the top; in other words, why does **prijoz** start immediately after the ledge?

These questions, however, do not carry as much weight as they might seem given ample epigraphic evidence in favor of the view that the Tune stone is a “runological torso” (cf. the discussion in Johansen 1984, 41–45; Sanness Johnsen 1969, 41f.; Moltke 1984, 24f., 31f.; see also Knirk’s 1991 report on the findings of the stonemason Halfdansen, at p. 106). As to the first point, there is at least a theoretical possibility that there was a crack after **staina**, where the top later broke off.¹⁵ Secondly, there are graphic indications that

¹⁵ However, it must be stressed that everything points to the presence of the ledge before the inscription was written. As reported by Knirk (1991, 106), in Halfdansen’s opinion, the ledge

lines B2–3 belong more closely together than B1 (Grønvik 1981, 137; 1998, 35f.). Among other things, the use of word separators in B1 (before and after **staina**), but not in B2–3, may suggest that this line belongs more closely with side A. Thus, lines B2–3 stand apart epigraphically and are not directly linked to B1, and arguably this is also the case with respect to the content. One possible account of this observation is that the text in B1 was written first, but the carver then chose to start with the text in B2 further below where there was more space. Another possibility, which in my view is more feasible, is that lines B2–3 were written before B1 (cf. Jónsson 1931, 143; Sanness Johnsen 1969, 41–44; Moltke 1984, 31f.). The direction of the writing on side A, beginning downwards from the top, supports this assumption. In this light, it is plausible to assume that lines B2–3 were written first, when there was enough space on the side, and line B1 written last.

Be that as it may, the assumption that the text in B1 continued after **staina** is not only possible but even perhaps plausible.

The lacuna at the beginning of B1

Many suggestions have been made as to the missing word at the beginning of B1. As already stated, this must be a relatively short word of four (possibly five) staves ending in **z**. In fact, items from a variety of word classes have been proposed: verb, preposition, pronoun, numeral, and noun. I now turn to a discussion of some of the proposals.

⟨**me**⟩**z** ‘*me (dat.)*’

Marstrander (1930, 304–07) believed that the inscription was complete, with the exception of a missing word at the beginning of B1. Marstrander’s reading ⟨**me**⟩**z** ‘*me (dat.)*’ was accepted by Krause (1966, 1971) and Antonsen (1975). This word consists of four staves before **z**, and could therefore fit into the space. The text presented in Krause (1966, no. 72) is as follows (with word division introduced):

⟨**me**⟩**z woduride staina þrijoz dohtriz dalidun arbija (a)rjostez arbijano**

‘Mir (?) dem Wodurid bereiteten den Stein drei Töchter, das Erbmal (aber) die vornehmsten ... der Erben.’

was due to moisture and ice-splintering/breaking, and thus it cannot have been leveled after the runes were carved.

The validity of this reading depends on two conditions. The first is that **dalidun** means ‘made (nice)’, a possibility which I have already argued against at length. The second condition is that the subject of the alleged verb phrase ‘made (prepared) the stone’ is ‘three daughters’. This, however, is implausible for epigraphic reasons, as line B2 is unlikely to be a direct continuation of B1, hence it would be unclear why the first word in B2 (**prijoz** ‘three’) did not begin at the top of side B, on the lost top-piece. A further problem with this proposal is that it would seem rather banal to say that the heirs prepared the funeral feast without mentioning their identity (cf. Grønvik 1981, 114). In addition, as pointed out by Finnur Jónsson (1931, 143f.), this reading would involve the only example in the corpus of runic inscriptions where the deceased person “speaks”. Although this is usual in Greek and Roman tombstones, it does not seem to have been customary in the early Germanic world. In short, then, the commonly accepted reading of Marstrander and Krause cannot be upheld.¹⁶

⟨**afte**⟩**z** ‘after’

In his edition of the Tune stone, Bugge (1891, in *NlæR*, 1: 28f.) proposed that the missing word at the beginning of B1 was a preposition **aftez** ‘after, in memory of’. He also assumed that there was a verb (such as ‘mark [mærke]’) whose subject was ‘three daughters’ missing at the end of B1.

⟨**afte**⟩**z woduride staina** ⟨‘mærkede’⟩ **prijoz dohtriz**

‘after Wōdurīdaz three daughters ‘marked’ the stone’

Following this reading, the persons who ‘marked’ the stone were the three daughters, but the identity of the ones sharing the inheritance was not stated. As this would be quite trivial, Bugge later revised his own view (1903, in *NlæR*, 2: 520–23), thinking it unlikely that ‘three daughters’ were the subject of the clause in B1. Rather, he suggested, the subject was Wīwaz, the person speaking on side A, and the missing verb (**satiðō* ‘(I) erected’) was placed at the end of A2.¹⁷ This assumption is quite implausible on epigraphic grounds, and it is particularly unlikely that there would have been space for any further words at the top of side A.

¹⁶ Even more far-fetched is Lehmann’s (1956, 78) reading ⟨**pe**⟩**z** ‘you (dat.)’; his account involves further problems, as shown by Marold (2012, 77f.).

¹⁷ Bugge (1903) assumed that the form of the missing verb was **sato** (“**sattō*”), but in the wake of the discovery of the Rō stone in 1919 scholars realized that the form must have been **satiðō* (see Grønvik 1981, 84).

A variation on this theme was proposed by von Grienberger (1906, 99 f.), who assumed that side B contains two conjunct clauses. ‘Three daughters’ is the subject of both clauses, and (according to his understanding) ‘the last surviving heirs’ is in apposition to it.

⟨afte⟩z woduride staina ⟨satun/satidun⟩ prijoz dohtriz da⟨i⟩lidun arbija sijostez arbijano

‘(After) Wōdurīdaz three daughters the stone (erected), (they) shared the inheritance, the last surviving heirs [post Voduridum lapidem posuerunt tres filiae, partitae sunt haereditatem, postremi haeredum].’

In addition to ignoring the epigraphic distinction between lines B1 and B2–3, this proposal suffers from the same problem as the ones of Bugge, assuming a form **aftez** on side B *vis-à-vis* **after** on side A.

⟨ee⟩z ‘honored’, and other proposals by Grønvik

Grønvik (1981), following scholars such as Marstrander (1930), Krause (1966, 1971) and Antonsen (1975), became a staunch defender of the position which holds that nothing is missing at the end of B1. Claiming that **dalidun arbija** must mean ‘made the funeral feast nice’, he concluded that a finite verb, of which **staina** could be the object, had been located at the beginning of B1. The first visible rune, usually taken to be **z**, was read by Grønvik as **h** (1981, 169–73):

⟨fal⟩h woduride staina

‘(I) dedicated the stone to Wōdurīdaz’

In view of general criticism of his analysis, Grønvik (1984, 51–54) was forced to accept the validity of the “top-piece hypothesis”. He also modified the reading of the missing word to ⟨bifal⟩hk (containing an enclitic pronoun *-k* ‘I’, in a bind-rune with **h**) ‘I dedicated’, this time assuming that the first two runes (**bi**) were written at the top of side A, and that the word continued on side B. Later, however, he gave up the reading **h/hk** for the traditional **z**, proposing a completely different word (1994, 48 f.):

⟨ee⟩z woduride staina

‘(I Wiwaz) honored Wōdurīdaz with a stone [(Jeg Wiwar) forærte Wodurid stein, egentlig æret Wodurid med stein, ved å gi ham stein].’

On this assumption the missing verb is <ee>z, a strong past tense form of ON *eira e-m* ‘spare someone [skåne en]’, but here in a derived meaning ‘show respect, honor [vise respekt, ære; honorare, honorificare]’. It is true that if the missing word is a verb, it must be a strong one, in view of the ending -z. In Old Norse, however, *eira* is a weak verb (past tense *eirða*), but Grønvik suggests that earlier it may have been strong, according to class 7, with past tense **ēr* (PGmc. **aizanaⁿ—eaiz*). Moreover, Grønvik assumes that an older expression **eēz Wōdurīdē staina*, with an instrumental dative of the object (‘æret Wodurid med stein’), was “transformed” to one with the object in accusative.

This proposal is open to criticism on several accounts. First, the weak verb ON *eira* is a denominal derivation from *eir* (fem.) ‘protection, grace, peace’ (< **aizō-*). There is no evidence for strong verb **aizanaⁿ—eaiz* in Germanic. Second, the meaning of *eira* ‘spare someone’ does not fit in the context; the derived meaning ‘show respect’ postulated by Grønvik is unattested. Third, the assumption that the dative was replaced by an accusative in this construction is *ad hoc*; the usual tendency in Old Norse has been in the opposite direction, with accusative being replaced by dative. Finally, this reading runs into the same epigraphic problems as other proposals to take line B1 as a continuation of side A; it would seem implausible that the carver continued with the text ending at the top of side A from the bottom of side B. In summary, none of the many valiant attempts made by Grønvik to fill the lacuna at the beginning of B1 can be deemed successful.

<we>z ‘we’

Western (1930, 291f.) proposed that the missing word at the beginning of B1 was **wez** ‘we’, modified by the appositional phrase **prijoz dohtriz**. He suggested that the latter noun phrase was separated from the pronoun due to the metrical form of the inscription.

[B1:] <we>z woduride staina <sati[B2:]-dum> **prijoz dohtriz da(i)lidun**
 [B3:] **arbija sijostez arbijano**

‘We three daughters erected the stone (in commemoration of) Wōdurīdaz; the closest male heirs controlled the inheritance [wir drei Töchter setzten den Stein (zum Andenken an) Wodurid; die nächsten männlichen Erben verfügten über (d. h. nahmen infolge der damaligen Rechtsauffassung) das Erbe].’

This reading is based on the conviction that the daughters were not entitled to inheritance: “Wie sie [*scil.* die Erben] dagegen das Erbe untereinander verteilten, davon hören wir nichts; die Hauptsache ist nämlich, daß die

Töchter nichts bekamen”. I will return to this issue below. This reading suffers from a number of problems, including the fact that the word **wez** has only three staves before **z** (oddly enough, Western himself admits the presence of four staves: “so wird man vor der Rune Ƴ vier kleine Spuren von Runen sehen”).

⟨**pri**⟩**z** ‘three (masc.)’

Sanness Johnsen (1969, 42–44) proposed that the missing word at the beginning of B1 was the numeral **priz** ‘three’.

si⟨**b**⟩**jostez arbijano** ⟨**pri**⟩**z woduride staina** ⟨**satidun**⟩

‘The three closest male relatives of the heirs erected the stone for Wōdurīdaz [arvingenes tre nærmeste mannlige slektninger (satte stenen for Woduride)].’

While the word **priz** ‘three’ might fit in the lacuna, it requires the assumption of a part of line B3 (**si**⟨**b**⟩**jostez arbijano**) preceding B1. Dividing line B3 in two involves an *ad hoc* assumption, which does not seem justifiable on epigraphic grounds. In addition, the occurrence of the genitive before the numeral would be unusual in a Germanic language. The question is, moreover, who these three anonymous individuals might be. Sanness Johnsen wonders if they might be the husbands of the three daughters. However, it must be considered unlikely that the sons-in-law, and not their wives, would be referred to as ‘the closest relatives’.¹⁸

Noun ending in -z

Finally, it has been proposed that line B1 begins with noun ending in **-z**, which is of course a common ending in masculine nouns. The form **staina** would be the object of a verb **satjana* ‘erect’ or something similar at the end of B1. The line then reads as follows.

...z woduride staina ⟨**satide**⟩

‘... (erected) the stone for Wōdurīdaz’

One possibility is that the missing word was a personal name in **-z**. In fact,

¹⁸ Consider also in this context Grønvik’s (1981, 120 f.) critical discussion of the original meaning of PGmc. **siþja-* as applying only to blood relations, referred to in my footnote 8 above. I remain agnostic toward his conclusion, however.

this was suggested already by Noreen (1903, 345; 1923, 309; cf. also Jóhannes-son 1920 [1923], no. 72; Jónsson 1931, 143). This would have a parallel elsewhere, most clearly in the inscription from Rö, Bohuslän: **ek hrazaz satido** < **s** < **tain** < **a** > **ana**... 'I Hrazaz set the stone on ...' (Krause 1966, no. 73; Antonsen 1975, no. 26). The question then becomes which name would fit in the space at the beginning of B1. For example, the name **paliz**, which is attested in an inscription from Bratsberg, Sør-Trøndelag (Krause 1966, no. 93; cf. Western 1930, 289; Moltke 1984, 31), would fit in the lacuna. There is, however, no other motivation for assuming this name and not some other. In the above-mentioned response to Marstrander's (1930) article on the Tune inscription, Jónsson (1931, 143) suggested that the name in question might be *Wiwaz*, presumably the same person as the one speaking on side A. In runic spelling this word consists of four staves before the final **z**, and could indeed fit in.¹⁹ As argued above, it seems a reasonable conjecture that **staina** is the object of a missing verb such as **satjana*ⁿ 'erect'. Given this analysis of the word at the beginning of B1, the only place where the verb would fit is at the end of the line, even though such an assumption may be paleographically tenuous. The placement of the verb at the end of a main clause was a possibility in Old Germanic, including the language of the early runic inscriptions (Eythórsson 2001, 17 f., 22 f., 45 f.; 2012, 38).

In conclusion, the following reading of side B is the most plausible one:

B1: <...>**z woduride : staina** · <**satide**>

'NN (*Wiwaz*?) erected the stone for *Wöduridaz*.'

B2-3: **prijoz dohtriz dalidun arbija sijostez arbijano**

'Three daughters shared the inheritance, the closest family heirs.'

It may seem odd that on one side of the stone *Wiwaz* himself speaks in the first person, but on the other side he is spoken of in the third person (there is hardly enough space in B1 for a pronoun **ek** 'I' preceding the name, even assuming some other, shorter name). This, however, may be due to the different contexts that the two sides of the inscription would involve. Thus side A contains a declaration, while side B polarizes two parties: the heirs as against the person who erected the stone. According to the reading defended here, therefore, on side A *Wiwaz* himself declares that he has

¹⁹ However, as James Knirk has pointed out for me, the space between the remains of the second and third staves would most likely imply a branch on the second stave, i.e. that it was not **i**.

written the runes in memory of the deceased Wōdurīdaz, but on side B it is stated that Wīwaz erected the stone for Wōdurīdaz. Concerning the identity and status of Wīwaz, we can only venture an educated guess. He may have been Wōdurīdaz's successor as a chieftain, either his son-in-law, or even his grandson, given the alliteration of the names, which may indicate blood relations. He could also have been an unrelated runemaster whose skill was required for this document.

It is unlikely, however, that Wīwaz was the son of Wōdurīdaz, as he — and not his three sisters — might then have been expected to be his heir. Although all of this is of course speculative, it nevertheless seems better motivated than Grønvik's (1994, 50) view that the person who erected the stone is the main heir ("hovedarvingen"), while the three daughters of the deceased are complimented for a nice funeral feast ("et hyggelig gravøl"). To be sure, there is no reason to doubt that Wōdurīdaz received a decent funeral, a fact which the splendid granite stone amply corroborates. It seems undeniable, however, that the three daughters mentioned in the inscription were not merely in the role of hostesses at the funeral feast, but got their share of the inheritance.

Daughters as heirs

Part of the reason why this almost self-evident interpretation, which used to be the prevailing one, was discredited may originally have had to do with reluctance by some scholars to accept the possibility that daughters could be legitimate heirs in ancient times, as the following passages suggest. In a letter to Löffler (1896), the legal historian Ebbe Hertzberg expressed support for the former's opinion that **prijoz dohtriz** 'three daughters' could not be the subject of **da(i)lidun arbija** 'shared the inheritance' because he considered out of the question that women in Scandinavia had the right to inherit already around A.D. 500. Hence **sijostez arbijano** would have to refer to the 'the closest male heirs' (Hertzberg, cited in Löffler 1896, 100):

Naar jeg imidlertid dog er enig med Dig i, at arvingerne maa formodes kun at have været mandlige, saa er min grund, at jeg, som jeg allerede før har skrevet til Dig, ikke kan tænke mig, at kvinder heroppe i Norden allerede ere blevne stedte til arv saa tidligt som ca. 500.

This view recurs in the paper by Western, who considered it to be a "fact" that daughters were excluded from inheritance in the relevant time period. Therefore, he could not believe that **prijoz dohtriz** was the subject of **da(i)lidun arbija** (1930, 289):

Aber eine Übersetzung: „drei Töchter teilten das Erbe“ widerstreitet dem, was wir wohl als eine Tatsache betrachten dürfen, daß zu jener Zeit die Töchter überhaupt nicht erbberechtigt waren. ... Das Subjekt für *dalidun* muß daher *sijoster arbijano* sein, das also, wie Löffler meint, ‘die nächsten *männlichen* Erben’ bedeuten muß.

Western concluded that the inscription is in effect a “desperate cry” for help by the three destitute girls, who were left only with the memory of their respected father, while some male collateral heirs, empowered by the law, seized the entire inheritance (s. 292):

... in meinen Ohren klingt sie [*scil.* die Inschrift] wie ein Notgeschrei der drei Mädchen, die mit nichts als der Erinnerung an einen angesehenen Vater hinterblieben sind, während einige männliche Seitenerben—kraft des Gesetzes— das ganze Erbe übernommen haben.

Contrary to this opinion, the investigation here reinforces the view that women were indeed entitled to inheritance in pre-Viking Scandinavia. The unbiased evaluation of the inscription leads to the conclusion that the three daughters mentioned in the text shared the inheritance left to them by Wōdurīdaz as his closest family heirs. It is possible that if there had been male heirs, only one of them would have been the designated **sījaz arbījan-*, who would not be sharing the inheritance with others. In any case, the fact that the heirs were three women must have been so unusual that it was thought important to make a specific mention of it in the inscription.

The writing on side B

In light of the results established here, it may seem worthwhile to have another look at the direction of the writing on side B. As I have argued, the text reports on two facts: first, who shared the inheritance, and secondly, who erected the stone in memory of Wōdurīdaz, the breadward commemorated by Wīwaz on side A. The first statement (in B2–3) is in a prominent place in the center, and hence more important than the second one (B1), which is at the edge. The positioning of the two statements on the stone is easily comprehensible on the assumption that B1 was written after B2–3. If, on the other hand, B1 would have been written first, it must seem strange that the carver started at the very edge of the stone, down below, given that he had enough space to fill. In view of this, it is plausible to assume that lines B2–3 were written first, when there was enough space on the side, and line B1 written last, as a kind of a “signature”. The direction of the writing on side A supports this assumption, as it starts at the top going downwards. Needless

to say, however, this idea remains speculative, and my account of the Tune inscription does not depend on it.

Finally, the question whether the different shapes of the runes point to one or two carvers must be briefly addressed. Despite the somewhat different ductus in side A and B, it cannot be excluded that the same carver was at work on both sides. For example, it could be that the text on side A was written while the stone was lying on the ground, but the text on side B was added later, after the stone was erected. This would explain the fact that the runes on side B seem to be less carefully carved than the ones on side A.

Conclusion

The interpretation of the inscription on the Tune stone advanced in this paper was made on the basis both of autopsy of the stone itself and of analyses by earlier scholars, in particular Bugge, Löffler, Noreen and Jónsson. On the other hand, it has been shown that some proposals in the more recent literature do not bear scrutiny. According to the reading defended here, there are lacunae at the end of line A2 and at the beginning and end of line B1. It is unlikely that there is anything missing at the beginning of B2. The order in which the lines are to be read is A1–2, B1–3 (although, as discussed above, B2–3–1 may also be a possibility).

- A1: **ek wiwaz after · woduri**
 A2: **de witadahalaiban : worahto : r⟨unoz⟩**
 B1: **⟨...⟩z woduride : staina · ⟨satide⟩**
 B2: **prijoz dohtriz da⟨i⟩lidun**
 B3: **arbija sijostez arbijano**

‘I Wīwaz wrought the r(unes) in memory of Wōdurīdaz, the breadward. (NN)z (Wīwaz?) (erected) the stone for Wōdurīdaz. Three daughters shared the inheritance, the closest family heirs.’

The text on side A involves a subject-initial declarative clause, in which the finite verb is flanked by an adjunct prepositional phrase and a direct object. It is possible that this word order is due to the rhythm of the statement and its metrical structure, for which there is ample evidence (cf. the analysis in Marold 2012, 75–78, with further references). Side B consists of two independent subject-initial clauses, occurring in B1 and B2–3, respectively. The B1 clause has a verb-final word order while the B2–3 clause is verb-medial (or verb-second order); both word order patterns have parallels in other inscriptions and elsewhere in Old Germanic (Eythórsson 2001, 14–18, 22f., 33–36; 2012, 32–40).

The above reading of side A is rather uncontroversial, in that it assumes a missing word **r(unoꝝ)** at the top, in accordance with the “top-piece hypothesis” (as in Bugge 1891 and most other scholars, with the notable exceptions of Marstrander 1930, Antonsen 1975 and Grønvik 1981). The reading of side B defended here is perhaps less straightforward, although it is based on arguments which have been proposed by earlier scholars. In this reading, side B consists of two independent subject-initial clauses, occurring in B1 and B2–3, respectively. Despite some apparent epigraphic problems, the clause in B1 must end in a verb meaning ‘erect (*vel sim.*)’, of which **staina** ‘stone’ is the accusative object (cf. Bugge 1891). The first word in B1 is likely to be a noun (a personal name) ending in *-z* (cf. Noreen 1903, Jónsson 1931, Moltke 1984). Moreover, the form **dalidun** in B2 should not be analyzed as *dālidun* ‘made (nice), prepared’, as has been standardly assumed since Seip (1929). Rather, it should be emended, giving **prijoz dohtriz da(ǰ)lidun arbija** ‘three daughters shared the inheritance’ (Bugge 1891). Finally, I presented arguments in favor of the reading **sijostez** (cf. Löffler 1892), dismissing the readings **si(b)jostez** (Bugge 1891), **(a)sijostez** (Marstrander 1930, Grønvik 1981 etc.) and **(a)rjostez** (Krause 1934, 1937, 1966, 1971, and others). Adopting a view which once was the prevailing one, I take the phrase **sijostez arbijano** to reflect an archaic legal term meaning ‘the closest of the family heirs’ (Löffler 1892). In addition to the epigraphic evidence, this analysis is supported by historical-comparative linguistic evidence and by a hitherto overlooked parallel in archaic Latin legal terminology (*suus heres* ‘family heir, self-successor’). I conclude that side B states that the three daughters of Wōdurīdaz shared the inheritance as the closest family heirs, while some other person (perhaps Wīwaz) erected the stone.

This conclusion reinforces the view that the inscription on the Tune stone bears witness to women’s rights to inheritance in the pre-Viking period, and that it is thus of even greater value for the earliest Scandinavian history than usually assumed (cf. Sawyer 2000, 111–16, on the role of women in Viking Age inscriptions). The reason why this interpretation, which used to be the prevailing one, was discredited may originally have had to do with the reluctance to accept the possibility that daughters could be legitimate heirs in ancient times.

Bibliography

Abbreviations for languages

Goth. = Gothic	OHG = Old High German
Lat. = Latin	ON = Old Norse
Norw. = Norwegian	OS = Old Saxon
ODan. = Old Danish	OSwed. = Old Swedish
ODutch = Old Dutch	PGmc. = Proto-Germanic
OE = Old English	pre-Gmc. = pre-Germanic
OFris. = Old Frisian	PIE = Proto-Indo-European

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The Germanic Diphthongs in the Continental Runic Inscriptions

Martin Findell

Abstract

Runic inscriptions on the Continent, excluding Frisia, are commonly treated as representing the precursors of Old High German and Old Saxon, which are attested in manuscripts of the eighth–eleventh centuries. If these literary languages are the result of regular sound change from a relatively homogeneous Northwest Germanic, then close study of the runic inscriptions might enable us to see some of those sound changes in progress. This paper examines the runic evidence for specific sound changes affecting the Germanic diphthongs */ai au eu/, and argues that the dialects of the inscriptions do not fit easily into a linear progression from Northwest Germanic to literary Old High German and Old Saxon.

Keywords: diphthongs, history of German language, Old High German, Old Saxon, phonology, runes, runic inscriptions

The following paper is extracted from a larger phonological study of dialects recorded in Continental runic inscriptions (Findell 2012), based on a corpus of ninety older futhark inscriptions with find-sites on the Continent, or for which there is some evidence of a Continental origin. The term “Continental” as employed here, in accordance with standard practice in runological studies, excludes the approximately twenty Frisian runic inscriptions, which belong to the Anglo-Frisian tradition (see Findell 2012, 8f.).

It has been common practice to treat the dialects of the inscriptions as precursors to Old High German and Old Saxon as we encounter them in manuscripts of the eighth to eleventh centuries. Phonologically, the “Continental runic” dialects are presumed to occupy a position in the development from a relatively uniform Northwest Germanic to early Old

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High German and/or Old Saxon (see, e.g., Klein 2001, 579 f.). It follows that we can look to the inscriptions for evidence for the sound changes which distinguish the later dialects from Northwest Germanic. In this paper I focus on those phonological processes relating to the Germanic diphthongs */ai/ au eu/. The runic data show a variety of spellings which are difficult to reconcile with the conventional history of pre-Old High German/pre-Old Saxon sound change.

The Germanic diphthongs in Old High German and Old Saxon

The Germanic *a*-diphthongs, both */ai/ and */au/, are subject to monophthongisation, unconditionally in Old Saxon and conditioned by the consonantal environment in Old High German (Braune 2004, §§ 43–45; Gallée 1910, §§ 89–101; Holthausen 1921, §§ 97–100).

Monophthongisation of */ai/ occurs:

1. before /r w h/. Inherited /h/ (< Proto-Germanic */x/) triggers monophthongisation, but the consonant-shifted reflex of */k/ does not: compare, e.g., *ēht* ‘property’ (< Proto-Germanic **aixtiz*), *eih* ‘oak’ (< Proto-Germanic **aikz*).¹
2. in certain interjections (*sē*, *sē-nu* ‘behold!’ < Proto-Germanic **sai*; *wē* ‘woe, alas!’ < **wai*). This is not a general rule in final position (compare *zwei* ‘two’ (neut.) < **twai*; *screi* < **skrai*, 1sg.pret. to *scrīan* ‘cry, moan’).
3. irregularly in other environments, e.g., *wēnag* ‘miserable, poor, low’ (< Proto-Germanic **wainagaz*/**wainaxaz*). The motivation for monophthongisation in these cases is not clear, but it is evidently not purely phonological, since formally similar words retain a diphthong, e.g., *weinōn* ‘to cry, wail’.

The Old High German reflexes of */au/ are monophthongal before /h/ derived from Proto-Germanic */x/, and before all dental/alveolar consonants.

Durrell analyses the monophthongisations into two stages: first, the off-glide is lowered to produce “pre-monophthongal” variants [ae ao]. The first element is subsequently raised as part of a general process affecting the first

¹ Unless indicated otherwise, all Proto-Germanic reconstructions in this paper are based on Orel (2003).

elements of complex vowel-segments in the late eighth or early ninth century: [ae] > [ɛ:]; [ao] > [ɔ:] (Durrell 1977, 59–63; see also van Coetsem 1975, 11–17).

Penzl (1971, 127 f.), on the other hand, argues that the digraphic spellings ⟨ae ao⟩² are simply an orthographic device for distinguishing the relatively open products of monophthongisation [ɛ: ɔ:] from the more close /e:/ derived from Proto-Germanic */e:/ (*ē2) and /o:/ derived from Proto-Germanic */o:/ (which are diphthongised in later Old High German).

The Proto-Germanic diphthong */eu/ undergoes several allophonic developments in Old High German and Old Saxon conditioned by following vocalics (this model draws on the accounts of Braune 2004, § 47; Klein 2001, 583; Krause 1971, 74–76; Nielsen 2000, 105 and 229; Ringe 2006, 221):

1. an allophone *[iu] appears before a syllable containing a high front vocalic (* /i i: j/), as part of the general raising of Proto-Germanic */e/ in this context.
2. *[iu] is also found before a syllable containing a high back vowel (* /u u:/), but not consonantal */w/. It is unclear whether this process is directly connected with the preceding one, or is an independent development. It is certainly attested in Old High German and Old Saxon, and possibly also in early Proto-Norse,³ which suggests that it may be common Northwest Germanic.
3. an allophone *[eo] develops before /a/ throughout Northwest Germanic, and (at least in Old High German and Old Saxon) before /e/ and /o/.

Following the loss of inflectional */-a/, which triggers change 3, the variants can be considered full phonemes */iu eo/ (see Findell 2012, 15–18, for more detail).

This pattern is retained in Old Saxon and in Frankish dialects of Old High German. In the Upper German dialects, however, a secondary process interferes with the inherited distribution of variants: */eo/ appears only before /h/ derived from Proto-Germanic */x/ or a dental/alveolar consonant. Before labial or velar consonants (including /h/ derived from Proto-Germanic */k/ via the second consonant shift), the surface form is always /iu/.

The consonant-conditioned alternation may be explained as blocking of the regular *a*-umlaut (* /eu/ > *[eo]) by the labial and velar consonants

² The ⟨ao⟩ digraph is widespread in Bavarian texts of the eighth and early ninth centuries, but is not found in Frankish or Alamannic (Braune 2004, § 45 n. 2).

³ The sole witness to this is **liubu** (Opedal stone, KJ 76), the reading of which is disputed; Antonsen (1975, no. 21) reads **leubu**.

(Braune 2004, § 47). We could alternatively regard it as a secondary raising of inherited */eo/ triggered by the labials and velars. This appears to be the model which Penzl (1971, 139 f.) and Wright (1906, § 56) have in mind.

Runic evidence for the monophthongisation of */ai/

The following runic sequences can be regarded with some confidence as containing reflexes of Proto-Germanic */ai/:

1. Freilaubersheim fibula: **wraet** → *wraet* 3sg.pret. ‘wrote’ (Proto-Germanic **wrait*).
2. Neudingen-Baar stave: **urait** → *wraet* 3sg.pret. ‘wrote’ (Proto-Germanic **wrait*).
3. Pforzen buckle: **aigil** → *Aigil* (Proto-Germanic **aiganan* [reconstruction after Ringe 2006, 261] > Old Saxon *ēgan*, Old High German *eigan* ‘to have, own’).
4. Pforzen buckle: **aīlrun** → *Ailrūn*, with the prototheme either *Ail-* (Proto-Germanic **ailan* > Old English *āl* ‘fire’; Nedoma 2004a, 168 f., and 2004b, 345 f.; Wagner 1999, 93 f.); or else a derivative of *Agil-* (Proto-Germanic **agez*/**agan* > Old English *ege* ‘fear’, or Proto-Germanic **agjō* > Old English *ecg*, Old Saxon *eggia* ‘edge’; Düwel 1997, 283 f., and 1999, 45).⁴
5. Pforzen ring: **urait** → *wraet* 3sg.pret. ‘wrote’ (Proto-Germanic **wrait*).

Several other sequences contain possible, though less certain, witnesses:

6. Neudingen-Baar fibula: **klef** → *klēf* < **klaiƿ*, 3sg.pret. to Proto-Germanic **klībanan* (> Old Saxon *(bi)-klīban* ‘to take root’; Old High German *klīban* ‘to adhere, stick to, be fixed to’; Fingerlin and Düwel 2002, 110).
7. Schwangau fibula: **aebi** → *Aebi* (Proto-Germanic **aib-ijaz*; cf. **aibō* > Langobardic *aib* ‘district’; Looijenga 2003, 257).
8. Weingarten fibula I: **aerguþ** → *Aergu(n)þ*, prototheme *Aer-* (Proto-Germanic **aizō* > Old Saxon and Old High German *ēra* ‘honour’; Looijenga 2003, 262).⁵

⁴ While **aīlrun** is the most popular transliteration of the Pforzen inscription, the first two runes are unclear, and plausible alternatives **alrun/alurun** have been proposed (Marold 2004, 227; Pieper 1999, 27–35).

⁵ Looijenga’s transliteration is at odds with the more popular **alirguþ** (Arntz and Jänichen 1957, 127; Bammesberger 2002, 119; Krause 1966, 306; Nedoma 2004a, 176; Opitz 1987, 49). Nonetheless, in my view both are plausible.

9. Weingarten fibula I: **feh**a → *Fēha* (Proto-Germanic **faixaz* I⁶ > Old Saxon and Old High German *fēh* ‘coloured, decorated’, or Proto-Germanic **faixaz* II > Old High German (*gi*)-*fēh* ‘hostile’; Arntz and Jänichen 1957, 128; Krause 1966, 306; Opitz 1987, 200); or *fēha* ‘colourful thing, i.e., rune’ (substantivised adjective < Proto-Germanic **faixaz* I; Schwab 1998, 418 f. and 1999, 13 f.).

As noted above, it is a matter of debate whether Old High German ⟨ae⟩ represents an intermediate diphthong [ae], a monophthong [ɛ:], or simply a free orthographic variant of ⟨ai⟩. The same may apply to runic **ae**. Our only clear example (Freilaubersheim **wraet**) has a find-site geographically separate from those of the other *wrait rûna* texts, in the Middle Rhine region.⁷ The **ae** digraph occurs in a context where we would not expect monophthongisation in Old High German. On this extremely scanty evidence we might tentatively postulate a variation between local orthographic traditions and/or dialects. Our other two (possible) **ae** spellings (Schwangau and Weingarten) are both located deep in Upper German dialect territory, and so are not amenable to this explanation unless it can be shown that the inscriptions were created elsewhere, or that the carvers were speakers of dialects from a hypothetical **ae**-zone.

If Looijenga’s transliteration of Weingarten I is correct, then **aergup** has **ae** in a context appropriate for monophthongisation in Old High German (before /r/). Schwangau **aebi**, on the other hand, does not. As for the **ai** of Pforzen **aïlrûn** (if this reading is correct), it is clear that this spelling does not reflect a general regional variation, since **ai** is found on the same object. This form cannot be explained as a pre-monophthongal phonetic variant, as it does not appear in a suitable phonetic environment.

Neudingen **klef** appears to contain a fully-developed monophthong in a position where it would not be expected in Old High German (3sg.pret. *kleib*, versus Old Saxon *bi-klēf*). It might be that this inscription reflects a more northerly dialect: the representation of /b/ as **f** (representing a fricative allophone [β] ~ [v]) is more reminiscent of Old Saxon and Middle Frankish than Upper German (Braune 2004, § 134). The remainder of the text is difficult to read (**klefilp** is generally favoured, with a variety of interpretations turning on a haplographic treatment of **f**; see Findell 2012, 188),

⁶ The homonym designations **faixaz* I and **faixaz* II are taken from Orel (2003).

⁷ The inscription has been classified as Rhine-Frankish (Arntz and Zeiss 1939, 213; Krause 1966, 283 f.), apparently on the assumption that the object and the inscription were produced locally to the find-site. This assumption must be treated with caution.

and gives us no clues which would enable us to identify the dialect with any confidence.

The presence of monophthongisation in Weingarten **feh**a is widely accepted, although Nedoma (2004a, 293–97) is sceptical, and suggests several alternatives with **e** representing an inherited monophthong (see also Düwel 1989, 44 f.). If we are dealing with a reflex of */ai/, the following /h/ provides a context suitable for monophthongisation. Given the range of suggested datings for the Weingarten fibula (estimates range throughout the sixth and seventh centuries), it is conceivable that **feh**a is a late sixth or early seventh-century form with an advanced monophthongal realisation; but we would need more substantial supporting evidence to give any weight to such a speculation.

The interpretation of **feh**a as a product of monophthongisation requires us to explain the apparent discrepancy between the monograph **e** and the **ae** digraph on the same object. We could posit a differential progress of the monophthongisation before /r/ as against /h/, which would be consistent with Braune's remark that diphthongal forms persist before /r/, but not before /h/ or /w/, in the earliest Old High German manuscripts (Braune 2004, § 43 n. 1).

With so few data, it is impossible to draw any firm conclusions. The only case where we can be entirely confident that we have a reflex of */ai/ represented as something other than **ai** is Freilaubersheim **wraet**, possibly explicable as evidence of a dialect in which unconditioned monophthongisation is underway. Weingarten **aergup** looks promising as a case of consonant-conditioned monophthongisation, but—as has been discussed—if we want to claim that the **ae** digraph represents a monophthong or some intermediate diphthong, we cannot simply ignore Schwangau **aebi**: our three **ae**-spellings all require different explanations. If the alternative reading of the Weingarten example as **alirgup** is correct, then we have only two witnesses which could as well be free variants as anything of real linguistic significance.

Runic evidence for the monophthongisation of */au/

The relevant sequences containing runic reflexes of Proto-Germanic */au/ are:

1. Igling-Unterigling fibula: **aunr?d** → *Aunrād*(?), prototheme *Aun-* (Proto-Germanic **aujan* > Proto-Norse *auja* 'luck', or the derived adjective **aunaz*/**aunuz* 'good, prosperous' > Old English (*ge*)-*ēan* 'pregnant'; Arntz and Zeiss 1939, 299; Krause 1966, 241 f.).

2. Lauchheim fibula: **aonofada** → *Aonofada*; or *Aono fa(ihi)da* ‘Aono made (the fibula? the inscription?)’ (Proto-Germanic **aujan* or **aunaz*/**aunuz*; see Nedoma 2004a, 194–96).
3. Nordendorf fibula A: **awa** → *Awa* (Proto-Germanic **aujan*).
4. Oettingen fibula: **auijabrg** → *Auijab(i)rg*, prototheme *Auija-* (Proto-Germanic **aujan*; Düwel 1991, 280; Findell 2012, 97, 443 f.; Nedoma 2004a, 137).
5. Pforzen ivory ring: **aodlip** → *Aodli(n)þ*, prototheme *Aud-* (Proto-Germanic **audaz*/**audan* > Old English *ēad* ‘prosperity, happiness’; Old Saxon *ōd* ‘happiness’; Nedoma 2004a, 191 f.).
6. Weimar buckle: **awimund** → *Awimund*, prototheme *Awi-* (Proto-Germanic **aujan*).⁸

Two further inscriptions contain possible—though doubtful—reflexes of **/au/*:

7. Lauchheim comb: **odag** → *ōdag* ‘rich, fortunate’ (Proto-Germanic **audagaz*/**audigaz*; Schwab 1999, 20).⁹
8. Mertingen fibula: **aun** → *aun* or *Aun-* (Proto-Germanic **aujan* or **aunaz*/**aunuz*; Babucke and Düwel 2001, 170).

Both of these involve speculative transliterations and interpretations; Düwel (pers. com.) has expressed caution about his interpretation of Mertingen. Schwab’s transliteration and interpretation of Lauchheim **odag** has not found wide acceptance (see note 9), and I do not consider it reliable.

These reservations aside, we have in this dataset two instances of a spelling **ao** (Lauchheim fibula; Pforzen ring) and (possibly) one of **o** (Lauchheim comb), all of which occur in contexts appropriate for Old High German monophthongisation (before alveolars, /n/ and /d/). On the other hand, we have **au** spellings before /n/ in Igling-Unterigling and Mertingen (if the latter is admissible).

The interpretation of the digraphs as reflexes of **/au/* is not controversial, yet the variation between **au** and **ao** has received little attention in the literature. Nedoma (2004a, 191 f.) regards Pforzen **aodlip** as either an idiosyncratic spelling or a dialectal/sociolectal variant, rather than an inter-

⁸ The two **aw** spellings (Nordendorf **awa**; Weimar **awimund**) are both believed to represent the name-element *A(u)w(i)-* derived from Proto-Germanic **aujan* via West Germanic gemination of **/w/*. Strictly speaking, the digraph **aw** is not simply an alternate spelling of */au/*, but a contraction of the phonemic sequence */auw/* (see further Findell 2012, 100 f.).

⁹ The sign which Schwab transliterates **o** is regarded by other commentators as a **g** (Düwel 1998, 16; Looijenga 2003, 265) or else a paratextual mark (Nedoma 2004a, 272).

mediate stage in the Old High German monophthongisation. He makes no comment on Lauchheim **ao**. I see no obvious geographical pattern that might indicate dialectal variation, and Nedoma does not explicitly adduce any evidence for a social or economic difference between the two spellings (such as differences in the quality and type of grave goods). The available information about dating is too imprecise for us to account for the variation chronologically.

If there is no positive evidence for a regional, social or chronological distinction between the spellings, we should not rule out the possibilities that (i) **ao** (and Lauchheim **o**, if admissible) indicate that monophthongisation is underway, and **au** in the same contexts is an “archaic” or “conservative” spelling; or (ii) **au** and **ao** are free orthographic variants, and **o** is either a misreading or does not represent a reflex of */au/.

Runic evidence for the developments of Germanic */eu/

We can be reasonably confident that runic reflexes of Proto-Germanic */eu/ appear in the following:

1. Bad Krozingen fibula A: **leub** → *leub* ‘dear’.
2. Engers fibula: **leub** → *Leub*.
3. Niederstotzingen strap end: **?liub** → *liub* ‘dear’.
4. Nordendorf fibula A: **leubwini?** → *Leubwini* (or *leub Wini* ‘dear to Wini’)
5. Schretzheim capsule: **leuba** → *Leuba*.
6. Schretzheim fibula: **leubo** → *Leubo*.
7. Weimar fibula I: **liubi:** → *liubi* ‘love’.
8. Weimar fibula I: **leob** → *leob* ‘dear’.

Two further witnesses may be present, although they are both highly problematic:

9. Mertingen fibula: **ieok a...** → *jeoka* ‘fight’(?) (Proto-Germanic **jeukō* > Gothic *jiuka* ‘quarrel’; Babucke and Düwel 2001, 169 f.). See above for Düwel’s note of caution about this item.
10. Weimar bead: **þiup/wiuw** → *þiup* (Verner’s Law alternant of Proto-Germanic **þeudjaz* ‘good’ (?); Krause 1966, 290; Nedoma 2004a, 314).

The most striking feature of this dataset is the predominance of the lexical root **leub-* ‘dear, lovely’ (whether as the adjective **leubaz*, the derived abstract noun **liubīn* > Old High German *liubi* ‘love’, or as a name-element). The only items which do not involve this root are Mertingen **ieok** and Wei-

mar **piuþ/wiuw**, both of which involve speculative and uncertain interpretations.

None of the witnesses provides us with clear evidence for the umlaut-driven split of **/eu/* into */iu eo/*. Weimar I **liubi** appears to contain a following high vowel, but the transliteration is questionable; indeed, Arntz's claim that a final **i** is present is partly motivated by the need to account for the spelling **iu** (Arntz and Zeiss 1939, 365–67; Findell 2012, 65).

If Weimar **piuþ/wiuw** is allowable as a witness to */iu/* derived from **/eu/* (which is doubtful), the initial **i-** of the following sequence **ida** (interpreted as the feminine name *Ida*) could provide a conditioning environment, if the umlaut process does not respect word boundaries (that is, if juncture is not a barrier to umlaut).

Mertingen **ieok a...** → *jeoka* appears to contain */eo/* conditioned by */-a/* (if we allow it at all). Weimar I **leob** is isolated on one of the fibula knobs, the relationship to the co-text being unclear. If this is a zero-suffixed reflex of **leubaz*, the underlying **/-a/* would produce */eo/* (→ Frankish *leob-Ø*, versus Upper German *liub-Ø*). Weimar I **liubi** and **leob** can be reconciled if we accept Arntz's reading of an *i*-rune and if we assign the inscription to a dialect in which Upper German consonant conditioning is not operative.

The most frequent spelling is **eu**, for which we can propose several possible explanations: (i) it is an orthographic archaism; (ii) it consistently represents one of the alternants */iu/* or */eo/*; or (iii) it is a free orthographic variant for both of them.

With the exception of Nordendorf **leubwini?**, every instance of **eu** occurs before an overt or underlying non-high vowel, where the umlaut process would regularly produce */eo/*. On the other hand, all of them appear in the root **leub-*, with a labial consonant which would regularly yield Upper German */iu/*. We could hypothesise that **eu** is either a free variant with **eo** for */eo/*, if the consonant conditioning does not apply; or with **iu** for */iu/*, if this conditioning does apply. If, on the other hand, we are dealing with a formulaic word **leub-* (see, e.g., Schwab 1998), it may be more resistant than other words to phonetically-motivated respelling.

Almost all of the inscriptions containing reflexes of **/eu/* come from sites well within Upper German dialect territory (the exceptions being Engers and Weimar). If all of the **eu** forms can be identified as dialectally Upper German, and if we accept the hypothesis that the Upper German consonant conditioning has taken place (as it must, if it is to be interpreted in terms of blocking *a*-umlaut, rather than as a later development of */eo/*), then **eu** may simply be a variant spelling of **iu** → */iu/*; although if this is the case, we might reasonably ask why **eu** is more frequent.

Conversely, if the **eu** sequences can be assigned to a regional dialect and/or to a chronological stage in which the Upper German consonant-conditioned change has not taken place, then **eu** might be an orthographic variant of **eo** → /eo/, which leaves us with the same question about frequency.

A simple solution to this is to hypothesise that **eu** is simply an archaism, as discussed above. Alternatively, we could postulate that the Upper German consonant conditioning is underway, but that in the dialects of the inscriptions it has reached an intermediate stage, with only the off-glide assimilated by the following consonant. This is not plausible in the “blocking” model of the change (in which /iu/ before a labial or velar is simply an inherited */iu/ unaffected by *a*-umlaut); but if Upper German /iu/ before a labial or velar consonant with a following non-high vowel is a secondary development (i.e., Proto-Germanic **leub-a-* > pre-Old High German **leob-a-* > pre-Upper German **leob-Ø* > **leub-Ø* > Upper German *liub-Ø*), then it is conceivable that the off-glide */o/ is raised under the influence of the following /b/. In Vennemann’s account (1972, 879), the dentals and /h/ do not block *a*-umlaut because the back of the tongue is relatively low during their articulation. This implies that the labials and velars involve a relatively high tongue posture which attracts the off-glide (*[o] > *[u]). The raised off-glide might in turn exert an assimilatory raising of the on-glide *[e]. A model of this sort does, however, require us to explain the **iu** spellings as either umlaut forms or “advanced” forms of the Upper German consonant conditioning.

The Engers witness may be problematic for this hypothesis: the find-site is in Frankish dialect territory and there is no evidence that it originated further south (though the possibility cannot be ruled out). The **eu** spelling in this instance is probably best accounted for as an archaism.

Mertingen appears to be anomalous in any model of Upper German consonant conditioning. Here we have an **eo** spelling with plausible umlaut-conditioning (if juncture is transparent to umlaut), but with a velar consonant, found well within Upper German territory. The fibula is an imitation of the “Nordic” type, which was probably manufactured in mid- or southern Germany (Martin 2004, 179 n. 45). We can, then, cautiously suggest that the Mertingen inscription may originate in an area in which Upper German consonant conditioning is not operative, and came south as an import.

The doublet of Weimar I **leob**, **liubi** is at odds with Upper German consonant conditioning (regardless of what model we use), unless we claim that the two examples belong to different dialects and are the work of different carvers. This is certainly possible: Nedoma comments that this

inscription and that on the paired fibula (Weimar II) are the work of multiple carvers and therefore contain multiple texts (Nedoma 2004a, 258), although he does not claim that different dialects are involved. The most straightforward explanation for the forms of Weimar I is as umlaut alternants in a non-Upper German dialect, as I suggested earlier.

The only case where Upper German consonant conditioning must be operative is Niederstotzingen **liub** (and even this is open to question, the co-text being unintelligible). If we are to claim that the Upper German distribution of /iu/ and /eo/ is present in the “runic” period, then we have also to find some other way of accounting for Mertingen **eo** (if we are prepared to accept Düwel’s speculative interpretation). Some hypotheses which would account for the data are:

1. The **eu** spellings represent an intermediate */eu/ derived from */eo/ (and Upper German consonant conditioning is a matter of raising triggered by labials and velars, rather than blocking of *a*-umlaut). Mertingen is an import, or an indicator that the raising process affects labials before it affects velars, or does not in fact contain a reflex of */eu/. Niederstotzingen is a later witness, with a fully developed Upper German /iu/. Engers is an isolated archaism, or an import from the Upper German area.
2. The **eu** spellings are archaisms in free variation with **iu** → Upper German /iu/ : **eo** → Frankish /eo/, and Upper German consonant conditioning on either the “umlaut-blocking” or the “raising” model is operative. Mertingen is an import, or is inadmissible (see hypothesis 1).
3. Upper German consonant conditioning is a later development (and must therefore be explained by the raising model rather than the umlaut-blocking model), attested only in the relatively late Niederstotzingen example. **eu** is an archaic spelling which can stand for any reflex of Proto-Germanic */eu/.

Conclusions

For each of the Proto-Germanic diphthongs, we have alternations between several graphic representations: */ai/ → **ai** ~ **ai̅** ~ **ae** (~ **e?**); */au/ → **au** ~ **ao** (with **aw** a related form, and one possible — though doubtful — case of **o**); */eu/ → **eu** ~ **iu** ~ **eo**. Of these sets of alternants, the reflexes of */au/ come closest to matching the conditions for the changes attested in the later dialects (in this case, monophthongisation); but even here, the small quantity of data limits the strength of our conclusions.

Because the conditions for the Old High German monophthongisation of */au/ are similar to those governing the Upper German distribution of the reflexes of */eu/, we might look for a common phonetic explanation. The runic data are of limited use for this purpose: reflexes of */au/ are attested only before alveolars (where the development [ao](?) > [ɔ:] is regular in Old High German), while we have reflexes of */eu/ only before labials and velars (where the surface form in Upper German is /iu/). The only reflex of */eu/ which cannot plausibly be accounted for as a product of umlaut is Niederstotzingen **liub**. If the */eu/ data can be explained without reference to consonant conditioning, and if there is no direct overlap between the consonantal environments of the attested reflexes of */eu/ and */au/, then we do not have grounds to advance a hypothesis in which their distributions can be viewed as part of a single process. This is not to say that (aside from Mertingen **ieok**, if admissible) the data are inconsistent with a hypothesis in which */eu au/ develop into *[iu au] before labials and velars and *[eo ao] before dentals and /h/ in Upper German dialect territory (*/eo/ appearing only where it is motivated by umlaut).

The suggestion of conservative spelling in accounting for the form **eu** is superficially appealing, but it presents us with a dangerously easy way to dispose of anomalies. How are we to evaluate the gap between spoken and written language? Who is enforcing the conservative orthography, and by what means? The situation differs from that of manuscript production in the Old High German/Old Saxon period, which we know to have orthographic conventions which can be transmitted through the institutions of the scriptoria. We have no evidence for the existence of comparable institutions governing the production of runic inscriptions.

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The Manx Runes and the Supposed Jæren Connection

Michael P. Barnes

Abstract

It has been argued that there is a connection between the Scandinavian runic inscriptions of the Isle of Man and a group from the district of Jæren in south-western Norway. The Manx inscriptions are dated on art-historical grounds to c. 930–1020, the Jæren group to around the year 1000—partly because they seem to span the period of the conversion of Norway to Christianity, partly on the basis of their rune forms and language. There are problems with these datings, not least for those who have considered Manx runic tradition influenced by that of Jæren. There is also a mismatch between the 930–1020 period assigned to the Manx inscriptions on art-historical grounds and the testimony of their rune forms and language, which suggests that many of them at least may come closer in time to the Jæren group. This article examines previous contributions to the debate and analyses the data from both Man and Jæren. It has two main aims: to inject clarity into the discussion and to distinguish fact from assertion and uncertain hypothesis.

Keywords: Isle of Man, Jæren, Manx crosses, runes, short-twig runes, runic inscriptions, rune-stones, Scandinavian language history

Introductory remarks

The year 1998 saw the publication of an article by Katherine Holman entitled “The Dating of Scandinavian Runic Inscriptions from the Isle of Man”. The article was based on a seminar paper given the year before at the Senter for middelalderstudier, Trondheim. The topic was apt, for the aim of the seminar was to throw light on various problems involved in dating inscriptions—runic inscriptions in particular.

The Isle of Man seems to have been a hive of runic activity in the mid- to

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late Viking Age. Over thirty stone inscriptions from that period, complete or fragmentary, have been found on the island. Comparison of this number with the approximately fifty rune-stones known from Viking Age Norway has over the years caused surprise and even astonishment in runological circles. The Isle of Man is after all only some 570 km² in area, insignificant when measured against Norway's roughly 324,000 km². And although thinly populated, Norway must have had vastly greater numbers of inhabitants than Man.

Traditionally almost all of the Manx runic inscriptions have been dated to the period c. 930–1020. The basis for this dating is chiefly art-historical, though runological features have been offered in support. Holman detects a conflict between the art-historical and runological evidence and suggests a later time-span for what she calls the “mainstream” of the Manx runic corpus. Her preferred dating, however, is not radically different from the traditional one: for the 930–1020 period she substitutes the slightly later 950–1025 (1998, 51). The reasons Holman adduces in support of her proposal can in summary be reduced to two. First: there is some evidence that the Borre art style found on certain of the Manx rune-stones remained in favour in the western Scandinavian colonies longer than the 850/75–925/950 period to which it is usually assigned. Second: there are a number of Norwegian rune-stones, dated on various grounds to shortly before or after the year 1000, which exhibit rune forms and orthographical practices seemingly identical to those found in the majority of the Manx inscriptions.

This line of reasoning runs up against various difficulties and uncertainties. David Wilson, one of the leading experts on Viking Age art forms, and on the Viking Age in the Isle of Man in particular, re-affirms the traditional dating of the Borre style ornament in the Manx corpus, on the basis not only of parallels from Scandinavia but also from north-west England (Holman 1998, 52). But even were we for the sake of argument to accept a slightly later art-historical dating, it is unclear where the Norwegian group of rune-stones referred to by Holman comes into the picture. If the Manx and the Norwegian stones concerned are indeed related, the re-dating of the Manx “mainstream” to 950–1025 is hardly a big enough leap forward in time. For Holman seems to assume that any influence there may have been from the one tradition on the other went from Norway to the colonies. But it is hard to claim that runic practices documented in Norway around the turn of the millennium underlie a Manx corpus dated 950–1025. Of course, the relevant practices may have existed in Norway for some time before they are first attested in that country, but that is no more than uncertain hypothesis. Possibly the influence went not from Norway to Man but in

the opposite direction, as suggested long ago by such luminaries as Sophus Bugge and Magnus Olsen (see below). Holman appears reluctant to think in those terms, but in the absence of other straws to clutch at does ultimately express willingness to reconsider arguments in favour of Manx influence on Norwegian runic writing (1998, 52). Yet even if we countenance a scenario in which Man becomes the *primus motor* in runic innovation, it is hard to see the relevance of Holman's revised dating. Do Manx runic practices become better suited to influence Norwegian rune-carvers if documented in the period 950–1025 rather than the slightly earlier 930–1020?

I am not an art historian, and therefore in no position to engage in serious debate about the dating of the Borre, Jelling, Mammen and Ringerike styles. I want instead to examine a question Holman's article rather begs: is there a demonstrable connection between runic writing in Man and any part of Norway in the tenth and/or eleventh centuries? And what is the nature of the connection, if any? Which leads on to the wider question: what place does Man occupy in Scandinavian runic tradition as a whole? Consideration of these matters is overdue, involving as it does areas of runological endeavour that have seen much in the way of casual suggestion or assertion but relatively little sifting of evidence.

Previous scholarship

Many of the conflicting views that have been expressed about Scandinavian runic writing in Man are bald claims rather than evidence-based conclusions, and as such hardly merit detailed scrutiny. It is, however, worth summarising the principal contributions, both to give a flavour of the debate and to establish a starting point for a more critical examination of the data.

P. A. Munch was the first scholar seriously to get to grips with parts of the Manx corpus (see, e.g., Munch 1850). His efforts were mostly directed towards the reading and interpretation of individual inscriptions. He did, however, assign the bulk of the material he tackled to a particular class, which he called “den *sudrøiske*” (‘the Hebridean’). The basis for this classification was the occurrence of **b** in the form **ḁ**, and the reason for the designation ‘Hebridean’ the use of a **b** of that shape in the Hunterston brooch inscription (SC 2), discovered in West Kilbride, Strathclyde, in 1826 or 1830.

It seems to have been Sophus Bugge who first proposed a close connection between runic writing in Man and south-west Norway. He expressed this and associated views in a number of publications, but the main plot is succinctly summarised in his 1902 monograph on the Hønen inscription from Ringerike (N 102). This contribution appeared under the general title *Norges Indskrifter*

med de yngre Runer and is thus a harbinger of the corpus edition whose first full volume finally came out in 1941. Bugge conjectured that the “short-twig” type of the younger rune-row was brought to Man and other western colonies by people from eastern Sweden, more specifically from Gotland and Östergötland. In Scotland they encountered Norwegians, especially groups from the south-west. These Norwegians then took the Manx variant of the “short-twig” rune-row back home to Rogaland—Jæren in particular—from where it spread further inland (Bugge 1902, 20). In support of the Gotland and Östergötland origins of runic usage in the western colonies and Norway, Bugge stresses (in another work) that the most prolific Manx rune-carver was called *Gautr*: “Es ist bemerkenswert, dass der Mann, der die meisten Inschriften dieser Art auf Isle of Man eingeritzt hat, *Gautr* heisst” (1910, 158).

Bugge’s ideas were refined by his pupil, Magnus Olsen (e.g. 1933, 89–92). Unlike Bugge, Olsen did not believe that the “short-twig” type of the younger rune-row had been exported directly from the Baltic to the Scandinavian colonies in the west. Instead he identified an area corresponding roughly to modern Vestfold as the birthplace of the “short-twig” runes, from where they spread to the west of Norway and beyond to the lands across the sea. Bugge had at least in part been influenced by chronological considerations. He and others were convinced that the bulk of the Manx inscriptions pre-dated those from Jæren, and therefore it seemed natural that the influence had flowed from west to east rather than in the opposite direction. Olsen drew attention to the “short-twig” runes from the ninth-century Oseberg and Gokstad ship burials, and to other “short-twig” inscriptions from Norway older than those found on the Isle of Man. On a crucial point Olsen did however agree with Bugge. He envisaged a special relationship between the Manx rune-stones and those from Jæren—so much so that he established a sub-group of the short-twig type, which he dubbed “Man-Jær-runes” (‘Man-Jæren runes’). This group is characterised primarily by the occurrence of ᚠ **b**, to which Munch had attached particular importance, and of ᚿ **m** (as opposed to earlier “short-twig” ᚠ **m**). However, detailed examination of Olsen’s work reveals that the boundaries between “Man-Jæren” and other “short-twig” varieties can be quite elastic (see further below).

Several decades later Ingrid Sanness Johnsen scrutinised runic usage on Man in connection with her study of the “short-twig” runes, which, following Carl Marstrand, she called “stuttruner” (‘short runes’; 1968, 1). Sanness Johnsen places greater emphasis than her predecessors on the historical background, which she tries to reconstruct on the basis of archaeological finds, place-names, and Snorri Sturluson’s history of the kings of Norway. And in the tradition of Magnus Olsen she seeks to identify

personal names in runic inscriptions with historical characters (1968, 100–08). This is a highly speculative foray, and the conclusions Sanness Johnsen draws correspondingly uncertain. She finds that in the ninth century there was Danish influence in Vestfold and other parts of eastern and southern Norway. Swedish influence also made itself felt in parts of Vestfold, in Agder, and more widely in the south-west. Sanness Johnsen seeks support for this interpretation of ninth-century Norwegian history in runic usage. “Short-twig” runes she associates with Sweden, “long-branch” with Denmark, and in the first two younger-futhark centuries, roughly the ninth and tenth by Sanness Johnsen’s reckoning, both types are attested in south-east Norway, whereas elsewhere in the country “short-twig” runes dominate. In trying to follow her line of argument from inception to conclusion I find it hard to decide whether this distribution of rune types really is an additional piece of evidence supporting the general findings, or whether the archaeological, place-name and literary evidence has been marshalled to account for the runic distribution map.

Sanness Johnsen goes on to note that there are very few Viking Age rune-stones from Norway and to make the following proposal: “Når bautastener med innskrift likevel forekommer i større antall enn tidligere, skyldes dette heller en sterkere innflytelse fra Vesterhavslandene med Isle of Man” (“The fact that raised stones with an inscription nevertheless occur in greater numbers than before is more likely to be due to increased influence from the western colonies with the Isle of Man”; 1968, 107). This proposal is based partly on a perceived coalescence of runic usage between Man and south-west Norway, partly on the appearance of rune-stones in Norway which “har korsform eller korsornament i vestlig kristen stil” (‘are in the form of a cross or decorated with a cross in western Christian style’; 1968, 107).

Thus, in Sanness Johnsen’s scenario, as I understand it, “short-twig” runes as a type most probably came to Norway from Sweden, arriving there in the early 800s. The type was then taken by Viking invaders, most likely hailing from eastern Norway, to the British Isles (1968, 106). Somewhere there perhaps, if not already in (eastern?) Norway, a particular “short-twig” variety arose, which was later exported back to south-west Norway, Jæren in particular. That variety is what certain runologists have called “Man-Jæren runes”. When these runes were first adopted cannot be established with any certainty, but they occur on the Kaupang hanging bowl (N 579) dated to c. 900 and regularly on the Isle of Man after c. 930. Equally unclear is when they made their triumphant journey back to Norway from the west, but their use in rune-stone inscriptions from Jæren generally dated to around the year 1000 provides a *terminus ante quem*.

As befits a sober and sceptical scholar, the next major contributor to the debate, R. I. Page, fails to produce such wide-ranging and exciting conclusions. Indeed, he offers little in the way of conclusions at all, often asking questions rather than suggesting answers. He queries the concept of “Man-Jæren” runes, pointing out: “This is less a type than a particular selection of items from the total number of rune forms available, a particular choice of the forms for **b**, **h**, **m** and **q**.” He goes on to stress the difficulty of identifying “a specific selection of rune forms in short or damaged inscriptions which may retain no examples of certain significant letters” (1983, 134f.). Earlier workers in the field, not least Sanness Johnsen, were often happy to assign the runes of an inscription to the “Man-Jæren” category on the basis of a single form. Page details some of the variety in Manx runic usage, without offering dogmatic explanations for its occurrence. He identifies the Manx carvers’ use of the word ‘cross’ rather than ‘stone’ to describe the monuments they erected as a factor that “encourages us to treat them as a coherent group” (1983, 135). He observes that the Manx runic crosses exhibit two fundamentally different kinds of lay-out: the runes are either cut along the narrow edge of the slab, or set on the broad face, on one side of the stem of a relief cross. In both cases the normal direction of writing is upwards. The second type, he notes, is not found in Norway, or for that matter Denmark, in the Viking Age.

Page considers there to have been different strands of runic usage on Man from the start. He also shows how up to the early part of the eleventh century the rune forms found on the island mirror the main lines of development known from Scandinavia (certain of the apparently later inscriptions incorporating dotted runes, for example). Thereafter runic activity among the Manx appears to tail off, and when a certain ‘Iuan the priest’ employs the script as late as the twelfth century, he copies the forms found in the earliest inscriptions. A degree of isolation seems to have set in. On the question of the relationship between the several Manx runic traditions Page identifies and those of Scandinavia, he is circumspect. He agrees that the earliest of the Manx inscriptions show a clear connection with Norway, but also emphasises the Celtic element. He is cautious about the direction of travel taken by runic innovations: “here I have assumed that new runic styles developed in Scandinavia and moved then to Man, but it may have been the other way round” (1983, 139). In a later contribution Page suggests that Iuan the priest’s two twelfth-century inscriptions (MM 144, MM 145) do not represent the end point of an ancient tradition at all but are antiquarian constructs, “the work of a man not well acquainted with runes, and perhaps not even with the Norse language” (1992, 136).

In her book on the historical context of the Scandinavian runic inscriptions of the British Isles, Katherine Holman examines different aspects of the Manx runic crosses, offering a number of suggestions but drawing few conclusions (1996, 86–172). She does however strongly urge reconsideration of the dating of the crosses in the light of the rune forms, orthography and language they exhibit, and their connection with the Norwegian stone inscriptions of Jæren, dated somewhat later than the bulk of the Manx corpus. She notes Olsen’s view that runic activity in Man provided the inspiration for “the early Norwegian inscriptions”—indeed that the Manx memorial formula formed the basis of the ubiquitous Scandinavian: ‘NN raised this stone in memory of MM’. She herself thinks Manx influence on the Jæren stones possible, but baulks at the wider implications: “... it is more difficult to see a small island like Man as the source of a custom of inscribing stones with Scandinavian runes in a formula which spread out across the whole of Scandinavia, rather than the other way round” (1996, 169). Furthermore, she notes, there are no inscriptions in Man that suggest experimentation with the basic memorial formula, as one might expect in a developing tradition; the formula appears to be fixed from the start, though with ‘cross’ substituting for ‘stone’. This is in contrast to Norway, where, according to Holman, there are “early rune-inscribed monuments that suggest the experimental beginnings of the memorial inscription”. She also draws attention to the Kilbar cross-slab from Barra in the Hebrides (SC 8), an artefact of disputed age, which rather than ‘NN raised this cross after MM’ seems to record ‘After NN is this cross raised’. A further pointer to dating, she thinks, is the Manx Andreas V inscription (MM 111), written in cryptic runes that have so far defied reading and interpretation. Holman notes that these have been compared with the cryptic runes of the mid-twelfth century Maeshowe No. 15 carving, and finds that they “suggest a degree of sophistication and experimentation that fits better with a later date” (1996, 169). In many ways Holman’s 1996 consideration of the Manx corpus foreshadows the sentiments expressed in her 1998 article, with which I began the discussion.

An examination of the material

This is then a suitable place to begin my own examination of the material. What does it consist of, and what, if anything, can it tell us about the position of Man in Scandinavian runic tradition? I will look at evidence to be derived from nine different areas of possible relevance: written sources; archaeology; onomastics; runography (rune forms in particular);

orthography; language; content (what inscriptions say and how they say it); lay-out (how inscriptions are placed on the stones); art. (Andersen 1995 provides a critical up-to-date summary of the debate about the Norse settlement in Man and its consequences.)

Written sources

A recent account notes that “written sources for the history of the Viking Age in the Isle of Man are mostly brief, tenuous, sometimes corrupt, and difficult to use” and finds that “no coherent story can be built up from them” (Wilson 2008a, 385). This appears to reflect the general view, and it is one in which I concur. There is no hint in these sources of a concentration of immigrants of south-west Norwegian descent, and nothing that might shed light on the origin or origins of runic writing in Man.

Archaeology

Archaeological evidence indicates that Scandinavian settlement of Man began in the late 800s. Grave-goods suggest that some of the earliest settlers may have come from north-west England or Scotland, but later waves appear to have included Scandinavians from Ireland. From what we otherwise know of patterns of Viking emigration, we would expect the bulk of the ninth and tenth-century settlers to have been of Norwegian descent, but there must also have been some of Danish extraction. How far the new arrivals overwhelmed the indigenous population is a question that cannot be answered by archaeology. However, the fact that the incomers seem to have adopted Christianity as early as the first quarter of the tenth century points to a significant native presence. And the inscriptional evidence (see below) speaks strongly of a mingling of Scandinavians and Celts. This meagre outline does not assist greatly in determining the inspiration behind runic writing in Man, but it would perhaps be strange if there were not a Norwegian element. There are however no positive indications that that element is to be associated with the south-west of the country. (For the latest survey of the archaeology of the Isle of Man, see Wilson 2008b.)

Onomastics

Man boasts a great many Scandinavian place-names. Just as the archaeological evidence, however, they fail to deliver a clear message about Scandinavian settlement patterns. Gillian Fellows-Jensen, one of the foremost

experts on Scandinavian place-names in the British Isles, observes: “the onomastic material is in general agreement with the view that the settlement was basically Norwegian” (1983, 45), but she goes on to suggest that the many Manx place-names in *-by* reflect immigration “ultimately from the Danelaw” (1983, 46). By this she means that the inspiration for names in *-by* is Danelaw nomenclature, but that the people giving the names in Man may have come there by circuitous routes, some from east to west across the Pennines, some perhaps via Ireland “after the expulsion of the Vikings from Dublin in 902” (1983, 48). As far as runic writing in Man is concerned, the onomastic evidence thus underlines the likelihood of Norwegian involvement, but does not exclude influence from other areas. As with the literary and archaeological sources, there are no pointers to a strong input from south-west Norway. (For a thorough examination of Manx place-names, see Broderick 1994–2005.)

Runography

Rune forms offer first-hand testimony of runic usage, so it is as well here to go into a little detail. Many of the Manx rune-writers are thought to have operated with a futhark containing the following forms:

ƿ ᆑ ᆒ ᆓ ᆔ ᆕ ᆖ ᆗ ᆘ ᆙ ᆚ ᆛ ᆜ ᆝ (i)

f u þ ā r k h n i a s t b m l r

(The rune **r** is found in only one inscription, the sound it denoted having apparently gone out of use in the whole of western Scandinavia by the early or mid-tenth century.) It is, however, worth recalling Page’s warning about identifying “a specific selection of rune forms in short or damaged inscriptions which may retain no examples of certain significant letters” (1983, 134 f.). Thus, it has sometimes been assumed that a carver using, say, **ᆓ h**, **ᆛ b**, or **ᆜ m** would have employed the whole range just illustrated even though several of the diagnostic forms may be lacking. Magnus Olsen, for example, felt able to conclude: “With few exceptions the Manx inscriptions can be referred to script-group III [i.e. the one given above]” (1954, 156). He does acknowledge that “we are far from having characteristic material for an absolutely certain decision in all cases”, but nevertheless considers “there is every probability that here in Man, within a small and sharply defined geographical area, we have a collection of runic memorials which compose a homogeneous series by themselves”.

The rune-row portrayed above can be equated in all respects bar one with

the “short-twig” row as identified by various handbooks. The exception is Υ **m**, which runologists have been inclined to assign to the competing “long-branch” type. Certain early, probably largely ninth-century, rune-writers, used a “short-twig” row in which **a** took the form \uparrow , and **b** \uparrow , while others had a predilection for crossing branches, making $\tilde{\alpha}$ \uparrow , **h** \uparrow , **n** \uparrow , **a** \uparrow , **t** \uparrow , **b** \uparrow , and **l** \uparrow . In the Manx inscriptions, we find \uparrow once, but **b** is always \uparrow . Runes with crossing branches are uncommon in Man, and where they do occur it is in company with forms such as \uparrow **s**, \uparrow **t**, and they are accordingly assigned to the “long-branch” type. Of runes diagnostic of the row thought to be favoured by Manx writers, \uparrow **h** occurs in four inscriptions, \uparrow **b** in seven (excluding the twelfth-century Maughold I and II by Iuan the priest—MM 145 and 144), and Υ **m** in slightly less than half. Although the evidence is thus only partial, the likelihood does seem to be that most of the carvers of runic crosses in Man learnt and used a row more or less identical with the one above. At least, positive evidence that they did not is absent. All other things being equal modern runologists would perhaps be inclined to assign the set of rune forms concerned to the tenth or very early eleventh century, although dating by runic form can be a hazardous undertaking.

The term “Man-Jæren runes” leads us to expect the occurrence of the same set of forms in Jæren, south-west Norway. As in the case of the Manx crosses, the evidence is only partial in that most of the inscriptions concerned lack one or more of the diagnostic shapes. The form \uparrow $\tilde{\alpha}$ occurs on Njærheim II (N 224), though apparently with the value /ɔ/ (see below), Klepp I (N 225), Stangeland (N 239), and Helland III (N 245); \uparrow **h** is to be found on Klepp I and perhaps also II (N 226); \uparrow **b** appears on Njærheim I (N 223) and II, Klepp I and probably also II, and perhaps Stangeland; Υ **m** is a possibility on Stangeland, is fairly certainly documented on Helland II (N 244), and clearly documented on Helland III. On Helland II, on the other hand, we have a possible \uparrow **h** and \uparrow **b**, and on the Stavanger III cross (N 252) \uparrow $\tilde{\alpha}$ (denoting /o/; since the primary focus is on sound values, I have here and in the following dispensed with marking length in phonetic and phonemic notation). The Tu inscription (N 228) exhibits a rather different set of “short-twig” runes: it has \uparrow **h**, but crossing branches in place of the one-sided variety, as, for example, \uparrow $\tilde{\alpha}$, \uparrow **a**, \uparrow **t**, \uparrow **b**. The diagnostic forms associated with the Manx crosses and the south-west Norwegian stones are not limited to these two groups by any means. Three early Swedish inscriptions have \uparrow for **b** (cf. Sanness Johnsen 1968, 112–16, 120 f., 136–38), though one, Kälvesten from Östergötland (Ög 8), uses \uparrow , the supposedly older “short-twig” variant of **m**. Inscriptions from other parts of Norway than the south-west also exhibit

relevant forms, while the Eikeland stone from Hordaland (N 300), perhaps a little too far north to be part of the south-west group, has \uparrow for **m** as on Kälvesten. Other regions of the British Isles than Man show examples of the forms that characterise the “mainstream” Manx corpus: the Hunterston brooch (SC 2), found in Strathclyde, Scotland, has \uparrow **b** and Ψ **m**, while the Penrith brooch (E 15), found in Cumbria, north-west England, parallels the Manx diagnostic forms exactly: its almost complete futhark records, *inter alia*, \uparrow **ã**, \uparrow **h**, \uparrow **b**, Ψ **m**. But of course we have no idea who carved the runes on either brooch, or where.

It is hard to know what conclusions to draw from this rather haphazard collection of material. Clearly the rune forms associated with the Manx crosses and the Norwegian stones of the south-west are not the only ones found in these areas; equally clearly, the relevant forms also occur elsewhere. Nevertheless, the idea of a Man-Jæren connection has persisted. What has inspired the belief is perhaps above all the unusually high number of inscriptions from the two areas, coupled with the fact that many exhibit, or are deemed to exhibit, the same selection of “short-twig” forms.

Deviations from this selection include the more complex “long branch” variants and a small number of runes marked with the diacritic dot that became a regular part of runic writing as the Viking Age gave way to the Middle Ages. It should be noted, however, that the designation “long-branch” does not refer to shape alone, but can depend on what company the relevant rune or runes keep. For example, \uparrow in an inscription that also contains \uparrow , \uparrow , \uparrow will normally be deemed “long-branch”, as distinct from its “short-twig” counterpart \uparrow , but \uparrow in company with, say, \uparrow **t**, \uparrow **b**, \uparrow **l** is happily designated “short-twig” (see the Tu stone, discussed above). With that reservation in mind two of the Manx inscriptions exhibit “long-branch” types: Michael III (MM 130), which has \uparrow , \uparrow , \uparrow , \uparrow , Ψ , and Maughold IV (MM 142) with \uparrow , \uparrow , \uparrow , \uparrow . Four Manx crosses include dotted runes: Michael III and Maughold IV again, the former with both \uparrow and \uparrow , the latter with \uparrow alone, and German II and Onchan (MM 140, 141) with one or more examples of \uparrow (on the sound values to be assigned to these forms, see “Orthography” below). “Long-branch” and dotted runes are also attested in south-west Norway, but the inscriptions that exhibit these forms are excluded from the Man-Jæren group. The line of reasoning that leads to their exclusion seems to be the rather circular one that they lack the forms that would admit them as part of the group, though I do not think this is explicitly stated anywhere. It is also the case, however, that many originate outside the district of Jæren.

Orthography

The Manx runic crosses display several noteworthy orthographical features. In three of the inscriptions the fourth rune, 𐌺, stands for /o/ rather than /ā/. This innovation, commonly dated to the first half of the eleventh century, is also, if sparsely, documented among the stones categorised as belonging to the Jæren group. On Njærheim II (N 224) 𐌺 stands for /ɔ/, and on the Stavanger III cross (N 252) its left-facing variant 𐌺 represents /o/.

The use of **b** to denote the voiced spirant [β] is encountered twice on both the Ballaugh and Braddan IV crosses (MM 106, 135). This is an uncommon spelling (cf. Barnes 2004), and does not seem to occur on any of the Jæren stones. It is, however, documented in Scotland, and can be found in places as far apart as Greenland, Gotland and Denmark. It also occurs in a handful of Swedish inscriptions, and occasionally in Norway, as on the Alstad stone (N 61).

There is vacillation in the denotation of the /ei/ diphthong and certain monophthongs both in the Manx and the Jæren corpus. Thus, expected /reisti/ ‘raised’ is written **risti** on several of the Manx crosses, while conversely expected /re:tti/ ‘raised up’ appears as **raiti** on the Jurby cross (MM 127), and /pān:a/ as **paina** on Andreas IV (MM 113). Various of the Jæren stones have, or in some cases appear to have, **risti stin** for expected /reisti stein/, Njærheim I (N 223) has **stan** for /stein/, while the Tu (N 228) carver spells /helgi/ **hailki** and /ketil/ **kaitil**. Such vacillation is by no means unknown elsewhere, but is quite a prominent feature of both the Manx and Jæren corpora. The use of **au** for /ɔ/, on the other hand, found in both Man and Jæren, is a runic commonplace of the mid- and late Viking Age.

On the Braddan II cross (MM 138) the semi-vowel [w] is written **o** in the word **aiþsoara eiðsvara** ‘sworn ally’. The only parallel I can find in Rogaland is on the Sørbo II stone (N 260) from north of Stavanger, where the assumed personal name Sveinki is rendered **soinki**. Not only is this inscription not from Jæren, however, it also contains orthographical features that seem to mark it out as somewhat younger than the group that has been associated with the Manx crosses. On the other hand, the carver of the Greenmount bronze strap-end (IR 1) from Co. Louth, Ireland, renders *sverð* **soirþ**. Greenmount is hard to date; somewhen in the eleventh century seems to be the closest we can get. (Dotted runes are transliterated in accordance with the system set out and justified in Barnes 2012, 6f.; cf. Barnes 2011.)

Where 𐌺̇, the dotted form of **i**, occurs in the Manx corpus, it seems always to denote some realisation of /e/ (perhaps also of /æ/, to the extent this was a separate phoneme), though the precise shade of sound may be uncertain.

The value of \mathfrak{H} , the dotted form of **u**, is harder to determine. This rune form occurs twice in Kirk Michael III (MM 130), both times in personal names of Gaelic origin. In **mal:ümkun** it might stand for /o/, since it is possible the name means ‘servant of Lomchu’; in **mal:murü** *Maelmuire* ‘servant of Mary’ it denotes perhaps a more indeterminate end vowel. The use of \mathfrak{H} for /o/ has parallels, most notably in the present context in IR 12, an inscription from Dublin on a red deer’s antler, archaeologically dated to around 1000.

Language

The language of many of the Manx inscriptions differs in one way or another from that found in the Jæren group—and the generality of Scandinavian runic inscriptions for that matter. Prolonged and intimate contact with Gaelic, as evidenced not least by the many Gaelic personal names on the Manx crosses, seems to have led to language interference. Such interference has been identified in (a) the Norse inflectional system, (b) the word-order.

In several cases expected inflectional endings are missing from the Manx inscriptions. This affects in particular nominative masculine singular *-r* (a phenomenon occasionally documented in Scandinavia as well), while on Kirk Michael II (MM 101), ostensibly one of the oldest of the Manx series, there is a fairly clear example of a genitive masculine singular minus its *-s*. In Maughold V (MM 175) **kuinasina** rather than the normal accusative *kvinnu sína* ‘his wife’ follows the preposition **iftir** ‘after’. And then there is Kirk Michael III (MM 130), whose language has been described as “rotten Old Norse” (Page 1983, 137), a reference to the impossibility of construing certain endings in such a way that obvious sense emerges. In the matter of word-order attention has been drawn to the common occurrence of ‘son/daughter of X’ on the Manx crosses, rather than ‘X’s son/daughter’. While it is true that ‘Y son of X’ is the regular formulation in commemorative ogams (and standard patronymic usage in the insular Celtic languages), apparent parallels can be found in Scandinavia, as **sun:nairbis** ‘son of Nærfir’ on the Tryggevælde stone (DR 230), **tutur:kunars** ‘daughter of Gunnarr’ on Klepp I (N 225—one of the Jæren group), and **mupir:alriks:tutir:urms** ‘mother of Alrikr, daughter of Ormr’ on the Ramsund rock (Sö 101). A distinction needs to be made, however, between true patro-/metronymics and appositional phrases, the latter simply supplying additional information about a person. Unfortunately the difference is by no means always obvious. The Scandinavian examples just given are most plausibly considered appositional phrases, the Klepp I and Ramsund rock examples clearly so. With Gaelic in the background, the Manx cases hover

uncertainly between the two interpretations. The Bride cross (MM 118) offers the most persuasive example of a patronymic. There is nothing in its opening — **truian:surtufkals:raistikrþina:** ‘Druian, son of Dufgall, raised this cross’ — to suggest additional information is being offered about Druian; in addition the punctuation seems to favour the patronymic interpretation.

Other noteworthy language forms in the Manx inscriptions include *kvinna/kvinnu* for the more usual *kona/konu* ‘woman, wife’, which may have been a local variant (*kvinna/kvinnu* and *kona/konu* appear three times each); further **þínsi** ‘this [acc. m.]’ (German II, MM 140), which has been identified as East Norse, although *þensi* is also found on two probably early eleventh-century Norwegian stones: Kuli from Møre og Romsdal (N 449) and Vang from Oppland (N 84); the form also occurs on Sele II (N 237), one of the Jæren group (this cross-shaped monument is known only from a drawing, however), and is further documented on the Iona cross-slab (SC 14). Kirk Michael III (MM 130) has been deemed an East Norse inscription through and through: not only does it have “long-branch” rune forms, it also sports the form **þan** ‘than’ (as opposed to more usual *en*) and the personal name **apisl** *Aðisl*. Bugge considered it Swedish (1899, 243f.), while Olsen (1954, 216f.) declared it Danish. It is true that *þan* ‘than’ is documented in the Swedish and Danish runic corpora, and not apparently in the Norwegian, but the word ‘than’ is in fact not much used in Norwegian inscriptions at all. The name *Aðisl* or *Aðils* is not uncommon in West Norse sources, but seems to go out of favour in the West towards the end of the Viking Age.

The varying forms of the word for ‘after’ have regularly been used in evidence of an inscription’s age. The most recent general treatment of this question of which I am aware concludes that the short form *aft/æft* was in common use until about the beginning of the eleventh century, whereafter it fades away, though persisting perhaps longer in Norway than in Denmark or Sweden (Peterson 1996, 242–44). The author is less specific about the age of the long form *aftir/aftir/æftir/æftir*, but reading between the lines, it seems we should expect it to occur only sporadically in the latter half of the tenth century. It is thus something of a surprise to find that a slim majority of the Manx inscriptions have the long form, given that the conventional dating of the corpus is c. 930–1020. Of course the long form may be a feature of the stones to be dated in the later part of this period, but it is found on several that are otherwise deemed to belong the “mainstream” of Manx runic tradition (Holman 1998, 47). Of the Jæren collection about half have the short form, half the long, though in one or two cases the reading is uncertain, and there can also be occasional doubt about whether a particular stone is to be assigned to the group. This result is, however, more in keeping

with the late tenth to early eleventh-century dating traditionally bestowed on the Jæren corpus.

Content

Content-wise the big difference between the Manx and Jæren inscriptions lies in the term used to describe the monuments whose existence they proclaim. In Man people raised ‘crosses’, in Jæren, as elsewhere in Scandinavia, they raised ‘stones’. The term ‘cross’ is not, however, restricted to the Isle of Man. It is attested from other parts of the British Isles where Gaelic traditions were strong — Ireland (IR 2 Killaloe) and the Hebrides (SC 8 Kilbar, SC 10 Inchmarnock). It is seemingly even found once in Norway, but in Sogn og Fjordane rather than Jæren (N 417 Svanøy). Jæren boasts three stone crosses inscribed with runes: Njærheim I (N 223), Sele II (N 237) and Stavanger III (N 252; the first two known only from drawings), but they are all identified by the term ‘stone’ rather than ‘cross’.

There is otherwise little in the content of either the Manx or the Jæren inscriptions that could be called in any way remarkable. Most detail only the essentials: who raised the monument, after whom, and their relationship. Occasionally something more is said, but no obvious patterns emerge in either region. A certain Gautr proudly identifies himself as the maker of Andreas I (MM 99) and Kirk Michael II (MM 101)—but whether we are dealing with a single rune-carver here as opposed to one and the same stone-mason is a moot point (cf. Page 1983, 136). No rune-carvers’ signatures are found in Jæren.

Lay-out

Fundamentally the Manx runic crosses display two types of lay-out: the inscription either runs up, or occasionally down, a narrow edge of the slab, or is placed on a broad face running up one side of a sculptured relief cross. The narrow edge type seems to be the norm in most parts of Norway, though there the direction of writing appears almost always to be upward. In Denmark and Sweden rune-carvers prefer broad faces for their inscriptions, but they deploy the runes in a quite different way from the broad-face carvers in the Isle of Man. The Jæren inscriptions exhibit a wide variety of lay-out, with runes running up, down, or up and down, or round, a broad face, or up a broad face and a narrow edge, and occasionally up a narrow edge alone as commonly elsewhere in Norway (e.g. N 228 Tu). It would be difficult on the basis of lay-out alone to postulate a connection between the runic monuments of Man and Jæren.

Art

The art of the Manx crosses has been exhaustively discussed (two convenient survey articles are Margeson 1983 and Wilson 1983), so there is no reason to plough this well-tilled field yet again. According to Wilson, “elements of style and form were drawn from the regions round the Irish Sea” (1983, 178). Reviewing the complete corpus of the Manx crosses—those both with and without runic inscriptions—he identifies Borre, Jellinge-Mammen, and traces of Ringerike style, giving a date range of roughly 850–1025. His conclusion, on the other hand, is that “there is little likelihood that any of the Viking crosses were made much earlier than the second quarter of the tenth century”, the reason being “that the Borre style represented here is associated with fairly developed motifs” (1983, 185). The iconography of the Manx crosses, which combines Christian motifs with heavy doses of imagery from Norse myth and heroic legend, points to a period in which Christianity was established, but perhaps not dominant—the tenth century rather than the eleventh.

The art of the Jæren rune-stones is more difficult to capture: truth to tell, there is not a great deal of it, when compared with what exists in Man. Njærheim II (N 224) places its runes on either side of a ring-headed cross; Klepp II (N 226), a mere fragment, has ornament that is difficult to identify; one of the broad faces of Tu (N 228) shows two figures that have been interpreted as a man and a woman; the Reve sinker (N 230) has decoration in Ringerike style, but this is separate from the runes and it has been suggested it was put on the stone before the inscription; Helland III (N 245) has Ringerike ornament placed between its two lines of writing. Only in the case of the female figure on the Tu stone does there seem to be a connection with the art of the Manx crosses. Michael VIII (MM 123) depicts what appears to be a woman in the same attitude as and with similar clothing to the Tu figure, though there are differences of detail. It has also been claimed that one of the figures on Michael VI (MM 129) has “samme drakttype” (‘the same type of dress’) as the woman on the Tu stone (Sanness Johnsen 1968, 83), but this is less obvious. Art, just as lay-out, it would seem, offers relatively little to those who would establish a connection between the runic monuments of Man and Jæren.

Conclusions

It is not easy to draw hard and fast conclusions from the disparate collection of material that has been presented here. There appears to be little appetite

on the part of art historians to revise the dating of the Manx crosses, and it is difficult for a mere runologist to challenge this opinion. If we accept the 930–1020 span the art historians offer, there are a number of problems that arise.

Certain runic innovations appear earlier in Man than in Norway, from where Manx runic tradition seems ultimately to derive. According to current opinion, dotted l (†) came into use in Norway in perhaps the second quarter of the eleventh century, and was a regular feature of runic writing there by about 1050. Other dotted runes were a little slower to catch on, though ƿ is clearly if sparsely documented in the first half of the twelfth century, and there is the odd example of ʀ — never a common form in western Scandinavia since /y/ was normally denoted by ʏ. The use of the fourth rune, ʒ ʒ, with the value /o/ rather than /ā/ is an innovation reckoned to have taken place in Norway in the 1020s or thereabouts. On the assumption that influence flowed from Scandinavia to Man and not the other way round, it is unexpected to find dotted runes and ʒ ʒ for /o/ some fifty to a hundred years before these phenomena make their appearance in Norway. Of course Manx runic usage may not be derived solely from Norway. Dotted runes are documented in Denmark in the late 900s — but even that seems rather late to have influenced practice in Man. It could be that the Manx stones with dotted runes and ʒ ʒ for /o/ are among the latest of the series, but that is far from assured. Borre style has been identified on Michael III (MM 130), for example, and that should place it among the earliest of the Manx crosses, yet it exhibits two examples of ʀ (with uncertain phonetic value) and six of † (denoting a vowel in the region of [æ–e]). To solve this conundrum it has been proposed that the runes were added to a pre-existing cross (e.g. Shetelig 1920–25, 270), which may, or may not, be the case. There remains the possibility that innovations such as dotting and the changed value of ʒ ʒ arose in Man or elsewhere in the British Isles, and from there spread to Scandinavia, as suggested by Hagland and Page (1998; for a rebuttal of this view, see Knirk 2010). The Dublin runic inscriptions, it is worth noting, exhibit the odd dotted rune, including a clear example of ʀ (seemingly denoting /o/), dated to c. 1000. And in Anglo-Saxon runic writing the fourth rune (whose shape was altered to ƿ) came by the seventh century to have the value /o/, and a name, *ōs* ‘river mouth’ to match (of less certain age). For what it is worth, several scholars have proposed that Manx use of the fourth rune to denote /o/ derives from Anglo-Saxon practice (cf., e.g., Olsen 1933, 89).

Another problem is the common occurrence of the long form *af̊tir* ‘after’ in the Manx corpus. As noted above, this form would not be expected to

appear much before the end of the tenth century, yet in Man it is a clear rival to short *aft*. That Manx usage gave rise to the adoption of *aftir/aftir/æftir/æftir* in Scandinavia is even harder to believe than that the British Isles is the cradle of the dotted runes. While runic writing practices might just spread from a small island to a wider area, it seems much less likely that a (spoken?) linguistic form would do so.

The proposed connection between the Manx crosses and the rune-stones of Jæren is a further factor that looks odd from a chronological perspective. The Jæren corpus is commonly dated to a period around the year 1000 — partly on the basis of its rune forms and language, partly on the grounds that it spans the period of the conversion. I see little reason to dissent from this conclusion. If it is correct, however, the Jæren rune-stones as we have them cannot have influenced Manx tradition, let alone inspired it: they are simply too late. Of course, the extant Jæren stones may represent the final flowering of an ancient tradition, but for that there is no evidence. Again, one wonders — if there is a connection between Man and Jæren — whether the influence went eastwards rather than westwards. Surveying the Jæren stones, however, I find it hard to identify many similarities with the Manx crosses, as the preceding account of the material will have made clear. The most striking connection is in the choice of rune forms, but these are in essence the “short-twig” variety with the not uncommon replacement of I by Y . That is a selection we might well expect to find in various parts of the Scandinavian world in the tenth century, and in Norway as late as the early eleventh. We should definitely dispense with the term “Man-Jæren runes”, which seems to owe its existence solely to the belief in a connection between the runic writing of Man and Jæren. Certainly, if the crossing-branch runes of the Tu stone (N 228) can be considered of “Man-Jæren” type (Olsen 1933, 91), the term is devoid of content.

In arguing for a slightly later dating of the Manx crosses and for the dependence of their runic component on Scandinavian and particularly Norwegian models, Holman stresses, as we have seen, the uniform wording in Man of what she called the “memorial formula”. It is as though it came ready-made to the island, and its place of origin, she clearly believes, was Scandinavia. The matter is not that straightforward, however. Some have toyed with the idea that the Manx carvers might have modelled their formula on Anglo-Saxon usage — that *arærde æfter* ‘raised after’ might have given rise to *reisti æftir* (cf. Olsen 1933, 89). Palm (1992, 250) goes further, attributing use in Scandinavia itself of what he terms the “raiser formula” to Anglo-Saxon influence. Neither of these suggestions is perhaps very likely. To entertain them one would require a more vibrant English tradition

of rune-stone raising, and heavier Anglo-Saxon involvement in Man and Scandinavia than seems to be the case. But they indicate the uncertain terrain through which we move. Holman also mentions the Kilbar cross from Barra in the Hebrides (SC 8). This appears to say: *Aftir Þorgerðu Steinars dóttur es kors sjá reistr*, though there are lacunae and some difficulties with the reading. If the general understanding is correct, Kilbar also shows the “experimental beginnings” with the “memorial formula” Holman finds in Norway (1996, 169). Of course, the crucial question is what relationship there might be between the Kilbar cross and the Manx corpus. Differing views have been expressed about this. Shetelig deemed Kilbar “the starting point of the remarkable series of Norse monuments in the Isle of Man” (1954, 125), a view echoed by Liestøl (1983, 92), who goes so far as to ask whether its carver may have been Bjørn, father of the Manx carver, Gautr. Wilson (1983, 183) offers the opposite point of view, noting that many have thought the Kilbar stone influenced by Manx tradition. In our edition of the Scandinavian runic inscriptions of Britain, Page and I conclude that Kilbar is probably slightly older than the Manx crosses, but that a connection between the two is hard to demonstrate with reference to specific features (Barnes and Page 2006, 231 f.).

The results of the foregoing survey of the Manx crosses and their relationship with runic tradition in Jæren and the wider Scandinavian world are of necessity inconclusive. The crosses overall show a mixture of Celtic and Norse influence, while their runic inscriptions demonstrate a clear connection with Norway. The language is of West Norse type, and the lay-out of many of the inscriptions similar to that commonly found in Norway. The runes are fundamentally of the “short-twig” variety, as we would expect of the majority of Norwegian inscriptions dated in the tenth century. It is inconceivable that Norwegian runic tradition as a whole stems from the Isle of Man, so Manx runic writing must in some way come from Norway or from Norwegian colonies in the British Isles. The immediate source or sources cannot unfortunately be identified in the current state of our knowledge. It is tolerably clear, however, that the extant rune-stones of Jæren were not the inspiration. These must be later than the bulk of the Manx inscriptions, and the points of contact between the two do not appear particularly strong. There are indications of East Scandinavian input into Manx runic writing, which may have come direct from Denmark, or conceivably Sweden, but more probably perhaps from Danish settlers in areas of the British Isles adjacent to Man. There remains a conflict between the art-historical dating of the Manx crosses and the runic and linguistic forms they display. If we accept the art-historians’ view, there are two

possibilities. The first is that certain runic innovations took place earlier in the British Isles, or Man at least, than in Scandinavia. The second: that these innovations happened first in one or more parts of Scandinavia (or perhaps simultaneously in Scandinavia and the western colonies)—earlier, then, than the evidence currently at our disposal would lead us to believe. Whatever the truth of the matter, if the Kilbar inscription opens with the word *aftir*, as seems probable, and is to be dated to the early 900s, as I believe, it can be no surprise that many of the Manx crosses also exhibit the long form of the preposition.

I did not start out on this article with the expectation of reaching exciting and innovative conclusions. Which is just as well, for by and large I seem to have confirmed the status quo. However, I hope to have distinguished what is more from what is less plausible, and to have injected a modicum of clarity into the debate.

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Ráð þat, If You Can!

K. Jonas Nordby

Abstract

Inscriptions with rune-like symbols continue to challenge the ingenuity of runologists. Such inscriptions may take the form of meaningless scribbles, complex bind-runes, or garbled texts with a hidden message. This article presents five almost identical inscriptions from such diverse places as Sigtuna, Skara, Oslo and Bergen. All five inscriptions consist of runes with added, apparently superfluous, features, and it is argued that these can be interpreted in a way that has not previously been considered.

Keywords: Viking Age, Middle Ages, bind-runes, cryptic runes, reading orientation, Skara, Sigtuna, Bryggen in Bergen, Oslo

The medieval runic corpus contains a large number of uninterpretable, or at least uninterpreted, inscriptions. Not infrequently, runologists have to put them aside and admit defeat. The reason is often that the inscriptions are damaged and the runes largely unreadable, or that the object bearing them is fragmentary such that only a few runes or sequences of runes are preserved. But equally often an inscription may be complete and as clear as day, and yet make no sense. In many examples of the latter type the trained eye will spot the hand of a total illiterate scribbling rune-like symbols, or an unsteady and untrained writer trying to copy a runic text without any understanding of what it says. The most difficult cases to give up on are those that yield no sense even though the runes are well executed and the carving apparently secure. In an attempt to find a solution the dedicated runologist will try everything, viewing the inscription from all conceivable angles. In the following I present five runic inscriptions made with precisely that purpose in mind: to compel us to look at them from different angles in order to arrive at an understanding of the texts.

The five objects bearing these almost identical sets of rune-like symbols are

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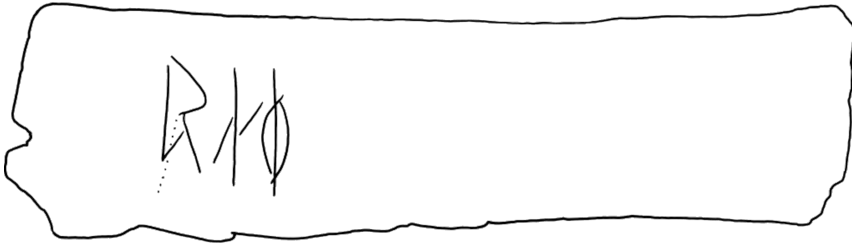


Fig. 1. Side a of the bone Sl 89 (U NOR2000;32B) Sigtuna. Drawing by the author.

all archaeological finds (three of bone and two of wood), from excavations carried out at different urban sites in Scandinavia: Sigtuna, Skara, Oslo and Bryggen in Bergen. They are dated to the late Viking Age and High Middle Ages. Each of them contains a set of three rune-like graphs or complex bind-runes. On four of the objects other inscriptions occur as well. In all five cases the runes seem clear (although in one instance only partially preserved), and to have been made with a definite purpose in mind.

In 1999 excavators found a roughly 12.5 cm long rib-bone incised with runes in the area “Professorn 1” in Sigtuna. The bone has been given reference code Sl 89 in the inventory of runic objects discovered in Sigtuna, and appears as U NOR2000;32B in the Scandinavian Runic Text Database. The bone has brief runic inscriptions on both of its broad faces, and its preliminary dating is to the early part of the 1100s (Gustavson 2001, 32 f.). In 2007 I was able to examine the bone. The drawing reproduced as figure 1, as well as the following account of the reading, is based on that examination.

On the convex side of the bone there are two apparently independent inscriptions. One consists of three or four runes. Conceivably these represent a hypocoristic name *Riki*, or nickname *Ruggi*, but the reading of the second character is too uncertain for any hard-and-fast conclusions to be drawn (Gustavson 2001, 33). The other inscription seems to be made up of two twig-runes—realisations of the *ættir*-cipher (based on the division of the futhark into three groups)—but the second lacks indication of the group to which it belongs: 1/5 0/3. Of greater interest for the current investigation is the inscription on the concave side of the bone. It is here we find the three complex rune-like symbols. The first character is clearly *r*. The knife appears to have slipped, however, as the writer, after carving the bow, directed it away from the vertical to start the tail: from this point a light scratch proceeds down to the left. Clearly deliberate on the other hand is a cut that runs up to the right from the base of the vertical. Helmer Gustavson considers this latter branch to be unintentional, although he admits the possibility that it

gives a malformed **b** (Gustavson 2001, 32). The next rune can be taken as a long-branch **a**, but if so, its branch has been cut in two separate sections, one on each side of the vertical. Gustavson notes that the section to the right does not quite meet the vertical, but fails to mention the clear difference of height between the two, which makes them appear independent of each other rather than as elements of one and the same diagonal line. The third and last character appears to be a double-sided **þ**. The top of the left-hand bow overcuts the vertical as does the bottom of its right-hand counterpart.

The reading and interpretation of the three runes that immediately suggest themselves are **raþ**, ON *ráð*, imperative of the verb *ráða*. (I use Old West Norse normalisation throughout, even though several of the inscriptions discussed are from Sweden.) In runic inscriptions the imperative form of this verb occurs almost entirely in the sense ‘read!’ or, more specifically, ‘interpret!’ (Dverstorp 2000). In the present case we might conclude that ‘interpret!’ is the intended meaning, since the deviant runes imply a challenge to the reader. The material from Sigtuna embraces several other inscriptions with the same kind of challenge. These are Sl 9 (U Fv1983;229) side b: ... *ráð þ[ú] rúnar!* written with twig- and bow-rune realisations of the *ættir*-cipher; Sl 74 (U NOR2000;25B) [*R*]*áð þú rúnar!* written with additional verticals placed between each rune; and Sl 91 (U NOR2000;34A) side a: *Ráð þú!* written in the *ættir*-cipher using short vertical lines to indicate the group and full length verticals (*íss*-runes) to mark position within the group. In our case, Sl 89, it would appear that *ráð!* refers to the challenge posed by the fact that the runes have apparently been made more complex through the addition of extra branches and bows. The disguising of runes through the use of seemingly meaningless additions is a feature runologists have identified in a number of inscriptions. Börje Westlund’s reading (1989) of the first nine runes on the Kvinneby copper plaque (Öl SAS1989;43) involves the exclusion of particular features which he deems superfluous. Similarly, Elena Melnikova’s understanding of the runes on the Gorodišče amulets (X RyMelnikova2001;181 and X RyMelnikova2001;189) assumes that certain graphic features have no phonetic value (Melnikova 1987, 165). Although this approach throws up obvious methodological problems, several interpretations based on it have achieved widespread acceptance (MacLeod 2002, 166–73). Viewing the Sigtuna inscription Sl 89 in the same light, one ought to be able to dismiss the superfluous branches and bow as features designed simply to make the reading more difficult — concealing the very exhortation *ráð!* ‘interpret!’.

We can, however, approach the inscription in a different way, and start with the presumption that the “superfluous” features are not after all super-

fluous, but in fact indicate sound values. Read as traditional bind-runes the characters appear to have little to offer. The branch rising from the base of **r** might perhaps be taken together with the tail and understood as **a**; the branch to the right of the second vertical could be assumed to indicate **k**, even though it does not extend to full height; and the third rune might with some reservation be read as two **ps**. This would give **raakþþ**, a reading that does not seem amenable to interpretation — or even convince as a runic text. If on the other hand we read the inscription — moving the whole time left to right — first one way, then turn the bone round 180° and read the same characters the opposite way, the graphic features which emerge all give good sense. With the one orientation we get **R1þ** and with the other **þ11**. This offers a wholly satisfactory reading and interpretation: **rap þat**, *rād þat!* ‘interpret that/this!’. Such a procedure is so far unknown in runic writing, and there is thus good reason to investigate the reading more thoroughly. If we look closely at the carving itself, we can find features that support the suggested change of orientation. The bow of **r** was clearly cut the expected way up. We can deduce this from the fact that the knife slipped downwards as the carver was completing this part of the character. We may further note that the bows of the final rune are both incised in the same way, but upside-down in relation to one another. The bow on the right does not connect with the vertical above, while it is mildly overcut below. Correspondingly, the bow on the left does not connect with the vertical below, but is overcut above. This indicates that the carver turned the bone before the left bow was cut. It also seems likely that the branches of **a** and **t** in **þat** were cut with the opposite orientation from **rap**. Evidence from the carving process thus supports the interpretation suggested here. Nor is the inscription on Sl 89 the only one of its type.

Side b of Vg Fv1992;172 Skara has an inscription almost identical to the one under discussion. This too is found on a rib-bone. The bone was discovered during excavations in the area “Rådhuset 30” in Skara and has been provisionally dated to some time before 1250 (Gustavson, Snædal and Åhlén 1992, 170, 172 f.). I have not had the opportunity of examining the object myself, but I reproduce the photograph on which my reading of side b is based in figure 2. Side a carries the inscription —**??stu·niþir·ok·rap·runur·si**, strongly reminiscent of B584 from Bryggen, Bergen, with its text: *Sezt niðr ok ráð rúnar, rís upp ok fíð við!* ‘Sit down and interpret runes, stand up and fart!’. Based on the wording of the Bryggen inscription it is tempting to take the —**??stu** with which the Skara text commences as *Seztu ...* ‘Sit down ...’. The bone is damaged at the beginning, but Marit Åhlén thought to see the remains of a branch or bow, as of **b** or **þ**, at the top of the second

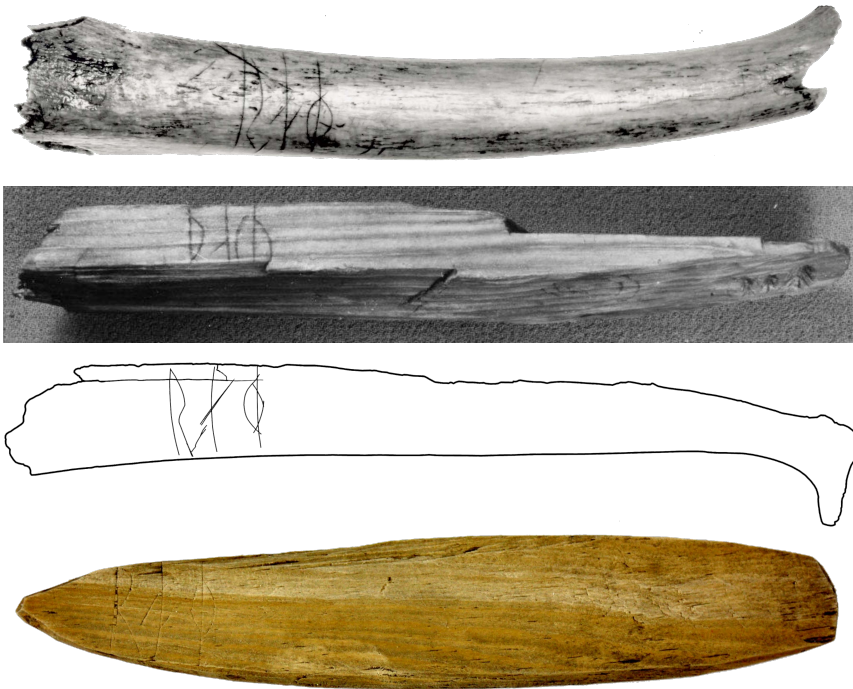


Fig. 2. Four *ráð þat* inscriptions. *Above*, Vg Fv1992;172 Skara (photo: Bengt A. Lundberg; © Swedish National Heritage Board, Stockholm); *upper middle*, A200 Gamlebyen, Oslo (drawing by the author); *lower middle*, B323 Bryggen, Bergen (photo: Aslak Liestøl; © Museum of Cultural History, University of Oslo); *below*, B235 Bryggen, Bergen (photo: Kristel Zilmer).

unreadable rune, which, if she is right, could indicate a spelling **sipstu** for *Seztu* (Magnus Källström, Riksantikvarieämbetet, pers. com.). Side b exhibits several diagonal cuts which appear to have arisen by chance. The side b inscription itself differs from Sl 89 only in one respect: the branch of **t** in **þat** is placed at the base of **r**'s tail rather than on its vertical. The reason is probably lack of space between the vertical and the tail. There is, however, little doubt the inscription is to be read **rap þat**. Just as in the Sigtuna example, we see that there is a distinct difference of height between the two “branches of **a**”; they do not appear to be parts of a single crossing branch.

On yet another bone, this time from Oslo, the three “bind-runes” appear once more. The bone has inscriptions on two sides and was found during excavations in the area “Søndre felt” in “Gamlebyen”, the Old Town (A200; Liestøl and Nestor 1987, 426 f., illustration p. 424). The find is dated to 1050–1150 (cf. Sand 2010, appendix A). I have examined the object on several

occasions. It is broken at both ends, but neither of the inscriptions has thereby been damaged. A piece of the edge recently broke off taking with it the uppermost parts of the two initial runes on side b, but the piece has now been stuck on again. On side a we have **fro**, interpreted by Aslak Liestøl and Svein Nestor as *fró*, a word meaning ‘alleviation’, ‘help’ and ‘relief’. On side b they read **rap**. In their view the left bow of **p** could have been added later (Liestøl and Nestor 1987, 427). The branch on the right of **r**’s tail elicits no comment even though it is recorded in Liestøl’s drawing (preserved in the Oslo Runic Archives). The branch traverses an area where the bone seems to have been damaged by a knife or had a piece cut away. The uneven surface may explain why the line was carved in sections and has a rather jagged appearance. I certainly see no reason not to regard it as an intentional incision. The branch of the second rune consists of a continuous line rising from left to right and crossing the vertical (as of † **æ**), but it has been incised twice, at a slightly different angle on each occasion. The left bow of **p** is more carefully executed than the right; the latter has been carved in two stages and is placed clumsily on the vertical. In spite of various uncertainties pertaining to the Oslo inscription, I find its similarity to the Sigtuna and Skara examples so striking that I am convinced *ráð þat!* is the correct interpretation. In Norway we would by and large expect one-sided **as** (†) at this period, but the continuous crossing branch perhaps illustrates a form of rationalisation, showing that this particular way of inscribing *ráð þat!* may have been so well known in certain circles that it developed a fixed form, rendering the marking of every distinctive feature unnecessary.

With some hesitation I would add two inscriptions from Bryggen (the Old Wharf) in Bergen to this group. The rune-like symbols which constitute inscription B323 occur on a narrow face of a roughly 17 cm long piece of wood, dated to c. 1250 (James E. Knirk, pers. com.) and of uncertain function. Ornamental carvings and apparently unintended scratches can be found on other faces of the same piece of wood. I have not had the opportunity of examining the object myself, and I base my observations on the photograph reproduced in figure 2, together with close-up images held by the Oslo Runic Archives. The lower parts of the runes were cut away when the wood was trimmed at a later period, and a little more was lost when a splinter came off the lower edge of the narrow face. In his discussion of Sl 89, Helmer Gustavson draws a parallel with this inscription, suggesting it can be read **rap** (Gustavson 2001, 32). Of the first character only the upper part remains; this is quite possibly **r**, but **b** cannot be excluded. No sign of any “upside-down” branch of **t** has been preserved. If the second rune once

had a branch marking the **a** of **þat**, it must have been placed very high up (in the direction of reading) in order for it not to collide with the bow of the double **þ**. The other inscription from Bryggen, B235, was brought to my attention by James Knirk. The object is a boat shaped piece of wood, 10.2 cm long, dated to c. 1185 (James Knirk, pers. com.). The inscription is carved on the “deck” of the boat, near the “bow”. Kristel Zilmer, Bergen, has kindly examined the inscription for me in Bryggen’s Museum. I base the following on her reading and photographs. The three rune-like symbols are almost identical to those of the Sigtuna and Skara inscriptions discussed above but there is no branch that would give the **t** in **þat**. A weak diagonal trace, barely visible on the photos, descending towards the left from the topmost part of the vertical of **a** in **þat**, could perhaps have been taken as the **t**-branch of a bind-rune **at**, but it proves to be a natural crack in the wood connected to damage on the edge of this side of the stick. Notwithstanding the comparative weakness of the evidence, I would suggest that these two inscriptions can also be interpreted *ráð þat!*

Even though complex “bound” runes of this particular type have not up to now been documented, there do exist inscriptions, whose different parts have been written upside down and in opposite directions relative to one another. B3 from Bryggen, Bergen, occurs on the base of a wooden bowl that has been turned in a lathe. The inscription is made up of single runes, bind-runes and crosses. In 1956 Aslak Liestøl interpreted it as an abbreviated *Av[e] Ma[ria]* (in a letter to Asbjørn Herteig dated 6 January 1956, preserved in the Oslo Runic Archives). Liestøl’s reading and interpretation have since been confirmed and expanded by James Knirk (*NIyR*, 6: 235 f.). You begin in the middle, proceeding from left to right: †**au**† (in the inscription itself the second cross is upside-down in relation to the first); you then turn the object through 180° and start in the middle once again, still reading left to right: †**ma**. In this case it is not necessarily a desire to make the inscription difficult to read that has determined the form; more likely, perhaps, the carver wanted to create a compact monogram.

Both B3 and the five inscriptions that have been the subject of this article show that rune-carvers were not averse to the idea of turning the object on which they were writing through 180° and continuing upside-down in relation to the beginning of their text. Juggling the writing around in this way was clearly often done with playful intent, but it might have a serious purpose too. When we consider that there exist a good many meaningful inscriptions with rune-like symbols that resemble those discussed here, it clearly behoves us, in the light of the foregoing, to look at them from every possible angle before dismissing them as uninterpretable.

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Runes about a Snow-White Woman: The Lund Gaming-Piece Revisited

Rikke Steenholt Olesen

Abstract

A small, well-preserved, wooden runic object was found in a well in the city of Lund in Scania (Skåne) in 2004 and has puzzled researchers ever since. It is presumably a gaming-piece for a board game. The dating of the archaeological layer in which the object was found suggests that it ended in the well between c. 1220 and 1235. The reading of the individual runes is in almost every case certain. The reading order of the lines, the interpretation of the linguistic content and the provenance, however, have caused disagreement among those who have studied the object. The inscription was tentatively discussed in the author's Ph.D. dissertation from 2007, but many questions remained unsolved. This paper reviews the discussion so far, and offers a more coherent linguistic interpretation. It also suggests a probable provenance for the object.

Keywords: medieval runic inscription, Lund, gaming-piece, order of reading, rune-carver formula, love, love sorrow

The runic find

A circular piece of beechwood ornamented and inscribed with runes came to light in the spring of 2004 in the city of Lund. It was found in the course of an archaeological excavation in the residential block known as “kv. Blekhagen 10–12”, in the filling of a well at a depth of about two metres. The object itself has not been dated, but technical analyses have cast light on the age of the well and the layer of filling. The woodwork in the well consisted of beech and reused oak. The beech was felled in the years between 1202 and 1214 (Swedish dendrochronology nos. 69316–47, household/phase 11). The filling has been dated by archaeological methods of stratigraphy to the period c. 1220–35 (household/phase 12; Ericsson et al. 2013). This means that

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the runic object ended up in the well sometime between c. 1220 and 1235, and the dating of the filling provides a *terminus ante quem* for the ornament and the runic inscription of c. 1235. Since there are no significant traces of wear and tear on the surface of the piece, the ornament and the runes were probably incised in the 1220–35 period. The object is now kept at Kulturen (Museum of Cultural History) in Lund where it is registered with museum no. KM 86581:1624.

In form, the object resembles an ice-hockey puck in miniature (it is about 5.1–5.2 cm. in diameter and 1.2–1.4 cm. in thickness), and that it is why it has been facetiously referred to as “the rune-puck” by the Swedish archaeological team involved in the excavation. Circular objects of this type are referred to as discs by archaeologists and are frequently found when excavating medieval towns. Discs are often interpreted as gaming-pieces for different types of board game, and gaming-pieces from medieval Lund have recently been discussed as a part of a master’s thesis (Spjuth 2012), which includes individual detailed find-lists of discs of horn/bone, stone and wood. The presumed gaming-piece from “kv. Blekhagen” is, however, not included in the list of wooden discs. It is not always possible to determine with certainty whether discs should be interpreted as gaming-pieces or as other types of objects, e.g. amulets or spinning whorls, but there are several similarities between the runic disc and other disc finds from Lund classified as gaming-pieces, and therefore the beech piece is here and below referred to as a gaming-piece.

Ornamentation on discs from Lund consists typically of concentric circles (particularly common on lathed horn discs) or simple patterns of incised lines or geometric shapes, though some are more exquisitely decorated (cf. illustrations of discs from Lund in Persson 1976, 380 f., and Spjuth 2012, 31). The ornamentation of the gaming-pieces is mentioned, but not explicitly discussed, in the recent thesis by Spjuth. Notable is perhaps the fact that the majority of wooden discs from Lund are not ornamented (Spjuth 2012, 65 f.). The runic gaming-piece has an almost centred simple cross-shape incised on the one flat face. The cross arms are narrow at the centre and broader at the perimeter. This particular type of cross is generally known as a cross pattée. It is very common in (Christian) medieval art all over Europe and also frequently depicted on Danish late Viking Age/medieval runestones. The runic piece is comparable to another ornamented wooden disc from Lund also interpreted as a gaming-piece. That disc dates to the first half of the 1100s and is incised on both flat faces (KM 66166:1326; Persson 1976, 381 [photo]). On the one face are two or more anthropomorphic shapes and on the opposite face, a centred cross-shaped ornament. One of the anthropo-

morphic shapes seems to have a halo. The depictions can be interpreted as religious markers, though secular shapes were the preferred explanation in Persson 1976.

The runic inscription on the Lund piece is spread over five rows separated by framing lines. It is not customary for discs interpreted as gaming-pieces to bear runic inscriptions. Examples do, however, exist, e.g. a Nordic gaming-piece (lathed horn) from the early medieval period found in Kałdus in Poland with the inscription **jonatafl**, *Jón á tafl* 'Jón owns the gaming-piece/the board game' (Lerche Nielsen 2004; here and below texts are normalized as Old West Nordic unless otherwise stated). In this case the runic inscription explicitly confirms the function of the disc as a gaming-piece. Other runic discs interpreted as gaming-pieces come from Norway and seem to be inscribed with personal names. Three of them were found in the Old Town (Gamlebyen) in Oslo and date from the 1100s or 1200s. On one of wood the text reads **sigrit:htḡ**, the first sequence of which has been tentatively interpreted as the feminine name *Sigríðr* (A 319; Knirk 1991, 16f.). A second likely gaming-piece, also of wood, bears the runes **sihurḡ**, probably the masculine name *Sigurðr* (A 263; Steenholt Olesen 2007, 140, with references), and on the third piece, of antler, a circle and the runes **arni**, the man's name *Árni*, are incised (A 300; Knirk 1989, 6). In addition, a disc from Bergen made of whalebone bears the inscription **uikigr** (followed by two verticals) interpreted as the man's name *Víkingr* (N 288; *NIyR*, 4: 46f.). The damaged runes on a fragment of a wooden disc from Tønsberg, not necessarily a gaming-piece, might spell the masculine name *Lóðurr* (A 50; Gosling 1989, 175–77). In addition, a chess-piece finely cut out of walrus ivory found in Helmond, Noord-Brabant in the Netherlands, and possibly Scandinavian workmanship, is also inscribed with runes (Stoklund 1987, 194f.), but since the chess-piece belongs to a different category of gaming-pieces it is left out of this discussion.

On account of the unique status of the Lund find it was with joy and excited anticipation that in the spring of 2004 Marie Stoklund, curator and senior researcher at the National Museum in Copenhagen, along with three other Danish runologists, accepted an invitation to attempt to interpret its inscription. The first examination was undertaken in April 2004 before the piece was conserved by freeze-drying, by Michael Lerche Nielsen, associate professor at the University of Copenhagen, and two doctoral students, Lisbeth M. Imer and the present author. The second examination took place in June 2005 ahead of a seminar held at the Institute for Language and Folklore, Department of Dialectology and Onomastics in Lund, where the piece was described and discussed by the archaeologist Conny Johansson Hervén and

Michael Lerche Nielsen. The examination was this time undertaken by Marie Stoklund, Michael Lerche Nielsen and the present author. The inscription was never formally published but a linguistic discussion is to be found in Steenholt Olesen 2007, 138–49.

It is natural to consider a runic find from medieval Lund as East Nordic. It is also fairly natural to expect a reading of a runic inscription like the one on the Lund gaming-piece to proceed from the top and downwards. The top and downwards order was the one followed in Steenholt Olesen 2007. An order of reading running from the bottom and upwards was, however, discussed at the seminar in Lund. In the summer of 2007 the piece was examined once again, this time by Professor James E. Knirk of the Oslo Runic Archives, and the question as to the order in which the lines should be read was raised again with reference to among other matters the name-riddle in the old church in Bø, Telemark (A 104; Knirk 1986, 76–80, and Louis-Jensen 1994), where the text ran from the bottom and upwards. (Further argumentation in favour of this order is found below.)

Knirk has also argued in favour of a Norwegian provenance for the runes on the Lund piece. Since the inscription both linguistically and content-wise betrays elements that could suggest a West Nordic tradition, Professor emerita Jonna Louis-Jensen (Copenhagen) has been consulted about possible literary parallels. The following exposition of the inscription builds on earlier works and discussions and not least on the tentative interpretation of the inscription that Louis-Jensen put forward in e-mail correspondence with Knirk in 2007.

The runic text

Transliteration

The reading here starts at the bottom line, and the lines are numbered upwards as shown in figure 1. The inscription uses short-twig forms of **a**, **t**, **n**, **s** and **o** (𐌰 𐌱 𐌲 𐌳 𐌴), which is typical for the medieval period. Short-twig *s*-runes occur relatively rarely in the area where Old Danish was spoken but examples are found, so that the shape of the *s*-rune is not a sufficient criterion for declaring the inscription as non-Danish (different shapes of the *s*-rune are discussed in *DR, Text*, cols. 972–74). From Lund itself there is a fragment of a comb bearing the inscription: —**lui:reist:runar:pesar:at:k**—, where the short-twig form occurs in both **reist** (with a digraphic spelling unusual for Old Danish) and **pesar**. There is a discrepancy between the dating of the comb-type to the late 900s or the 1000s and the runic inscription, which in a

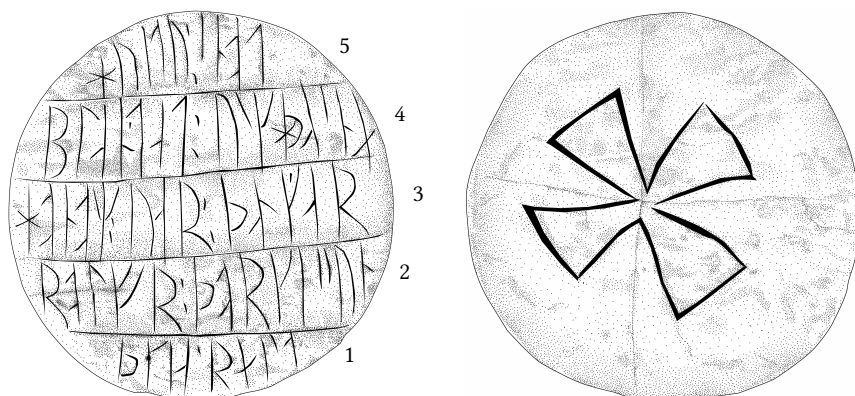


Fig. 1. The Lund gaming-piece, face with runes and face with ornament. 1:1. Drawings by architect Stine Bonde Bendixen.

Danish context gives the impression of being younger than the typology of the comb would imply (Stoklund 1998, 7f.). It could perhaps be argued that the provenance of the comb inscription is uncertain, and that the diphthong in **reist** combined with short-twig forms might indicate West Nordic origin.

On the Lund gaming-piece, the traditional runic orthographic principle of not writing the same rune twice in succession is disregarded in **porkissun** in line 2. However, the doubled s-rune would not be controversial (if controversial at all in a late medieval inscription) if the sequence were read as two separate words: **porkis** followed by **sun** (further discussed below). The *ansur*-rune, with twigs facing left (as normally in medieval inscriptions, \uparrow), is employed in the inscription with the sound value *o*, while \uparrow would seem to occur in complementary distribution with the short-twig *a*-rune (\uparrow) and is therefore to be understood as denoting *æ*. Use is made of a colon or two-point punctuation mark (short strokes made by pressing the knife-tip into the wood) whose purpose seems generally to be to divide up the text into words. The punctuation mark is, however, not employed consistently, for several sequences without separation marks must be assumed to consist of more than one word (the final sequence in lines 2 and 4, presumably also line 3). The inscription displays three dotted runes: an *e*-rune in line 1 (an *i*-rune with a strong point on the vertical just above the middle: \uparrow), a *g*-rune in line 3 (a *k*-rune with a short stroke in the space between the vertical and the branch: \uparrow), and a *d*-rune in line 4 (a dotted *t*-rune with a weakly cut short stroke in the space between the vertical and the twig: \uparrow). This last rune was first read as **d** by James Knirk in 2007. In line 3 the tenth character is certainly a bind-rune \overline{an} (hardly to be read with the unnatural order \overline{na}),

while the tenth character in line 4 is a certain *þ*-rune perhaps combined with an *h* though this is more probably a correction than a bind-rune. Although there is uncertainty on this point, the transliteration is given below as **þ[þ<h]**. Rune-typologically the occurrence of short-twig forms in a Danish inscription would point to some time after the 1000s. Late medieval features such as twigs carried all the way down to the framing line are absent. On a rune-typological basis alone the dating would seem to be to the 1100s or the 1200s and this fits well with the dating suggested by the archaeologists.

In addition to the order of the text, the following transliteration differs in a few respects from that found in Steenholt Olesen 2007, 143:

- (1) **þeta:ræist**
- (2) **rolfr:þorkissun**
- (3) **honom:uar:þāngar**
- (4) **blandat:ump[þ<h]asn**
- (5) **huitu:snót**

The rune-carver formula

The name of the rune-carver, **rolfr**, *Hrólfr*, is of common occurrence. It is a contracted form of an originally dithematic Common Scandinavian *Hrōðulfr* (Peterson 2007, 122). Its form suggests that the author of the text can hardly have been an Icelander, since *h* before *r* survives in Icelandic. In a Danish/Scanian context the survival of the nominative ending *-r*, perhaps only sporadically, would point to a dating at the latest in the “Older Middle Danish” period, i.e. 1100–1350 (cf. GG, 1: 9). However, a svarabhakti vowel before the *-r* would be expected from around 1200 (see the discussion of **āngar** below).

The given name, *Hrólfr*, is followed by the sequence **þorkissun**. This can be explained as a masculine name in the genitive case followed by the noun *sonr* ‘son’ (with the nominative case unmarked, see below) and understood either as a compound patronymic designation or as an example of an actual name (primary compound patronymic). Unfortunately the use of punctuation marks in runic inscriptions is not consistent and can therefore not give any indication as to which of the alternative understandings is to be preferred. Therefore, although the spelling of similar collocations as one word or two in manuscripts can reflect both regional and chronological differences in the Nordic countries (cf. Kousgård Sørensen 1984, 83f.), a runic spelling **þorkissun**, without punctuation marks, does not possess similar strength as evidence of provenance.

The spelling of the noun *sonr* is **sun**, as would be expected in a high medieval Danish/East Nordic context, cf. the compound designation **þorgils:sun** on a medieval grave-slab from Galtrup (DR 152). The situation, however, is perhaps different in Norway, in that it has been argued that the use of a *u*-rune for *o*-sounds in runic inscriptions from Bergen and Trondheim can reflect Icelandic language (Hagland 1989, 92–94). Runic spelling of the word *sonr* (in the nominative case) varies, however, both as an individual word and as the second element in compound patronymic designations in Norwegian inscriptions, and the variation reflects both dialectal and chronological differences. The element **sun** has no visibly marked nominative form (i.e. no final **-r**), a well-known and widely discussed phenomenon (Peterson 1993, 164). A runic spelling **sun** in **þorkissun** would, in my view, not reflect any inconsistency with the medieval Norwegian runic corpus, supported as it is by several **sun** spellings in compound patronymic designations. For example, the historically known Norwegian chieftain Sigurðr Jarlsson carved his name as **sigurþr:ialssun** in the famous inscription no. 1 from Vinje stave church (N 170), dated to the 1190s (N 170; *NlyR*, 264–68).

The first element, written **þorkis**, must reflect a Nordic, dithematic name with the first element *Þór-*, i.e. the god's name 'Thor', whereas the runes **-kis** must reflect a genitive singular form. Formally the name element could be Old Danish *-gēr*, identical with the noun meaning 'spear'. Names in ODan. *-gēr* belong to the masculine *a*-stems, and the genitive singular should be *-gērs* (with an *r*). The long *ē*, which was developed from the Common Scandinavian diphthong *æi*, was shortened at an early date to *i* in weakly stressed position so that the nominative form came to coincide with the masculine *ija*-stem's nominative form in *-ir*. This led to a transition to the declensional pattern of the *ija*-stems and hence a genitive singular form in *-is* (cf. *GG*, 1: 244 f.). A late Viking Age runic instance of this development is the spelling **askis**, genitive singular of Old Danish *Asgēr* on the runestone from Grensten (DR 91).

The element **-kis** may formally also represent a way of writing the genitive singular of the element *-gísl* or of a form with metathesis *-gils* (concerning metathesis in this name, see Hagland 1990). A runic inscription on a stick from Bryggen in Bergen contains the sequence **þorkis**, most likely the nominative singular form of a man's name. The runes are interpreted as a form of the masculine name *Þorgísl* (cf. the Scandinavian Runic Text Database, signum N B307; cf. Seim 1998, 219). This interpretation appears to presume that the carver simply forgot an *l*-rune, and this would also have to be the explanation of a runic spelling **-kis** of a form with metathesis *-gils*. A genitive form of *-gísl*, i.e. *-gísls*, written **-kis** can be explained linguistically

as a loss of *-l-* between two consonants (the three-consonant rule, see Seip 1955, 164; GG, 2: 301f.). A name-form *Thorgiss* (dated between 1396 and 1439, *Mathias Thorgiss*) recorded in *Danmarks gamle Personnavne* under the headword *Thorger* but referred to either *Thorger* or *Thorgisl* (DgP, 1: 1384), along with several other similar name-forms from the 1400s, corresponds to a possible primary patronymic **porkissun**. On a formal, linguistic basis it is not possible to make a definite decision as to which name lies behind the runic spelling on the Lund gaming-piece.

The demonstrative pronoun written **þeta** must represent the neuter accusative singular *þetta* ‘this, that’ and be understood as the direct object of **ræist**, the 3rd person singular preterite of ODan. *rīsta* ‘carve’, where the spelling with a diphthong is a typologically archaic feature in a Danish context. Gradually, as more and more examples of what have been explained as digraphic spellings without a phonetic basis in East Nordic have appeared in the area where Old Danish was spoken, the perception of this feature as unambiguously foreign has had to be modified (Lerche Nielsen 2001). The dating of most of the inscriptions with digraphic spelling of the historical diphthong *æi* indicates that such spellings belong typologically to the oldest strata of medieval inscriptions, and that does not fit very well with a dating to the 1200s for the Lund piece. The only other example from the area where Old Danish was spoken of the runic spelling **ræist** is found in Tornby Church, North Jutland (DR 169; *NlyR*, 5: 234f.). That inscription has several features which suggest that the provenance is Norwegian — among others the distribution of *s*-runes for *s* and *z* (¹ and ^h respectively), and diphthongs manifested in the spellings **þorstæin** and the form **ræist** itself. This inscription is probably from the 1200s. Thus, in conclusion, while lines 1–2 of the Lund gaming-piece clearly contain a rune-carver formula with a man’s name, his national origin is as yet unclear.

The sentence structure of the rune-carver formula on the Lund gaming-piece can play a role in establishing the order in which the lines are to be read, which, as mentioned above, has been the focus of the discussion of the inscription. The most frequent type of rune-carver formula in the Middle Ages has a subject-verb-object structure (SVO, with *X* here representing a name), *X reist rúnar þessar*, while in a few examples the formula shows inversion and verb-subject-object structure (VSO), *Reist X rúnar þessar*. The object-verb-subject order (OVS), *Þessar rúnar reist X*, is also recorded, and several instances are found in Norwegian runic inscriptions from Bryggen in Bergen. One of the finds (B 572), a triangular stone, displays two initial OVS rune-carver formulas corresponding precisely to the one on the Lund piece: **þeta:ræist:æirrikr:baki:um:not f**, *Þetta reist Eiríkr*

..., literally ‘This carved Eiríkr ...’, and **þetaræistârne** ..., *Þetta reist Árni* ... ‘This carved Árni ...’, while another Bergen inscription (B 417) reads: **þettaræistblindermapertilþinhy—**, *Þetta reist blindr maðr til þin* ... ‘This carved (a) blind man for you ...’. There is thus nothing aberrant about an initial rune-carver formula with an OVS structure, which is the order in the inscription on the Lund gaming-piece when it is read from the bottom and upwards. Unfortunately both inscriptions from Bryggen seem to be fragmentary. Still the OVS structure in all three instances appears to serve as a prelude to a following text. This might also be the case in the inscription from Lund. It is certainly very difficult to find parallels in medieval runic writing to the SOV word-order (reading from the top and downwards, as done previously) **rolfr:þorkissun / þeta:ræist**, although if the writer intended some kind of verse, he might conceivably have opted for an unusual sentence structure.

The lyrical statement in lines 3–5

The most significant linguistic argument in favour of an order of reading from the bottom and upwards, however, is the possibility of linking the end of line 4 with the beginning of line 5. The sequence **snæ/huitu** can then be read as a compound adjective, *snæhvitr* ‘snow-white’, most likely weakly declined in some oblique case. The adjective would then modify the following **snot**. It is first necessary to examine the runic spelling **snæ**. Although an Old Danish form **snæ* is not recorded, it can be postulated on the basis of parallels (e.g. the alternative form *sæ* of ODan. *sīo* ‘lake’; GG, 1: 237 n. 2). Thus the runic spelling **snæ** does not constitute an argument for non-Danish provenance. Nevertheless it is worth noting that the form does not need any special explanation in a Norwegian context.

The last word in line 5, **snot**, is in all probability identical with the well-known poetic word *snót* (f.) ‘woman’ (Sveinbjörn Egilsson 1913–16, 523), also attested in Norwegian dialects, but not recorded in East Nordic sources. The word appears in two medieval runic inscriptions from Bryggen in Bergen. The first (B 111) is in verse (the metre being *dróttkvætt*) but only fragmentarily preserved, and seems to refer to a situation in which a woman has in some way or other yielded to a man or lover (agreed to marriage or love-making?), but is still ‘by men assumed to be a maiden’ or maybe ‘by men is considered to be unmarried/a virgin’; cf. Liestøl 1964, 32 f.). The second inscription (B 404) contains the wording **snot·uliota**, *snót úljóta* ‘unugly woman’, and this statement can be connected with that on a third Bryggen stick (B 524): **konouena**, *konu væna* ‘beautiful woman’, since the

accompanying introductions on the sticks (**sisi-si-sissi** and **sesesæssese** respectively) must have their roots in the same formula. The repetitive formula is also known from the Greenlandic Narssaq stick, which might be 150–200 years older than the two examples from Bryggen (cf. also MacLeod and Mees 2006, 68–70, with references). The word *snót* occurs in manuscript sources as a synonym of *kona* ‘wife, married woman’ and *mær* ‘maiden, unmarried woman’. The term *mær* is employed on the Narssaq stick, and this word occurs in other places modified by the adjective ‘white’, e.g. *hvít mær, en bráhvíta mær* (‘eyebrow-white’), *en línhvíta mær* (‘linen-white’; these examples are taken from Sveinbjörn Egilsson 1913–16, 416). The poem *Sólarljóð*, in Eddic metre, contains the formula (*þá*) *hvítu mær* ‘(the) white maiden’, and there is no doubt that the adjective ‘white’ combined with a word for a woman both in *Sólarljóð* and in other poetic contexts has the positive connotations of fair and beautiful (cf. Sveinbjörn Egilsson 1913–16, 302).

The runes **um** in line 4 must be either the adverb or the preposition *um* (+ accusative), and **þa** is probably the feminine accusative singular form *þá* ‘this, that’ of the demonstrative pronoun, although formally the possibilities are several, e.g. the adverb *þá* ‘then’, the preterite of the verb *þiggja* ‘receive, accept’ and a form of the noun *þá* (f.) ‘thawed ground’. It is, however, natural to analyse **þasnæ/huitu:snót** as an alliterating, semantically and grammatically concordant unit in the accusative feminine singular: *þá snæhvítu snót* ‘the snow-white woman’. This is probably governed by the preposition **um** and forms the last part of the inscription (lines 3–5).

Line 3 begins with **honom:uar** and it seems reasonable to interpret the first word as the dative of the personal pronoun *hann* ‘he’. The expected Danish runic form around 1200 would be **hanum** with umlaut of the vowel of the first syllable unmarked, cf. that the oldest Scanian manuscripts have *hanum*, *honum* only appearing in the late 1300s. Presuming the runic form **honom** to reflect marked *u*-umlaut in the first syllable corresponding to the manuscript form *honum*, the **o** in the second syllable could be explained as an example of vowel harmony *u > o* under the influence of the *o* in the preceding syllable. If, however, the first syllable is regarded as long, the lowering *u > o* could be explained as an example of vowel balance. But whereas vowel harmony can be demonstrated in Scanian manuscripts from the early 1200s (GG, 1: 402), vowel balance is not definitely attested in the area where Old Danish was spoken, although it is assumed that it could have existed in Scanian in the 1100s and 1200s (GG, 1: 403; Bjerrum 1973 [1952], 121). The vocalic systems underlying the designation of vowels in runic inscriptions from Scania and Bornholm during the period 1000–1250

cannot be established with certainty, perhaps due to irregular variation, combinations of vowel harmony and vowel balance or other rules (Bjerrum 1973 [1952], 58). In a Danish/Scanian context the appearance of the form **honom** as early as c. 1200 must be considered somewhat aberrant. In Old Norwegian manuscripts, however, both phenomena, vowel harmony and vowel balance, occur widely (Seip 1955, 128–32) and runic inscriptions from Bø, Atrå and Bryggen show examples of the spelling **honom** (A 98, N 148, B 181; all from the late 1100s or early 1200s). The runes **uar** are probably the 3rd person singular preterite of the verb *vera* ‘to be’, but it is not obvious what the subject of **uar** is.

The runes following **uar** in line 3 have caused a good deal of trouble and not yet received a satisfactory interpretation. The sequence **þāngar** can hardly represent a known word, but if one accepts Jonna Louis-Jensen’s proposal (in the previously mentioned e-mail correspondence with James Knirk in 2007) and reads the *a*-rune twice in the bind-rune **ān**, two individual units, **þa** and **āngar**, can be separated. The double reading of **a** is admissible according to classical runic orthography, in which the same rune is not normally written twice in succession. The motivation for not carving two adjacent *a*-runes in **þāngar** cannot be lack of space since there is room enough for another *a*-rune and a punctuation mark. With double reading of the *a*-rune the first unit would constitute a word, *þá*, for which formally the possibilities of interpretation are again many. The remaining runes, **āngar**, can be explained as a form of the noun *angr* (m./n.) ‘sorrow’ written with a svarabhakti vowel. This would provide a subject for the verb **uar**, and **þa** would then probably be best interpreted as the adverb of time *þá* ‘then’. Svarabhakti vowels are documented early in both East and West Nordic texts. In Norway svarabhakti *a* occurs frequently in texts from the southern and south-eastern parts of the country, while Icelandic texts have *u* (Seip 1955, 137 f.). In Denmark, svarabhakti vowels between a consonant and final *-r* develop over the whole country in the medieval period. In manuscripts the vowel is usually *æ*, but *a* is also employed in accordance with the system of vowel harmony (GG, 1: 424 f.). The translation of the middle line under this interpretation would be ‘For him was then sorrow’. It is likely that **honom** refers to the carver, Hrólfr.

The word **blandat** in line 4 is probably a typologically late, weakly conjugated past participle of *blanda* ‘to mix’. The form *blandat* occurs in thirteenth-century manuscripts containing early poetry (e.g. *Hávamál*), but is explained as a later form introduced by scribes (Sveinbjörn Egilsson 1913–16, 50 f.). The verb *blanda* has a long string of meanings, both concrete and figurative. The basic meaning is ‘blend, mix’, typically used of liquids,

e.g. blood, poison and mead. According to Finnur Jónsson's dictionary of the Icelandic *rimur* the verb *blanda* is used in that genre particularly when describing the production of the poets' drink, i.e. the mead of poetry (Old West Nordic *skáldskapar mjoðr*), and the noun *bland* (n.) occurs similarly in kennings for the drink itself. In addition, the word appears in expressions such as *með blandinn ekka* 'with sorrow-mixed mind' (Finnur Jónsson 1926–28, 29) and in constructions involving the mediopassive form *blandask* (*við* or *með*), which can mean 'to mix/involve oneself in something' and may even denote sexual intercourse. The past participle can indicate opacity, both in concrete terms (e.g. 'cloudy [liquids]') and figuratively as an expression for being unreliable (see further in *ONP*, 2: cols. 411–15). Such connotations are for instance reflected in *Lokasenna* stanza 32 where Loki accuses Freyja of being a sorceress much 'mixed' with evil: *Þegi þú Freyia, þú ert fordæða, oc meini blandin miðk* (von See et al. 1997, 447–49). Zoe Borovsky (2001) has dealt exhaustively with the meaning of the adjective *blandinn* when used in insults of this kind directed at women. In such contexts *blandinn* is chiefly employed in accusations of unreliability. Borovsky feels it possible to associate the use of these accusations of 'being mixed' with meta-narratives concerning the theme of imbalance between the masculine and the feminine elements in the Old West Nordic universe (Borovsky 2001, 10 f.). In theory this could also be the theme of the inscription on the Lund gaming-piece.

Lines 3–5 of the Lund piece can thus be interpreted: *honum var þá ang(a)r blandat um þá snæhvítu snót*. It is difficult to find precise parallels to an expression *blanda angr um* but it may be compared with expressions such as *bera angr um fljóð* 'to bear sorrow about a woman' (Finnur Jónsson 1926–28, 5), *baka einhverjum sorg* 'prepare sorrow for somebody' (Sigfús Blöndal 1920–24, [1:] 57), cf. *bland iðranar* 'blend/mixture of repentance, i.e. repentance' (Sveinbjörn Egilsson 1913–16, 50), and perhaps also but less obviously *brugga svík* 'to brew/contrive fraud' (Fritzner, 1: 198). Against this background the entire inscription on the Lund gaming-piece can tentatively be interpreted as follows: *Þetta reist Hrólfr Þorgeirs/Þorgísls sun; honum var þá angr blandat um þá snæhvítu snót*, 'Hrólfr, Þorgeirr's/Þorgísl's son, carved this. For him sorrow was then caused concerning the snow-white woman'. Within this text the word *þá* in *honum var þá angr*, established above by double reading of an **α** in line 3, is somewhat superfluous and does not read well so close to the *þá* in *þá snæhvítu snót* (lines 4–5). The vocabulary in the inscription is steaming with emotion and lyrical expression, and the statement could perhaps be considered poetic. However, apart from the carver-formula, which could be an imperfect *fornyrðislag* couplet or a likewise imperfect first two short-lines of a *ljóðaháttir* stanza with *reist*

and (*H*)rólfr alliterating, and lines 4–5 *um þá snæhvítu snót*, which would be an acceptable third or full line of a *ljóðahátt* stanza with alliteration between *snæhvítu* and *snót*, evidence for metrical structure is hard to find. The inscription could nonetheless be characterized as an irregular or imperfect stanza in a mixed Eddic metre. The imperfections may stem from its being a garbled rendering of a quotation, or perhaps even a deliberate re-writing of such. The inscription exemplifies a genre, poetry, that is meagrely represented in Old Danish. Among the runic evidence from Lund we find, for example, the inscription on the rib-bone designated as Lund bone no. 4, from the residential block known as “kv. Glámbeck 5” (DR Til5): **bondi × ris × ti × mal × runu/arar × ara × æru × fiaþrar ×**, *Bóndi risti málrúnu; árar ara eru fjaðrar* ‘Bóndi carved speech-rune(s); (the) oars of (the) eagle are (its) feathers’. (The final *r*-rune in **arar** and **fiaþrar** is dotted, probably in an attempt to represent **ᚱ**.) This cannot be classified as poetry but has a proverbial character that illustrates the carver’s acquaintance with learning. Similarly the carver Hrólfr was seemingly not a poet, but had intellectual skills over and above his ability to write in runes.

The inscription on the Lund disc does not explicitly confirm the object as a gaming-piece as does the one on the Kaldus find, but there is nothing in it that conflicts with such an interpretation either. It is hardly likely that the wooden disc simply functioned as an inscription bearer since it is carefully formed and decorated. If its function was not that of a gaming-piece, it may have been an amulet, since such a small object would have been easy to carry around.

Provenance

With Lund as its find place and an archaeological dating to the early 1200s, it is natural to assume that the language of the inscription on the gaming-piece is Old Danish/Scanian. It is questionable whether it is worth trying to determine the provenance of medieval runic inscriptions from the area where Old Danish was spoken on the basis of the traditional parameters so long as these inscriptions display such great variation and have as their background early urban environments that probably housed people from many different places of origin. Linguistically the inscription seems in several respects to harmonise with the Scanian of the 1200s, although some features appear to contradict this. For instance, it is possible to argue that the word *snót* is documented for the first time in East Nordic in this inscription and explain the lack of other occurrences as the result of the different source traditions in East and West Nordic.

The question of provenance is, however, important. Taking runic typology, linguistic forms, vocabulary and genre into account it is much easier to explain the inscription as being in Old Norwegian. On the principle of always giving the greatest weight to the most straightforward explanation in linguistic analysis, it seems most reasonable to conclude that the Lund gaming-piece is of Norwegian origin.

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N 244 Helland II: A Topsy-turvy Runestone

James E. Knirk

In 1951 and again in 1952, as part of his work for the Runic Archives at the Collection of Antiquities (Oldsaksamlingen), University of Oslo, Aslak Liestøl examined and photographed the runestone at Helland “midtre”, the middle Helland farm, in Sola township, Rogaland county. His notes and photographs were used and reproduced by Magnus Olsen in his 1954 presentation of the inscription as N 244 Helland II in the Norwegian corpus edition, *Norges innskrifter med de yngre runer* (*NIyR*, 3: 212–17). The monument is striking, leaning somewhat and with a right-angled flat top (see fig. 1). The runes are in places difficult to read, but the inscription is interpreted in *Norges innskrifter* as follows: “Skarði erected this stone in memory of Bjalfi(?), his son, a very good man.” Strangely enough, the runes run in two rows from the top of the stone downward, contrary to the normal pattern for such Viking Age runestone inscriptions in Norway, i.e., from the base upward.

When first documented, the runestone was lying in the farmyard, where it was portrayed by an anonymous draughtsman working on behalf of the Stavanger bishop Thomas Wegner. The bishop included it in a manuscript prepared in 1639 which he sent to Ole Worm in Copenhagen (now AM 368 fol., on 25r), who published the drawing in his *Monumenta Danica* (1643, 509). The drawing shows the stone as if erect with two lines of runes, each starting at a horizontal line near the bottom and within framing lines; at the top, the rune-bands, which are here empty, bend and connect, describing a rounded end (see *NIyR*, 3: 213, where the drawing was printed horizontally, as it also was in Moltke 1956–58, 1: 214). In 1745 the stone was still lying in the farmyard according to the county prefect Bendix Christian de Fine (1952, 112), who drew two lines of runes without any outline of the stone itself; he relates that it had previously stood on a mound not far from the

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Fig. 1. Photograph of N 244 Helland II taken in 1976. © Museum of Archaeology, University of Stavanger.

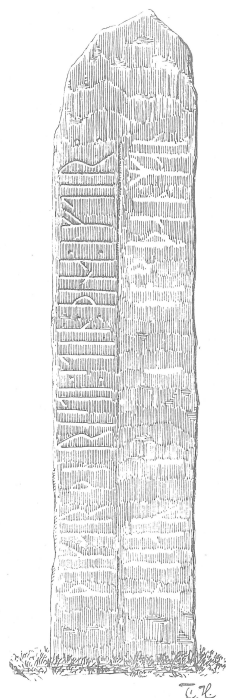


Fig. 2. Drawing of Helland II in 1902 by Tor Helliesen (1903, 68)

farmhouses. It was probably still prone when Gerhard Munthe drew it in 1828, but that cannot currently be determined since the University Library in Bergen has been unable to locate this report among Munthe's materials in their manuscript collection. In his published *Norske fornlevninger*, Nicolay Nicolaysen (1862–66, 309) records that the stone had for many years stood at the entrance to a turf shed beside one of the buildings on the farm. As early as 1863 Oluf Rygh noted that it had been taken out of the shed and laid down on the ground, and that an old man related that he had struck off a half ell (c. 30 cm) from each end in order to get it to fit into the shed. From this Magnus Olsen (*NlyR*, 3: 214) assumed that the stone had been part of the wall rather than simply standing near the entrance to the shed. The stone was apparently raised somewhere on the farm, probably in the farmyard, by the time Rygh examined it again in 1866, since he notes its height “above the ground” (“over Jorden”; cf. Rygh 1888, 261, where he says it was standing). However, the museum curator in Stavanger, Tor Helliesen (1903, 67–69),

found the stone once more lying in the farmyard in 1902. On the remains of a circular mound 12.5 m in diameter, which he had identified during his inspection, Helliesen discovered two immovable stones, the distance between which corresponded to the thickness of the runestone, thus—he surmised—indicating that they were the structural base for the monument. He thereupon had the runestone erected in its assumed original foundation at its probable original site, and that is where it stood when Liestøl made his field trips in the early 1950s.

The reported measurements of the stone, especially its length, have varied. Length according to Wegner’s manuscript in 1639: 5 ells (about 315 cm), de Fine in 1745: 4 ells (about 252 cm), Rygh in 1863: 3¼ ells (about 205 cm), Rygh in 1866: 2 ells 21 in. (about 180 cm) but he specifies “above the ground”, Sophus Bugge’s undated drawing: 81½ in. “above the ground” (about 205 cm, apparently combining Rygh’s measurement from 1863 with his comment “above the ground” from 1866), Helliesen in 1902: 210 cm. Liestøl gives the length as 2 m (200 cm), but he apparently did not see the entire stone (see below). The present full length, in 2012, is 217 cm. There is no good reason to assume any damage to the stone between 1639 and 1745, and it would seem most likely that the correct measurement was somewhere between the 5 ells in Wegner’s manuscript and de Fine’s 4 ells, both probably crude approximations. It would thus have been c. 285 cm long, and could have projected well over 2 m above the ground when standing. The only shortening of the stone about which anything is known, seemingly took place some time between the 1830s and the 1850s. The old man probably struck off at least 40 cm, perhaps as much as 60–70 cm, but from only one end of the stone, not both (see below). Measurements of the width have also varied: Wegner: 1½ ft. (c. 46 cm), de Fine: 1 ell (c. 63 cm), Rygh 1863: 16 in. (c. 41 cm), Bugge: 25 in. (c. 63 cm, i.e., de Fine’s measurement), Helliesen: 50 cm, Liestøl: 46 cm. The variation is probably due to attempts to simplify the measurements by using approximations in the unit-systems employed, perhaps also to measuring either average or greatest width. The maximum width in 2012 is 52 cm.

During a violent storm in the winter of 2012, possibly in February of that year, Helland II fell over, but was fortunately not damaged. When the present author and K. Jonas Nordby visited the site on 19 June that year, they were able to examine the entire stone and quickly realized that it had stood upside down during Liestøl’s autopsy in the 1950s. The real top narrows slightly to a very blunt point, somewhat as in the drawing from 1639, and most of the top surface is weathered as much or more than the rest of the stone. Obviously very little has been broken off there since the Viking

Age or Middle Ages. The 90-degree angle edge, on the other hand, exhibits a relatively freshly broken surface, not an old weathered one, in spite of the fact that it has formed the top of the monument and thus been the part most exposed to weathering for at least sixty and probably over eighty years (see below).

Tor Helliesen's own drawing of the runestone, published in 1903 and included by Magnus Olsen in the corpus edition's bibliography for the inscription, shows that the curator from Stavanger had indeed set the stone up correctly (see fig. 2). But it must have fallen sometime thereafter and been put up again topsy-turvy. There is a clearly quite old, anonymous photograph in the folder for Helland II in the Oslo Runic Archives showing the runestone with its base in the air, obviously taken long before Liestøl's visits. The picture is apparently recorded ("1 fotogr.") in the two-volume handwritten list of the contents of the folders in the Runic Archives originally compiled in the 1920s.

Thus the stone fell less than twenty-five years after Helliesen had put it up, but was raised again, upside down, and remained that way until it fell at least eighty years later, in 2012. On Liestøl's photographs in the Runic Archives from the early 1950s one can just about see that the "base" of the stone begins to taper off on the right side near the ground (cf. here fig. 1).

The reason the stone was raised upside down in the early 1900s may have been because the end below the runes is shorter than the other, somewhat narrower and bluntly pointed one. The runes begin now only 22 cm from the square end and finish 40 cm from the top. It was not likely that the person who put it up again in the first decades of the 1900s could actually make out the runes well enough to know that placing the broad end down might almost cover up the initial characters. He could perhaps have thought that the somewhat narrower end would go further down into the ground and thus keep the stone in an upright position for a longer period of time. If that is so, he was right: It stood for at least eighty years after being inverted, whereas it had fallen within twenty-five years after having been set up in its foundation with the correct orientation — but that was after at least 40 cm had been removed from the base in the early or mid-1800s.

It is a sobering fact that as good a runologist as Aslak Liestøl could have missed the point that the runestone stood upside down during his autopsy. It is even more disconcerting that Magnus Olsen was able to refer to Helliesen's article and drawing without realizing the importance of the illustration for the correct orientation of the runestone.

Although Helland II has been upside down for some eighty years, it will in 2013 be re-erected with the correct orientation. The runes will run in two

lines from the base upward, as they also do on N 245 Helland III, standing just 350 m away on the eastern Helland farm. It is only to be hoped that there will be sufficient support at the base this time for the stone to stay standing for hundreds of years.

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Early Runic Metrics: A Linguistic Approach

Bernard Mees

Recently much attention has been given to the metrical (or otherwise stylised) nature of many of the older runic inscriptions, the latest such study being that of Edith Marold (2012). Historiographically it has been linguists who have made the most lasting contributions to the study of metrics, the Neogrammarian mentor Eduard Sievers being the most fundamental contributor to the Old Germanic tradition, in particular in his *Altgermanische Metrik* (1893). It was somewhat unexpected, then, to see Michael Schulte (2009, 2010) follow Hans-Peter Naumann (1998, 2010) in choosing to use Andreas Heusler's (1925) much more accommodative scheme of Old Germanic scansion for his two surveys of the matter as Heusler is often criticised for having been rather too adventurous and unempirical in his treatment of the Old Germanic material. It is the Sieversian approach that dominates most recent linguistic investigations of Old Germanic metrics.

Marold's invocation of Sievers's more explicit system of description of Old Germanic metre makes better sense to me, as can be seen from the papers I published in 2007 and 2008. Yet Marold offers no comment on the key issue that informs my contributions to the matter. So few of the older inscriptions that alliterate (relative to those which appear in the younger futhark) can reasonably be classified as metrical under a rigorous application of Sieversian scansion that I came to the conclusion several years ago that the likely reason for this was the essential anachronism inherent in applying a system of scansion developed to explain literary material from the tenth century and later to texts of a much earlier date.

One of Elmer Antonsen's (2002, 3–13 and 129 f.) key criticisms of scholars such as Ottar Grønvik was their continual attempts to interpret the older texts so exclusively through the prism of Old Norse. Antonsen counselled that scholars should be aware of the fundamentally anachronistic nature of

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such an approach, of the danger of medievalist projection back on to earlier sources. The best way of controlling for such matters has long been seen to be to admit the importance of a balancing comparative perspective—i.e. one that seeks to view early runic texts from the opposite chronological perspective as well. The controversial form *arbija* on the Tune stone, for example, seems best translated as ‘funeral feast’ from the perspective of Old Norse as this is what its direct descendant *erfi* (usually) means. From a comparative Germanic or Indo-European viewpoint, however, the term seems better glossed as ‘inheritance’, as its relationship to Greek *orphános* ‘orphan’ (and the sense of being separated from the family) has long been held to indicate (Polomé 1982, 56). That Grønvik (1982) went so far here as to challenge the usual interpretation even of the comparative evidence seems to me to underline how compelling the tendency to anachronistic projection has often been in the runological field.

The opposing perspective in the case of runic metrics, however, is the one advanced in my papers (Mees 2007 and 2008)—i.e. the comparative metrical scheme adduced by the leading comparativists Antoine Meillet (1897, 1923), Roman Jakobson (1959 = 1962–88, 4: 414–63) and Calvert Watkins (1963 = 1994, [2:] 349–404). Marold seems to misunderstand this and not to have realised why I have sought to investigate the older runic texts from this perspective, so my reasoning may warrant re-emphasis. Rather than taking a medievalist approach to much earlier materials (à la Grønvik and Marold), I have tried to assess the earliest inscriptions in light of the scheme established by comparativists at the Indo-European level that the long line attested in early Northwest Germanic sources should have replaced or from which Seiichi Suzuki (1988) has proposed it may even have directly derived. This syllable-counting scheme with its alternation of short and long lines is prominently presented in Martin West’s (2007) survey of Indo-European poetics and also appears in Benjamin Fortson’s (2010) well-received introduction to Indo-European studies, so it seems an obvious (and legitimate) approach to explore, not one to be cursorily dismissed.

Suzuki (2008, 3) writes of *fornyrðislag* as being closer in form to the common Old Germanic long line than are any of its West Germanic counterparts, but how old the long line truly is (and whether or not other forms of metre were used by Germanic speakers in earlier times) remains unclear. It seems quite possible that knowledge of such a verse form made its way to England (say) along with the story of *Beowulf*—i.e. that it was not brought over from the Continent during the Anglo-Saxon conquest—and that alliterating long lines simply became more popular at some date than earlier forms of Old Germanic metre. We just do not know how old the

Northwest Germanic alliterative tradition is or how singular (or diverse) Iron Age Germanic poetry may have been either, the lack of comparable Gothic texts making things rather unclear from a comparative Germanic perspective. Marold's study is useful, however, in that it demonstrates clearly how medievalist projection can work. And it is perhaps her treatment of the Gallehus inscription, KJ 43, which brings out this issue best. The problem I see here is methodological, and in order to explain my reasoning it is perhaps worth detailing how I usually approach such matters from an analytical perspective. One debatable point I can excuse, two I consider cause for caution, but three seems to me to mark the boundary where a problematic interpretation crosses over into the realm of the unlikely.

There are three issues of concern in the way in which Marold approaches the Gallehus inscription, the first being her metrical assessment of dactylic *Holtijaz*. As Geoffrey Russom (1998, 3) indicates, this is clearly a form which is not acceptable under Sievers's system of scansion—indeed Erik Brate (1898, 336 f.) proposed that the reason that *fāhi* does not feature a pronominal clitic *-ka* (as does the form usually interpreted as *tōjeka*) in the Noleby inscription (KJ 67) was because of the Sieversian constraint against forms like *Holtijaz* appearing in the second feet of half-lines. Marold makes reference, however, to the *-i-* of *Holtijaz* being an “Übergangslaut”, citing the authority of the Germanic grammar of Krahe (1965–67, 2: 13). But what Marold is referring to here (without giving it its usually accepted linguistic name) is Sievers's law, a morphological process in Germanic (and Indo-European) which governs the production of *-i-* before *-j-* on the grounds of the natural prosody of the languages. Sievers's law is intrinsically linked to (natural) scansion—as Krahe explains in his *Formenlehre*, the metrically “long” syllable (i.e. “heavy” moraic weight) of *Holt-* generates the following *-i-*. Marold does not give examples of clearer cases of Sievers's law vowels being ignored in Old Germanic scansion (other than making a circular reference to the Tune inscription's equally problematic forms in *-ij-*) because there are none.

One such oddity does not make Marold's approach untenable, but it surely gives some cause for concern. The second issue, though, involves a question famously studied by Hans Kuhn in 1933. It is, moreover, a matter that I raise in my earlier papers, one of which (2008) is not cited in Marold's essay. Kuhn's 1933 paper seeks to explain the restrictions on the employment of pronouns which obtain in Old Germanic poetry, a matter that Marold curiously fails to consider in her 2012 offering.

As Russom (1998, 3) points out, the appearance of *ek* on the Gallehus horn in what has usually been taken to represent anacrusis is not expected

from the perspective of Old English. The allowance of material before the first Sieversian foot of a half-line Kuhn ascribed to the Common Germanic tendency to allow clitics and other weakly stressed (or unstressed) words to appear at the beginning (rather than in second position) of the sentence. Yet Old English *ic* 'I' never appears in anacrusis and the recent studies of Suzuki (2010) and Þorgeirsson (2012) suggest that anacrusis would not be expected to occur at all in the apparent on-line in the Gallehus text.

Each of the Old Germanic metrical traditions seems to have had subtly different rules concerning anacrusis and which terms may appear there. But Marold provides no examples of equivalent metrical feet featuring anacrusis in Old Norse for comparison or the appearance of *ek* in a metrically comparable environment. Forms such as Noleby's *fāhi* and the Vimose buckle's *wīja* (KJ 24) suggest that no pronoun needs to appear in the Gallehus text; unlike Old Icelandic, early Nordic seems to have had the option of discourse-initial pro-drop—of eliding a pronoun from the beginning of a text—so why the strong form *ek* would appear on the Gallehus horn remains problematic if it is to be understood as anacrustic. Although not so obviously a problem as that concerning *Holtijaz*, it still seems reasonable to recognise here a second significant difficulty with Marold's analysis.

The third issue relates to the inscription's genre. Erik Moltke (1985, 83f.) argued that a maker's text is unlikely to be expressed poetically and that the name of the inscriber and the medium of the object it appears on could be what has led to the appearance of alliteration in the text. Marold counters this objection by stressing the extraordinary nature of the Gallehus horns, but she does not provide any typological evidence to support her claim—Moltke was surely right to observe that maker's texts are not a genre in which we would expect to witness metricity. Marold does point to the syntax of the inscription, however (despite not invoking syntax as a criterion for metricity anywhere else in her paper), seemingly projecting the opinion of Lehmann (1993, 60) regarding the position of the verb in this inscription. As Eythórsson (2001, 2012) has shown, most of the early runic texts feature verb-second word order (much as do all of the later North Germanic languages), the Gallehus text being one of the very few older inscriptions where the verb comes after both its subject and direct object—i.e. in an analytically verb-late (rather than verb-second) position.

Why this might be considered evidence that the Gallehus text is metrical is not explained by Marold. She is presumably thinking of Lehmann's observation that verbs in the dependent (or "bound") clauses of Old Germanic poetry often come in last position and are typically unstressed (cf. Eythórsson 2009, 70, and Klein 1997, 269–80, regarding Lehmann's

mischaracterisation of Sanskrit accentuation). The problem with this notion is that the verb in the Gallehus inscription does not appear in a dependent (or “bound”) clause — and the verbs of main clauses do not usually appear in verb-late positions in Old Norse poetry. Marold seems to be continuing an error made by Lehmann here in her treatment of the Gallehus inscription.

Thus there are three quite odd and unexplained features in the five-word Gallehus inscription that Marold has failed satisfactorily to explain. Given so many questionable aspects and assumptions, it is hard to trust her declaration that the inscription is metrical. It may well be, and has often been taken to be so. But I do not think that Marold has approached her evidence in a sufficiently rigorous manner in this case. As Doug Simms (Southern Illinois University Edwardsville) has suggested to me, the problem of the extra syllable in *Holtijaz* can be explained by allowing the metrical expansiveness characteristic of the Old Saxon *Heliand* to apply at Gallehus (although this might upset Suzuki’s understanding that such developments of the long line should be seen as characteristically Old Saxon and relatively late), and it may well be that the use of *ek* in the Gallehus text is to be explained in a similar manner. Grønvik’s (1999) argument that *taujan* is to be understood as a *verbum donandi* might also explain the stylisation of the inscription, particularly in light of Lindemann’s (2000) formal demonstration that *taujan* is related etymologically to forms such as Latin *donare* (cf. Grønvik 2005, 17f., and MacLeod and Mees 2006, 176f.). The unexpected position of the verb may also be considered poetic (much as the Vimose buckle’s *wija* also seems to appear in a verb-late position), but not in the manner suggested by Lehmann, for as a careful reading of Eythórrsson’s (2001, 2012) syntactic analyses reveals, there is a tendency for early runic inscriptions which feature triple alliteration to display unexpected word orders. The Gallehus inscription may be considered metrical on grounds other than those adduced by Marold, but the problematic nature of her approach to the lost golden-horn inscription surely calls into question her assessments of the other texts surveyed in her paper, many of which seem to me to be marred by similar omissions, misunderstandings and doubtful characterisations.

I am only too aware that academic papers are rarely flawless and that the large number of contradictory interpretations of early runic experience can prove bewildering to assess — our principal aim should be to progress the runological historiography, not to focus on faultfinding. But James Knirk (2006, 334) has observed that it is how one defines “metrical” which is most crucially at stake here, and I am far from sure that the approach taken by Marold or even those (such as Schulte’s and Naumann’s) that she criticises at such length are the appropriate ones. Most of the work on Old Germanic

poetry produced by linguists in the century or more since Sievers's metrical studies first appeared suggests that Marold's medieval projection backwards in time is fundamentally mistaken; that her assessment does not address most of the metrical scholarship which has appeared since 1893 is more than just unfortunate. It can only be hoped that in future treatments more closely historicised and developmental perspectives (such as those afforded by Eythórsson's syntactic analyses or even Suzuki's rather more sophisticated Sieversian approach) may be employed in attempts to come to better understandings of (suspected) older runic metrical behaviour.

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Entgegnung zu Bernard Mees: „Early Runic Metrics: A Linguistic Approach“

Edith Marold

Allgemeines

Bernard Mees stellt gleich am Beginn seiner kritischen Bemerkungen („Early Runic Metrics. A Linguistic Approach“) zu meinem Artikel in der letzten Nummer von *Futhark* („Vers oder nicht Vers. Zum metrischen Charakter von Runeninschriften im älteren Futhark“) die entscheidende Frage, die sich alle Forscher stellen müssen, die an sehr frühe Texte mit den Werkzeugen einer Wissenschaft herangehen, die zumeist an mittelalterlichen oder späteren Epochen entwickelt wurden: Handelt es sich dabei um Anachronismus? Kann man die Regeln und Prinzipien, die man an mittelalterlichen Texten entwickelt hat, auch auf die Zeit davor anwenden? Das gilt nicht nur für die Metrik, sondern in gleicher Weise für Disziplinen wie Poetik oder Rhetorik. Darf man z. B. in frühen Texten von Metaphern oder Metonymien sprechen? Oder um ein Beispiel aus der historischen Linguistik zu nehmen, darf man Termini wie Bahuvrihi oder Dvandva aus der altindischen Poetik und Sprachwissenschaft auf Wortbildungen der germanischen Sprachen anwenden? Darf man mit den Kategorien einer aristotelischen Poetik an mittel- und frühmittelalterliche Texte herantreten? Nun, meines Erachtens, man darf es, weil diese Termini und Begriffe für den Fachmann wohl definiert sind (wenn es auch wie bei der Metapher erhebliche Differenzen geben kann). Jede Wissenschaft braucht Kategorien und Bezeichnungen, um das, was an einem Objekt erkannt oder begriffen wurde, für jedermann verständlich zu bezeichnen. Bisweilen dienen diese Kategorien und Begriffe

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auch als eine Art Brille oder Lupe, unter der man das Objekt betrachten und das Vorhandensein bestimmter Erscheinungen untersuchen kann. So und nicht anders soll die Anwendung der Sievers'schen Metrik auf die frühen Texte verstanden werden. Sie impliziert keineswegs, dass die frühen Texte etwa dieselben metrischen Schemata wie angelsächsische, altsächsische oder altisländische poetische Texte verwenden. Deswegen habe ich auch vermieden, Termini wie *fornyrðislag* oder *ljóðaháttir* zu verwenden. Dass die Untersuchung Ähnlichkeiten zu diesen altisländischen Metren aufzeigte, hängt zum einen damit zusammen, dass der metrische Befund gleich oder ähnlich ist, und das erklärt sich daraus, dass diese Texte des Früh- und des Hochmittelalters in einem historischen Kontext stehen, der zumindest keine Phänomene zeigt, die auf absolute kulturelle Brüche schließen lassen, wohl aber ein größeres oder kleineres Maß an Veränderungen voraussetzen.

Wenn man die Sievers'sche Metrik anwendet, geht man von bestimmten Voraussetzungen und Annahmen aus, die nicht mehr hinterfragt werden. Diese sind: 1. Metrische Dichtung der Frühzeit ist rhythmische Dichtung, nicht takthaltige Dichtung (Heusler). 2. Dieser Rhythmus setzt sich zusammen aus einem Gefüge von Hebungen und Senkungen, die nach bestimmten Schemata verteilt sind. Hebungen und Senkungen unterscheiden sich durch Wortbetonung, und für die Hebungen durch die erforderliche Silbenlänge. Wieweit auch Satzbetonung oder syntaktische Phänomene dabei eine Rolle spielen, kann erst die Untersuchung selbst ergeben und ist nicht vorauszusetzen. 3. Und genauso ist die Frage, ob dieses rhythmische Gefüge auch den Sievers'schen Typen entspricht, nicht Voraussetzung, sondern kann erst Ergebnis der Untersuchung eines Textes sein. Die Sievers'schen Typen gehören also überhaupt nicht zum Ausgangspunkt einer metrischen Untersuchung, sie können aber ihr Ergebnis sein. 4. Diese Untersuchung kann auch charakteristische Entsprechungen und Abweichungen von späteren Epochen aufzeigen. So führte die Untersuchung der Inschrifttexte zu dem (auch für mich überraschenden Ergebnis), dass die untersuchten Texte durch dieses Typensystem beschrieben werden können, wenngleich sich auch deutliche Unterschiede zu späteren Zeiten zeigten, wie z. B. die Häufigkeit von dreihebigen, stabenden Zeilen.

Mees bedauert, dass „so few of the older inscriptions that alliterate (relative to those which appear in the younger futhark) can reasonably be classified as metrical under a rigorous application of Sieversian scansion” (S. 111). Dazu ist zu sagen, dass Alliteration keinen Vers voraussetzt und ihn auch nicht konstituiert; es gibt Alliteration auch in Prosatexten. Sie kann zum Vers hinzukommen und sich mit den Hebungen verbinden, und damit

entstehen spezielle Versmuster wie z. B. die germanische Langzeile oder die dreihebige Vollzeile.

Mees wendet sich gegen den Anachronismus, den er nicht nur in der Anwendung der Metrik späterer Zeiten auf frühere Texte sieht, sondern auch in der Heranziehung von späteren Sprachen zur Erklärung von frühen Texten, wie er sie Grønvik vorwirft. Er schlägt dann aber ein nicht minder anachronistisches Verfahren vor, nämlich die Anwendung von rekonstruierten metrischen Schemata von „Indo-European poetics“. Hier kommt zur erheblichen zeitlichen Distanz (diesmal in anderer Richtung) auch noch eine erhebliche geographische Variation hinzu. Um dies zu veranschaulichen: Sievers entwickelte seine Metrik aus Texten, die zwischen 350 bis 700 Jahre nach Tune zeitlich situiert sind. Wenn man aber von einer indogermanischen Poetik ausgeht, so müsste man wohl gut zweitausend Jahre Distanz überbrücken. Wenn der Anachronismus der Erklärung von in Skandinavien gefundenen Texten durch das Altisländische beanstandet wird, was soll man bei einer indogermanischen Metrik sagen, die aus Sprachen entwickelt ist, die aus Sprachen entwickelt ist, die geographisch über Europa hinaus verbreitet sind?

Das Horn von Gallehus

Mees hat seine Kritik auf die Inschrift des Hornes von Gallehus beschränkt und dabei auf vier Fehler hingewiesen. Dazu ist im Einzelnen Folgendes zu sagen.

*Die metrische Skansion von **holtijaz***

Nur wenn man die Sievers'sche Metrik auf das beschränkt, was er zur skaldischen Metrik schreibt, wird man annehmen, eine solche Form wie **holtijaz** sei „not acceptable“, wie Mees meint. Es handelt sich dabei aber zunächst ganz einfach um eine Hebung und zwei Senkungssilben. Dass Sievers so etwas durchaus akzeptiert, möge folgendes Zitat belegen (1893, 28):

Zur bildung einer senkung im engeren sinne (leichte senkung) genügt eine sprachlich unbetonte silbe Es können jedoch auch mehrere solche silben ... zusammentreten, vorausgesetzt dass ihre folge nicht durch einen sprachlichen nebeton durchbrochen wird.

Natürlich handelt es sich bei **holtijaz** um ein Phänomen, das in der Sprachwissenschaft auch als Sievers' Gesetz bekannt ist. Allerdings zeigt ein Durchgang durch das sprachliche Material der älteren Runeninschriften, dass es zumindest zur Zeit der älteren Inschriften eine erhebliche Zahl von Fällen

gibt, wo es nicht zutrifft. Vgl. *-ijaz/-ijō* oder *-ijan* nach (wahrscheinlich) kurzer Silbe: **ladawarijaz** Tørvika A, **bidawarijaz** Nøvling, **stainawarijaz** Rö, **harija** Skåäng, **awija** Öttingen, **prawijan** Kalleby, **talijo** Vimose, Hobel. Krause (1971, 94) hat das bereits bemerkt, und nahm an, dass es sich um „eine Abweichung vom ursprünglichen Zustand“ handle. Man könnte aber auch erwägen, ob sich diese Regel nicht vielleicht erst später entwickelt haben könnte. Wie dem auch sei, mit der Metrik hat dieses Gesetz überhaupt nichts zu tun. Die Annahme das Sievers'sche Gesetz sei „intrinsicly linked to (natural) scansion“ wie Mees meint, wird gerade durch die Existenz einer nicht geringen Zahl von Ausnahmen widerlegt. Dass es bei diesem Gesetz auch um kurze und lange Silben geht, ist wahrlich kein Argument für einen Zusammenhang mit der Metrik. Eine metrische Analyse von *Holtijaz* hat zunächst zu konstatieren, dass zwei Senkungsilben vorhanden sind. Man kann sich allerdings fragen, inwieweit dem *-i-* in *-ijaz* der Wert einer Mora zukommt oder nicht, Argumente sind hier allerdings schwer zu finden. Am Verscharakter ändert das jedoch nichts, ob man hier nun eine oder zwei Senkungsilben annimmt.

Der Auftakt in der Gallehus-Inschrift

In den Einwänden von Mees zeigt sich deutlich die Gefahr, vor der er selbst warnt: Ausgehend von Kuhns (nicht unumstrittenem) Satzpartikelgesetz und den Beobachtungen am Altenglischen stellt er fest, dass *ek* nicht im Auftakt stehen dürfe. Nun, wir haben zunächst einmal zu akzeptieren, dass das der Fall ist. Nach einem vergleichbaren Phänomen zu suchen, ist angesichts des schmalen Corpus von 16 Inschriften nicht sinnvoll. Generell ist zu sagen, dass Auftakte in dem vorliegenden Corpus von metrischen Inschriften der frühen Zeit zwar nicht häufig, aber durchaus in der Metrik Sievers' vorgesehen sind (1893, 31).

Eine metrische Herstellerinschrift

Auch hier gilt: Zunächst ist zu akzeptieren, dass die Inschrift metrisch ist, d. h. dass sie einen bestimmten Rhythmus und eine dem Langzeilenschema entsprechende Verteilung der Alliteration hat. Natürlich könnte man das alles dem Zufall zuschreiben: Zufällig habe ein Mann mit einem mit *h* anlautenden Namen ein Horn gemacht, das nun fatalerweise auch mit *h* anlautet, und die Rhythmik des Satzes, die eine klare metrische Skansion möglich macht, samt der abweichenden Syntax sei ebenfalls Zufall. Das Argument, das diesem metrischen Befund entgegensteht, ist: Hersteller machen keine

metrischen Inschriften. Man sollte sich aber hüten, wie Moltke (1985, 83 f.) die eigene kulturelle Perspektive zu überschätzen, was ein angemessener Inhalt für eine metrische Gestaltung sei. Um ein Beispiel aus sehr viel späterer Zeit anzuführen: Wer hätte gedacht, dass Goethe seine botanischen Erkenntnisse über die Urpflanze in Versen niederlegen würde?

Die Verbposition

Es ist schwierig, zu Gedanken Stellung zu nehmen, die man in keiner Weise im Text niedergelegt hat, sondern die jemand vermutet, dass man sie gedacht hat. Daher sei hier ganz deutlich gesagt: Ich habe keineswegs an Lehmann gedacht, weil seine Beobachtungen, was die Stellung des Verbs in abhängigen Sätzen betrifft in der Inschrift von Gallehus nicht anwendbar sind; dies erstens, weil es sich um einen Hauptsatz handelt, und zweitens, weil das Verb Träger einer Hebung ist und daher betont.

Zum abschließenden Urteil, dass es dem Verfasser scheine, dass die Untersuchung der restlichen Texte unter ähnlichen Auslassungen, Missverständnissen und zweifelhaften Charakterisierungen leiden würden, ist zu sagen, dass solche Extrapolationen in der Relation von 1:16 nicht eben wissenschaftlich sind, dass ich aber hoffe, dass diese Fehler und Irrtümer ebenso leicht aufzuklären und zurückzuweisen sind wie die zur Gallehus-Inschrift. Zur Kritik des methodischen Ansatzes, eine Untersuchung mit Hilfe der Sievers'schen Metrik an diesen Texten zu machen, als „fundamentally mistaken“, ist zu sagen, dass die Prinzipien der Metrik Sievers' auf den heutigen Tag angewandt werden und sich bewähren. Dass diese Metrik und ihre Prinzipien bereits 1893 so klar dargelegt wurden, spricht nicht gegen sie. Aber den Vorwurf, ich würde die metrische Forschung seit dieser Zeit nicht kennen, muss ich zurückweisen. Man muss nicht alles, was man gelesen hat, in das Literaturverzeichnis aufnehmen. Dieser Versuch mit der Sievers'schen Metrik schließt jedoch keineswegs weitere und andere aus und es ist den Vertretern einer indogermanischen Metrik unbenommen, dasselbe zu versuchen.

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Finsta i Skederid (U ATA3916/47)

Svante Fischer

Övergångs- och tidigvikingatida inskrifter som blandar äldre och yngre runformer är av stort intresse för runologin. Oftast stör inte de olika runformerna läsningen. Äldre och yngre allografer återger i regel ett och samma språkljud, till exempel på Sparlösastenen (Vg 119), där både **ᚱ** (ᚱ) och **ᚱ** (ᚱ, ᚱ), liksom **ᚱ** (ᚱ) och **ᚱ** (ᚱ) förekommer. Mer komplicerad är dock övergången från en äldre till en yngre runrad där antalet tecken reduceras samtidigt som specifika grafer försvinner. Exempel på detta är runorna **g**, **w**, **e**, **d**, **o** som ersätts med **k**, **u**, **i**, **t** resp. **u**. För den runologiska teoridiskussionen måste framförallt understrykas att en grafematisk förändring inte kan ske för bara en graf i taget. Istället måste grafematisk förändring ta hänsyn till samtliga använda grafer i en regional kontext. En grafs möjliga utformning begränsas av andra i bruk varande grafer, med vilka den inte får förväxlas, annars inträder grafematisk förvirring. Det är inget problem att ha flera allografer för samma språkljud med motsvarande allofoner. Problemet är att man inte kan använda grafer som är identiska med andra grafer med annorlunda ljudvärde. Grafematiska innovationer som förs in utifrån är därför särskilt problematiska och måste successivt integreras om de ska användas i någon större utsträckning. Detta betyder att grafematisk förändring från den äldre samgermanska till den yngre nordiska runraden nödvändigtvis skett stegvis, över en längre tid (Schulte 2009, 116; Barnes 2009, 139). Det förefaller vara så att språkljud kunde få ny grafematisk representation efter det att andra ljud inte längre återgavs grafematiskt på samma sätt, t ex ersattes den urnordiska **H**-runan med en enkel bistav med den graf som tidigare användes för **A**. Men det krävs en konsekvent jämförande bevisning som vetenskaplig metod för att undvika cirkelresonemang eller att man frångår deduktiv metod. Jämförande belägg för en successiv anpassning kan återopas från fornhögtyskt område i form av griffelruninskrifter och manuskriptrunor. I kontinental karolingertida klostermiljö under sent 700-tal och tidigt 800-tal ges inlånade grafer successivt nya ljudvärden i samklang med regional

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fonologisk språkförändring (Arntz 1944, 169, 229, 231f.; Derolez 1954, 2; Nievergelt 2009, 16, 49–51). Jämförelsen med dessa kontinentala belägg visar att viktiga grafer i nordiska övergångsinskrifter, särskilt den äldre **d**-runan och olika varianter av den tidigvikingatida **o**-runan (𐌺, 𐌻, 𐌿, 𐌾), borde tolkas utifrån de möjligheter de hade att kunna representera specifika ljudvärden inom ramen för regional ristarpraxis snarare än utifrån antagandet om existensen av ett artificiellt supraregionalt spridningsmönster.

Fyndomständigheter, tidigare läsning och tolkning

1947 återfanns ett runstensfragment av sandsten under jordbruksarbete vid Finsta i Skederid i Uppland på en åker nära kyrkan i ett rikt fornlämningsområde. Sven B. F. Jansson inspekterade fyndet 1950. Först fyra år senare publicerade han dock en bild och en kort notis om fyndet i *Fornvännen* (Jansson 1954). Samtidigt med fyndet 1947 grävdes ett par störda gravkontexter en bit därifrån. Gravarna var skelettgravar utan några anmärkningsvärda gravgåvor. Däremot återfanns kistspikar av järn, vilket antyder en sen datering till yngre vikingatid och en grav inom ramen för kristen ideologi. Runstensfragmentet från Finsta i Skederid visar däremot upp runformer både ur den äldre och yngre runraden. Den tillhör därmed ett betydligt äldre stadium av runristningar av övergångstyp som sträcker sig från vendeltid in i tidig vikingatid och står därmed knappast i säker relation till de utgrävda gravarna. Runstensfragmentet från Finsta translittereras enligt Magnus Källströms redogörelse (2007, 52):

Rad 1 ...-**ont**rsta...

Rad 2 ...**ua**ish**a**...

Rad 3 ...-**rum**

Noteras bör att den femte läsbara runan i andra raden tolkas som ett **h**, fast med dubbla korsande bistavar (s. 51):

The rune form of the Finsta fragment does not contradict an interpretation of the rune as **d**, but if ...-**ont**r is the remains of the word *stændr* the carver has used the **t**-rune to denote /d/ and we do not expect him to also have a special **d**-rune. Consequently, the disputed rune on the Finsta fragment should be interpreted as an **h**-rune.

Denna tolkning förutsätter ett unikt grafem, fortsättningsvis benämnt ”**h**” (𐌺), som mig veterligt inte uppträder utanför Finsta-inskriften. Källström (s. 52, fig. 4) ser också ett ”**h**” i den första runan i den fjärde raden i en chiffer-

inskrift på en kopparamulett från Staraja Ladoga (X RyMelnikova2001;196), vilken sålunda skulle utgöra förebild för en av allt att döma helt ochchifferad och utan vidare läsbar steninskrift i Uppland. Det förefaller mindre sannolikt, eftersom McKinnell, Simek och Düwel (2004, 64f.) med all tydlighet visar att Staraja Ladoga-inskriftens chiffer är enhetligt utförd enligt redundansprincip, där bistavar dubbleras på de första runorna i de tre första raderna och därmed med sannolikhet också i den första runan i den fjärde raden. Den återopade runan ifråga måste följaktligen läsas som ett helt vanligt **H** som i ett chiffer fått ytterligare en korsande bistav och alltså ingenting har med Finsta-inskriften att skaffa. Det är osannolikt att man använt en graf, som annars bara förekommer i en chifferinskrift på ett ideologiskt laddat lösföremål, i en helt vanlig steninskrift med en annan form av ideologiskt innehåll. På samma vis är det inte troligt att redundanschiffer skulle kunna influera generell grafematisk utveckling av runraden. Här vill jag också understryka att jag som helt förståeliga felläsningar av Arthur Nordén (1962) kan avfärda de påstådda belägg för enstaka **M**-runor på amuletterna från Birka och Ulvsunda som tidigare anförts som belägg för övergångs- och tidigvikingatida inskrifter, efter ha fått ta del av Sonia Steblin-Kamenskayas mikroskopfotografier. Faktum är att de med Finsta-ristningen i tid och rum närmaste jämförbara beläggen är Rickebytarningen (U HG1989;44) i Uppland från c:a 650 e.Kr. och Ög 136 Rök från omkring 800 e.Kr. Slutsatsen blir därför att Finsta-fragmentets runa med två huvudstavar och dubbla korsade bistavar skall läsas som **d**, liksom i alla andra ochchifferade runinskrifter från Norden.

Ett nytt läsnings- och tolkningsförslag

Res unica nulla est. Ett enstaka belägg bevisar inte en allmängiltig sanning, oavsett tentativa fonotaktiska fördelar. Eftersom det är osannolikt att en graf ”**H**” med dubbla, korsade bistavar skulle förekomma en enda gång i den samlade korpusen av övergångs- och tidigvikingatida inskrifter måste tills vidare en annan tolkning presenteras. Som jag ser det måste den nya tolkningen utgå från flera jämförbara belägg där en graf med två huvudstavar och två korsade bistavar emellan står för **d**, samtidigt som **t** också kan denotera /d/ och alltså är en allograf till **d**. Det finns flera sådana fall med vilka man kan jämföra Finsta-inskriften. Jag vill därför presentera en ny läsning och tolkning av runstensfragmentet från Finsta. Totalt förekommer 18 runor i Finsta-inskriftens tre rader. Ordgräns markeras inte. Andra tecken än runor saknas. Rad 1 är sedan tidigare korrekt återgiven. Rad 2 kan inledas med en mening med adv. *svā* ’så’ (jfr. **sua** på Vg 59, U 947, H SS1979;13), följd av relativpartikeln *es* ’som, han’ (jfr. **is** på bl.a.

Sö 217) eller pres. 3 p. sg. *es* 'är' (jfr bl.a. Ög 136 Rök; Stroh-Wollin 1997). Rad 2 skulle då kunna syfta på frasen i rad 1: *stændr stæin, svā es da...* Det finns också möjligheter att läsa **uais**, men av fonotaktiska skäl bör det oavsett vilket gå en ordgräns mellan denna sekvens och **d**. Eftersom bara ytterligare en runa **a** följer **d** i Finsta-ristningen är det svårt att föreslå en entydig tolkning. Det kan röra sig om en begreppsruna **d**, med betydelsen *dagr* 'dag' som på Ög 43 Ingelstad, eller om möjligheten att *Dagr* skulle vara ristat **dakr**, vilket dock saknar paralleller. Andra lösningar är tänkbara så länge de utgår från att vi har att göra med ett **d**. De två **a**-runorna i rad 2 har bistavar som går tvärs över huvudstaven. Så är inte fallet med den första runan i rad 3. Inget hindrar att det är en skadad runa **h**, med endast en från vänster stigande bistav, precis som på Rickebytarningen (Gustavson 1989). Källström (2007, 53) föreslår att den skadade första runan i rad 3 skulle kunna vara ytterligare ett "h" med dubbla korsade bistavar och vill därför i tredje raden se ett manligt personnamn *Hrōðmundr* (jfr Peterson 2007, 120). Efter min egen undersökning av Finsta-fragmentet 3 december 2009 anser jag den tidigare läsningens identifikation av den skadade första runan som felaktig men suppleringen och tolkningen av runorna i rad 3 som riktig. Det bör understrykas att där dubbla korsande bistavar förekommer, runan **d** i rad 2 och runan **m** i rad 3, är ristningslinjernas ytterst tunna men djupt huggna. Den breda och ojämna fördjupning som finns i underkanten av den skadade första runan i rad 3 har en liknande motsvarighet utmed underkanten efter sista runan i rad 3. Dessa är bägge snarast ojämnheter i stenytan och inte ristade linjer. Jag skulle därför istället vilja föreslå följande translitterering, normalisering och översättning:

Rad 1 ...-ontr sta...	... [st]ændr sta[in] ...	"... står sten ..."
Rad 2 ...ua is da...	... [s]va(?) es(?) Dagr(?)...	"... så(?) som(?) Dag(?) ..."
Rad 3 ...Hrum...	... Hrom[und(r)](?) ...	"... Hromund(?) ..."

Grafematisk och fonologisk förändring i ett supraregionalt perspektiv

Källström önskar att se en koppling mellan Finsta-ristningen och danska och ryska kontexter, särskilt Helnæs-Gørlev-gruppen (2007, 55):

Since the rune-stone from Finsta has traits in common with the Danish rune-stones of the Helnæs-Gørlev group, it should maybe be seen as a forerunner to the rune-stone custom in this part of Uppland, and would further strengthen the idea of Denmark as the prime source for the rune-stone tradition in the area.

Det verkar dock finnas för få specifika grafematiska drag mellan Finsta och de danska inskrifterna för att man skulle kunna säkerställa ett specifikt inflytande, ett belägg för en supraregional tendens i form av en enhetlig ristarkultur i övergångsfasen mellan de både runraderna. Inte minst den yngre **o**-runans bistavar tvärs över huvudstaven i Finsta-inskriften är signifikanta samtidigt som **a** fortfarande förekommer på Snoldelev-stenen (DR 248), Flemløse-stenen (DR 192) och Avnslev-stenen (DR 189). **m** saknas i futharkerna på Gørlev 1 och Malt-stenen (DR NOR1988;5). Ett annat viktigt argument mot en koppling är att Helnæs-Gørlev-gruppen begagnar sig av en typ av chiffer som inte förekommer i vare sig Staraja Ladoga eller Finsta-ristningen, nämligen repetitiva sekvenser av åtskilda runor, jfr Gørlev 1-stenens **pmkiiisssttiiiilll**, Gørlev 2-stenens (DR EM85;239) **pppuuu?nnn?** och Malt-stenens **titultitul** och **utu:tuuut**. Rökstenens olika räknechiffer ska vi inte gå in på, men de visar knappast på någon direkt relation till Staraja Ladoga eller Helnæs-Gørlev. Det finns heller inga säkra belägg för att runstensresarformlerna i Helnæs-Gørlev-gruppens inskrifter på något sätt skulle ha influerat uppländska runstensresare och runristare. De äldsta beläggen och närmsta parallellerna finns istället i Östergötland, jfr KJ 59 Ällerstadstenens **raisidoka stainar** och Rökstenens **aft uamup stonta runar þar**. Inget hindrar därför att regional variation i Mälardalen står bakom Finsta-inskriftens betydande särdrag. Tills vidare kan vi notera att inga kända belägg för Helnæs-gruppens maskros-**m** (𐌛) eller Gørlev-gruppens **o**-runa förekommer i Uppland, något som starkt talar emot en direkt koppling mellan dessa danska runinskrifter och Finsta i Skederid.

Rökstenen som jämförelse

På Rökstenen finns många av de äldre runorna belagda i två partier av inskriften, raderna 21–22. Jämförelsen med Rökstenen är ytterst relevant eftersom Finsta-ristningens **d**-runa fyra gånger motsvaras av samma graf på Rökstenen (Källström 2007, 51, fig. 3). Ett viktigt påpekande är att de äldre runorna används för närliggande ljudvärden på Rökstenen, man leker med allofoner och allografer. Det förefaller finnas en avsiktlig baktanke i detta. Det skulle kunna handla om en lärd uppvisning där man medvetet använder sig av äldre allografer, vilka man av hävd känner till har underordnats som passiva, vilande allografer i den nya 16-typiga nordiska runraden som av allt att döma är fullt utvecklad på Rökstenen i de andra raderna. I de tre raderna med äldre runor (varav en helt säkert är ett chiffer) växlar ristaren mellan att använda **w** och **o** (𐌛) för **u**. Tidigare forskning har gjort gällande att Rökstenens chifferformer för **a** (𐌛) och **i** (𐌚) skulle vara nya påfund. Jag

tror detta är ett misstag. Jag vill hävda att både härstammar från den äldre **j**-runan, men att man skiljer mellan **A** och **j** precis som på DR 357 Stentofte. Det rör sig alltså inte om en chifferad **i**-runa, utan en **j**-runa som används för **i**, precis som **e** används för **i**. Raderna 21–22 med äldre runor ska alltså translitteras sålunda:

sagwmogmeni þad hOar igOldiga Oari gOldin d gOonar hOsli

Här kan man notera att runan **d** representerar språkljuden /d/ i *goldinn* och /t/ i *þat*. Runan **d** måste alltså ha någon form av allografisk status jämte **t**, enligt ristaren. Jämförelsen mellan Rökstenen och Finsta i Skederid stöder alltså läsningen av vad som ser ut som en **d**-runa som just en **d**-runa. Och inget hindrar således att runorna **d** och **t** bägge betecknar /d/ i en och samma inskrift.

Den fornhögtyska runtraditionen och manuskriptrunrader som jämförelse

Kan utvecklingen av **t** och **d** till allografer under en övergångsperiod beläggas i andra områden med liknande runtraditioner? Man kan notera att den speciella runformen av **d** som uppträder på Finsta-fragmentet tidigast kan beläggas i Rhenlandet redan under äldre Merovinger-period II (c:a 530/540–560/570 e.Kr.), eftersom runformen uppträder både på dräktpännet från Freilaubersheim (KJ 144): **boso wraet runa þk dapiña goldida** samt svärdskidsmunblecket från Eichstetten, grav 186 (Opitz 1981, 29–31): **danil muni wiwol**. Bägge inskrifterna kommer från väldaterade slutna gravkontexter (Krause och Jankuhn 1966 resp. Sasse, Alt och Hollack 2001, 201, 434, pl. 79). Men under andra hälften av 600-talet e.Kr. upphör skicket att nedlägga gravgåvor på kontinenten och därmed försvinner möjligheten att finna runristade föremål på merovingertida gravfält. Mellan c:a 630 och 750 e.Kr. finns alltså en lucka där runornas utveckling är svår att följa eftersom den materiella kulturen manifesterar sig annorlunda i övergången från merovingertid till karolingertid. Låt oss därifrån flytta blickfånget till de karolingertida klostren som till exempel Fleury i Frankrike, Fulda i Tyskland, och Sankt Gallen i Schweiz. Här finns en rad olika runrader och marginalia i handskrifter. Till denna korpus skall tillfogas runalfabetet från Rom (Franzén 1988) och de fornengelska runristningarna på relikskrinen från Auzon, Gandersheim och Mortain. Ett tiotal olika griffelinskrifter på fornhögtyska (jfr Nievergelt 2009, 75) samt runica manuscripta i totalt elva olika handskrifter finns bevarade i Sankt Gallen. Detta mycket viktiga material får betraktas som relativt okänt och kunskapsluckor

är därför helt ursäktliga. De fornhögtyska griffelinskrifterna visar på en bred folklig tradition och kan inte viftas bort som ”lärd spekulation”. Det går helt säkert att spåra ljudförändringar i manuskriptrunraderna (Derolez 1954, 125). De som skriver ned runraderna står inför samma problem som alla andra runkunniga vid den här tiden. Vad gör man när de ursprungliga ljudvärdena och runnamnen inte längre motsvaras i det talade språket? Det som framförallt är tydligt i manuskriptrunraderna är att man tacklar den andra tyska ljudförskjutningen stegvis, och inte med en enhetlig reform. Ljudövergången kan spåras mellan runinskriften på bältespannet i Pforzen, grav 239, som dateras av Babucke (1999b, 22) till äldre Merovinger-period III (ca 560/570–590/600 e.Kr.) och en uncial griffelinskrift i Sankt Gallen handskrift 11, vilken dateras till 800-talet (Nievergelt 2009, 41): Pforzen **andi** resp. Sankt Gallen handskrift 11 *enti*. Vidare kan man visa att **t**, **d**, och **p** har specifika ljudvärden ända in i denna period, jfr runinskriften på elfenbensringen i Pforzen, grav 255: **aodlip wrait runa** som är det yngsta belägget där alla tre runorna förekommer samtidigt (Babucke 1999a). Efter äldre Merovinger-period III svävar man i dunkel fram till dess att 800-talets runmanuskript kommer i dagen.

Runraden i Sankt Gallen, Stiftsbibliothek, handskrift 270

Den långa runraden i Sankt Gallen handskrift 270, den s k Isruna-traktaten, har tidigare påståtts vara en förtyskad anglosaxisk runrad (jfr Derolez 1954, 90–94). Utifrån senare forskning (Nievergelt 2009, 49–51, 68 f.) skulle Isruna-traktaten kunna ses som en fornhögtysk runrad som utökats med inlånade anglosaxiska grafer. Traktaten dateras till första hälften av 800-talet och är således helt samtida med Rök- och Sparlösa-inskrifterna, och därför sannolikt också med Finsta-ristningen. Eftersom runraden i Sankt Gallen handskrift 270 inte är anglosaxisk utan fornhögtysk speglar runornas namn och ljudvärden inte fornengelskan utan övergången mellan protofornhögtyskan och fornhögtyskan. Här har *p* > *d* (runan *Dorn*) och *d* > *t* (runan *Tag*) skett men däremot ännu inte *t* > *z*. Runan som senare skall komma att bli *Ziu* heter fortfarande *Ti*. Då runorna *Ti* **t** och *Tag* **d** återger samma språkljud /t/ har de ansetts vara allografer och återges med samma unciala *t*.

Sankt Gallen handskrift 878

Handskriften 878 har på lösa grunder tillskrivits abboten Hrabanus Maurus i Fulda och daterades därför tidigare till 819 e.Kr. (Arntz 1944, 168). Derolez (1954, 74) anför sannolikt med rätta en datering till 840-talet. Handskriften tillhörde 1457 katedralbiblioteket i Chur och har först senare hamnat i Sankt

Gallen (a.st.). Den visar att man kände till den 16-typiga futharken, liksom den anglosaxiska futhorcen även om man själv använde den fornhögtyska versionen för att skriva på både latin och fornhögtyska. Det finns viktiga skillnader mellan handskrift 270 och handskrift 878. I de hrabaniska runalfabeten har hela ljudövergången $p > d$, $d > t$, $t > z$ redan skett och runan **t** beskrivs följaktligen i uncialer som *ziu*.

Runraden i Bryssel, Koninklijke Bibliothek, handskrift 9565–9566

Handskriften Bryssel, Koninklijke Bibliothek, 9565–9566 härstammar från klostret Saint Laurent i Liège. Kontakterna mellan Sankt Gallen och detta kloster var betydande och munken Notker från Sankt Gallen blev sedan biskop i staden åren 972–1007 e.Kr. (Derolez 1954, 95). I handskriften ges **t**-runan namnet *Tag*. Utvecklingen är helt logisk eftersom det gamla namnet på **t**-runan förändrats till *Ziu*. Men någon **z**-runa var inte önskvärd, följaktligen fick namnet på **d**-runan som förändrats till *Tag* helt enkelt flyttas till **t**-runan, samtidigt som både runnamnet *Ziu* och **d**-runan utgick ur systemet. Runraderna på kontinenten visar en stegvis övergång i samklang med ljudförändringar. Så borde det ha gått till i Mälardalen, och Finstaristningen är rätt tolkad det främsta beviset på detta.

Sammanfattning

Sammanfattningsvis kan sägas att Finsta-inskriften innehåller de äldre runorna **d** (𐌆) och **m** (𐌚), samt troligen **h** (𐌺) med en enkel bistav från vänster sluttande mot höger. Säkra belägg för chiffer eller en tidigare oattesterad graf ”**h**” (𐌺) med dubbla korsande bistavar saknas i inskriften. Troligare är att den speglar en regional grafematisk utveckling snarare än en supraregional. Inskriften bör ses inom perspektivet där övergången mellan den äldre och yngre runraden sker stegvis, vilket också förefaller vara fallet i det merovingertida Nordsjöområdet och på den karolingertida kontinenten. Jämförelsen med Rökstenen och Finsta i Skederid å ena sidan, den fornhögtyska runtraditionen och manuskriptrunraderna å den andra är mycket relevant och tillför en hel del i diskussionen. Jämförelsen visar att under övergångsperioder kan runorna **d** och **t** som ursprungligen stått för olika ljudvärden få allografisk status om de under en period av fonologisk språkförändring hamnar tillräckligt nära varandra fonologiskt samtidigt som runformerna är tydligt distinkta från varandra.

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Finstastenen i Skederid — ett genmäle

Magnus Källström

I mitten av 1900-talet hade Riksantikvarieämbetet ett energiskt ombud i Norrtäljetrakten, som hette J. M. Johansson. Han var kantor i Frötuna församling, och det är till stor del hans förtjänst att det lilla runstensfragmentet från Finsta i Skederid har räddats åt eftervärlden. Johanssons skrivelser till Riksantikvarieämbetet kännetecknas av utförlighet och stor noggrannhet. Som ett exempel kan nämnas hans behandling av ett tidigare okänt fragment av runstenen U 517, som sommaren 1944 hade påträffats under golvet i Skederids kyrka. Johansson författade en så uttömmande rapport om stenen och dess inskrift (Johansson 1946) att Sven B. F. Jansson inte gjorde någon egen beskrivning, utan nöjde sig med att tillfoga: ”Inskriften är riktigt läst av kantor J. M. Johansson”. Även för Finstafragmentet skrev Johansson (1947) en utförlig rapport, men här möttes han av en del svårigheter. Märkligt nog var det inte den nu omtvistade runan \mathfrak{M} som i första hand bekymrade honom — den läste han utan större tvekan som ett kryssformat skiljetecken omgivet av två **i**-runor — utan förekomsten av ett tecken liknande den äldre runradens **m**-runa.

När Sven B. F. Jansson undersökte och målade upp fragmentet 1950 valde han att endast återge inskriften med runor i sin rapport och han gav mycket summariska kommentarer till läsningen. Runformerna \mathfrak{M} och \mathfrak{M} kommenterade han inte alls. Däremot publicerade han ett foto av stenen och skrev några rader om fyndet i *Fornvännen* (Jansson 1954, 3 f.), vilket fram till att jag publicerade min artikel 2007 i stort sett var det enda som fanns i tryck om denna sten.

Kring 2000-årsskiftet började jag undervisa på kursen i runologi vid Institutionen för nordiska språk på Stockholms universitet, och jag brukade då ofta ha en tentamensfråga om just denna runsten. Uppgiften var ganska enkelt utformad. Studenten skulle utifrån ett fotografi undersöka olika drag

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i inskriften och försöka datera stenen. Det var också en tacksam uppgift för mig, eftersom det var svårt för studenterna att fuska. Runtexdatabasen var då inte särskilt spridd och den translitterering som fanns där byggde ännu på J. M. Johanssons delvis felaktiga läsning. De allra flesta kom ändå fram till att stenen måste tillhöra den s.k. Helnæs-Gørlevgruppen och alltså dateras till tidig vikingatid. Många uttryckte dock sin förvåning över runan **ᚱ**. Några trodde att det kunde vara den urnordiska (eller anglosaxiska) **d**-runan, men de allra flesta kom fram till att det måste vara en variant av **h**. Skälet var att de i den övre raden kunde läsa ...**ᚱontr** [s]tændr, där det tydligt framgick att ristaren använde **t**-runan som tecken för /d/. Rent logiskt borde därför **ᚱ** svara mot ett annat tecken i den yngre runraden och skillnaden mot **ᚱ** som ju står för ⟨h⟩ inom Helnæs-Gørlevgruppen är ju inte särskilt stor.

Nu har Svante Fischer i ett föredrag vid det internationella runsymposiet "Runes in Context" i Oslo 2010 och i en artikel i detta nummer av *Futhark* hävdad att det inte rör sig om en **h**-runa, utan en **d**-runa. Detta kräver givetvis ett genmäle och en granskning av hans argument.

Fischers huvudskäl är att **ᚱ**-runan inte kan vara en **h**-runa, eftersom den formmässigt sammanfaller med en variant av den äldre runradens **d**-runa. En sådan **h**-runa skulle alltså vara omöjlig att använda, så länge **d**-runan var i bruk. Han ansluter sig här till Michael Schultes förslag att övergången från den äldre till den yngre runraden har skett gradvis och över en längre tid (Schulte 2009).

Hur förhåller det sig då med Finstastenens runor? Vad man säkert kan konstatera är att ristaren använder två runformer som aldrig förekommer i det äldre systemet: **ᚠ** **o** och **ᚡ** **a** samtidigt som **t**-runan i ...**ᚱontr** [s]tændr uppenbarligen används för två olika ljud: /t/ och /d/. Det rör sig alltså helt klart om en inskrift ristad med den yngre, 16-typiga runraden. Dessutom förekommer en äldre variant av **m**-runan (**ᚱ**). Denna runa uppträder som bekant på de äldsta danska runstenarna med yngre runor, t.ex. Helnæstenen på Fyn (DR 190), och det verkar alltså rimligt att Finstastenens tillhör ungefär samma tid som dessa. Dessutom kan det noteras att **r**-runan ännu används efter dental konsonant i ...**ᚱontr** [s]tændr, vilket är ett ålderdomligt drag.

Runan **ᚱ** har två huvudstavar och måste följaktligen motsvara en runa ur det äldre systemet, eftersom tecknen i den yngre runradens bara har en huvudstav. Formmässigt svarar den aktuella runan mot den **d**-runa som förekommer på baksidan av Rökstenen (Ög 136) i de två rader som är ristade med äldre runor. Detta omöjliggör enligt Fischer att **ᚱ**-runan på Finstastenens skulle kunna vara en **h**-runa. Han menar nämligen att det aktuella partiet på Rökstenen inte skall uppfattas som ett chiffer, utan som "en lärd uppvisning"

där man har använt sig av ”äldre allografer” som i den 16-typiga runraden hade ”underordnats som passiva, vilande allografer”. Ett stöd för denna uppfattning söker han i utformningen av runorna **ᚠ** **α** och **ᚢ** **i**, som han menar båda återgår på den äldre runradens **j**-runa. Att **ᚢ** på Rökstenen kan ha sitt ursprung i denna runa har tidigare förslagits av bl.a. Erik Brate och Aslak Liestøl, men med andra skäl. Liestøl (1978–81, 257 f.) menade exempelvis att det hade skett en förskjutning i ristarens uppfattning av det äldre systemet, där **j**-runan intagit **i**-runans plats i runraden och följaktligen också dess namn (fvn.) *í*ss. Enligt Fischer rör det sig i stället om ”en **j**-runa som används för **i**, precis som **e** används för **i**”. En sådan förklaring kräver nog att man samtidigt antar ristaren har varit medveten om att runan en gång hade haft namnet **jāra* med initialt /*j*/, trots att det är just bortfallet av detta ljud i runnamnet några hundra år tidigare, som är skälet till att **j**-runan började användas som tecken för /*a*/. Sannolikheten för ett sådant antagande måste väl betraktas som rätt liten, och svär dessutom mot Fischers egen idé om hur tämligen olikartade runor har fått nya ljudvärden just genom förändringar i runnamnen. Den vedertagna uppfattningen att de äldre runorna på Rökstenen används som ett chiffer, där man i huvudsak har valt de runformer som inte fanns i den yngre runraden samt nyskapat några, är nog att föredra. Det går då inte att påstå att **d**-runan här är multifunktionell. I stället skall den (som man i regel har gjort) dechiffreras med den yngre runradens **t**-runa, vilken i sin tur står för /*t*/ och /*d*/. Rökstenen kan alltså inte användas som ett exempel på att ”runorna **d** och **t** bägge betecknar /*d*/ i en och samma inskrift”, eftersom exemplen uppträder i olika delar av inskriften, som utgörs av två olika runsystem.

Det är också värt att notera att det inte är de ordinära runorna ur den äldre futharken som möter på Rökstenen, utan raderna innehåller flera specialvarianter som aldrig förekommer under urnordisk tid. Orsaken är rimligtvis att det rör sig om en lönnskrift, som inte skulle vara läsbar för vem som helst. En del av dessa runformer har senare dykt upp i vikingatida amulettinskrifter från Hovgården på Adelsö i Uppland (U NOR1994;26A) och i Gorodišče vid Novgorod i Ryssland (Melnikova 2001, 180–88, 490). Dessa inskrifter har ännu inte fått någon övertygande tolkning, men de kan mycket väl innehålla ett chiffer av liknande typ.

Vad Fischer även väljer att bortse ifrån är att **d**-runan på nordiskt område aldrig ser ut som på Rökstenen. Den enarådande formen i inskrifter med den äldre runraden är i stället **ᚩ**, där de korsande bistavarna ansluter till basen och toppen av huvudstavarna (se Odenstedt 1990, 119–21). Det har ibland påståtts (Antonsen 1974, 9; Odenstedt 1990, 121) att varianten **ᚩ** skulle finnas på Grumpanbrakteaten (Vg 207), men det är en gammal

missuppfattning. Runans bistavar ansluter till huvudstavarna en bit ovanför baslinjen, vilket ger runan ett \mathfrak{M} -liknande utseende (Svärdström i *SRI*, 5: 376), men det råder knappast något tvivel om att det är varianten \mathfrak{M} som är avsedd (se fotografier i *SRI*, 5: pl. 167). Det bör också noteras att den urnordiska **d**-runan i den mån den förekommer i vikingatida sammanhang verkar ha behållit samma form. Det tydligaste exemplet är inskriften med kortkvistrunor från Ingelstad i Östergötland (Ög 43), där \mathfrak{M} av allt att döma används som en ideograf för namnet *Dagr*.¹ Varianten \mathfrak{M} är däremot typisk för de anglosaxiska runinskrifterna, där den dock först verkar tas i bruk efter ca 650 (Odenstedt 1990, 121).

Det är här intressant att anknyta till Elmer H. Antonsens bekanta grafematiska analys av den äldre runraden (Antonsen 1975, 6–10). De runor som har två huvudstavar, väljer han att klassificera dels utifrån antalet bistavar, dels efter var dessa ansluter till huvudstavarna i tre olika positioner. Resultatet blir det som återges i (a) i fig. 1. Som Antonsen (1975, 9) har visat möjliggör den tomma positionen [två huvudstavar, två bistavar, mitten] att **d**-runan också kan anta en form där bistavarna har förskjutits mot runans mitt (b), vilket sedermera sker på framför allt anglosaxiskt område. Om vi däremot håller oss till Norden, där **d**-runan alltid har särdraget [botten], borde en **h**-runa med dubbla korsande bistavar också skulle kunna fylla denna position utan att systemet äventyras (c). Ur grafematisk synvinkel finns alltså inga hinder för att en graftyp \mathfrak{M} motsvarande grafemet ⟨h⟩ skulle kunna uppkomma redan inom den äldre runraden. Jag måste här understryka att jag inte menar att det har varit så, men att den teoretiska möjligheten finns.

I den yngre runraden fanns som bekant ingen **d**-runa och här borde risken för förväxlingar med andra runor ha varit minimal. På Helnæsstenen och i liknande inskrifter från tidig vikingatid finns bara två runor med dubbla bistavar, \mathfrak{M} och \mathfrak{H} . När vi på Finstastenen möter ett system om 16 runor, som innehåller två runor med dubbla bistavar, där den ena är \mathfrak{M} och den andra \mathfrak{M} , så ligger det nära till hands uppfatta den senare som en variant av runan **h** och inte som en runa hämtad från ett annat teckensystem.

¹ Ett \mathfrak{M} -format tecken är också känt som graffiti på arabiska silvermynt i svenska och ryska skattfynd. Den publikation som finns av det svenska materialet (Hammarberg och Rispling 1985) är dock enligt James Knirk (2006, 176 not 8) inte pålitlig, vilket gör att det inte kan läggas till grund för några resonemang. Däremot skall inskriften på en dirhem från ett skattfynd från Timerevo, daterad till den första hälften av 800-talet, enligt Knirk (pers. komm.) verkligen läsas som **gud** (se Melnikova 2001, 128, 436, 489). Det bör dock noteras att denna inskrift i så fall genomgående består av äldre runor och alltså inte utgör en blandning av det äldre och yngre systemet. Runan **d** har här formen \mathfrak{M} .

	(a)	(b)	(c)						
	1 bistav 2 bistavar	1 bistav 2 bistavar	1 bistav 2 bistavar						
Topp	<table style="width: 100%; border-collapse: collapse;"> <tr> <td style="border: 1px solid black; padding: 5px;">M e</td> <td style="border: 1px solid black; padding: 5px;">M m</td> </tr> </table>	M e	M m	<table style="width: 100%; border-collapse: collapse;"> <tr> <td style="border: 1px solid black; padding: 5px;">M e</td> <td style="border: 1px solid black; padding: 5px;">M m</td> </tr> </table>	M e	M m	<table style="width: 100%; border-collapse: collapse;"> <tr> <td style="border: 1px solid black; padding: 5px;">M e</td> <td style="border: 1px solid black; padding: 5px;">M m</td> </tr> </table>	M e	M m
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Fig. 1. Grafematisk analys av några teckenformer i den äldre runraden baserad på Antonsen (1975)

Längst till vänster i den tredje raden på Finstastenen finns en skadad runa som består av den övre delen av en huvudstav samt början av en bistav snett nedåt vänster. Jag tror att detta kan vara resterna av ytterligare en runa med formen \mathfrak{M} (ett förslag som ursprungligen kommer från ett av de ovan nämnda tentamenssvaren). Fischer anser i stället att det rör sig om en **h**-runa med formen \mathfrak{H} och ser här ett starkt argument för sin läsning av \mathfrak{M} som en **d**-runa. Problemet är att brottkanten går på ett sådant sätt att det är omöjligt att avgöra vilken form runan har haft. Såväl \mathfrak{M} som \mathfrak{H} (och den gamla läsningen $\uparrow \mathfrak{a}$) är formellt möjliga.

I min artikel från 2007 gjorde jag en jämförelse mellan Finstastensens \mathfrak{M} -runa och ett tecken med motsvarande typ på en vikingatida amulett från Staraja Ladoga, som både har uppfattats som en **d**-runa och **h**-runa. Eftersom inskriften annars i huvudsak består av spegelrunor drog jag slutsatsen att det låg närmast till hands att tolka runan som en variant av **h**. Enligt Fischer rör det sig här om en chifferinskrift och han menar att denna runa därför inte kan jämföras med den som förekommer på Finstastenen. (Principerna för detta chiffer uppges McKinnel, Simek och Düwel (2004) ”med all tydlighet” ha klargjort, men vad jag kan se har de inte några som helst resonemang om runorna på amuletten, se vidare a.a., 64f.) Att \mathfrak{M} -runan skulle ha sitt ursprung i en spegelform är dock bara en av de möjligheter som jag laborerade med i min artikel 2007. Jag föreslår där även att det kan röra sig om en dekorativ variant inspirerad av den urnordiska \mathfrak{M} -runan eller om en övergångsform mellan den äldre och yngre runradens **h**-runor (Källström 2007, 53). Det senare skulle ge en mycket tilltalande bild av en möjlig utveckling: \mathfrak{H} , $\mathfrak{H} > \mathfrak{M} > \mathfrak{M}$, och detta är faktiskt mitt huvudförslag till förklaring av Finstastensens \mathfrak{M} -runa.

Det kan här säkert invändas att det är märkligt att en sådan övergångsform bara skulle vara belagd en enda gång, men tittar man lite närmare på hur många **h**-runor vi säkert känner från äldsta vikingatid, så inser man att materialet är mycket begränsat. I den danska Helnæsgruppen finns runan

belagd i fyra inskrifter: DR 190 Helnæs, DR 248 Snoldelev, DR 250 Høje Tåstrup samt skalltaket från Ribe (DR EM85;151). Sammanlagt rör det sig bara om sex förekomster av detta tecken, men med nedslag som täcker ett område från Sydjylland i väst till Själland i öst. Det är samtidigt intressant att notera att runans form växlar. På skalltaket från Ribe (Sydjylland) och Høje Tåstrup (Själland) lutar bistaven mot höger (H), medan den på runstenarna från Helnæs (Fyn) och Snoldelev (Själland) lutar åt vänster (H). Den växlande riktning som bistaven har i den äldre futharken (se t.ex. Odenstedt 1990, 55 f.) består alltså även under äldsta vikingatid. Från nutida svenskt område känner vi endast ett par inskrifter med **H**-runor som av typologiska skäl kan misstänkas tillhöra detta tidsavsnitt. Den ena utgörs av Rävsaalstenen i Bohuslän (Bo KJ80), som bör läsas **haripulfs • staina[R]** och som utan tvivel skall uppfattas som en inskrift av Helnæstyp.² Det andra exemplet utgörs av brynet från Borgholm i Råpplinge socken på Öland (Öl NOR2003;26), där inskriften av Henrik Williams (2005) övertygande har lästs som **· Hain ·**. Runan **H** har i båda inskrifterna formen H, även om den i det senare fallet är något skadad.³

Det bör här även noteras att runan för ⟨a⟩ uppträder i olika former i den äldsta gruppen av danska inskrifter med yngre runor. På skalltaket från Ribe och stenar som DR 189† Avnslev, DR 250 Høje Tåstrup, DR 356 Sølvesborg och DR 333 Örja förekommer endast den äldre formen **†** som brukar transkriberas med **A**. Ristaren av Helnæsstenen (DR 190) har däremot genomgående använt den yngre varianten **† a**. Dessutom finns några fall där **† A** och **† a** står vid sidan av varandra (DR 191 Sønderby, DR 192 Flemlöse 1 och DR 248 Snoldelev). I denna växling av teckenformer som H och H resp. **†** och **†** är det väl egentligen inte särskilt otroligt att **H**-runan någon gång kan ha antagit formen H, vilket i sin tur kanske har fött idén att förenkla H, H till just **†**. Denna graftyp var ju efter att **†**-runan hade blivit en etablerad form för ⟨a⟩ överflödigt i det yngre systemet och kunde därför ges en ny funktion.

Bristen på fynd gör det svårt att följa denna utveckling i detalj. Extra besvärligt är det på nutida svenskt område, där inskrifter med långkvistrunor är extremt ovanliga under vikingatidens två första århundraden. När Fischer postulerar att Finstastenens teckenuppsättning är ett resultat av en lokal utveckling från det äldre systemet utan kontakt med den danska

² Den femte runan kan inte läsas som något annat än **p** och den **w**-läsning som har figurerat i diskussionen om denna inskrift kan uteslutas, se Gustavson 1996. Den nu skadade runan på slutet kan också suppleras efter äldre läsningar, som entydigt anger att den har varit **h** (se Boije 1884, 265)

³ I chifferavsnittet med äldre runor på Rökstenen finns däremot både varianten H och H.

Helnæsgruppen, bortser han helt från den isolerade ställning som denna inskrift har i Mälardalen. På de mycket få runstenar som är kända från området under detta tidsavsnitt är kortkvistrunorna det dominerande teckensystemet (se karta i Källström 2012a, 38). Förutom Finstastenen finns det endast en runsten med långkvistrunor som kan misstänkas tillhöra äldre vikingatid, nämligen U 10 från Dalby på Adelsö (se Nordén 1943, 219–23).⁴ Stenen saknar dock belägg för ⟨h⟩ eller ⟨m⟩, så vi vet inte hur dessa runor har sett ut i denne ristares repertoar.

Helnæs-Gørlev har länge varit en knäsatt beteckning för den äldsta gruppen av danska runstenar, men som Marie Stoklund har argumenterat för i flera sammanhang (t.ex. Stoklund 1996, 200; 2010, 244 f.) borde man egentligen skilja mellan inskrifter av Helnæstyp, där äldre teckenformer som **ᚠ** **A**, **ᚱ** **H** och **ᚱ** **M** ännu förekommer, och inskrifter av Gørlevtyp, där runan för ⟨a⟩ konsekvent återges med **ᚠ** och där ⟨h⟩ och ⟨m⟩ motsvaras av formerna **ᚠ** och **ᚱ**. I sitt senare arbete använder Stoklund beteckningarna Ribe-Snoldelev respektive Gørlev-Malt för de båda grupperna. Till den första kan räknas ett tiotal inskrifter, medan den senare förutom av Gørlevstenen (DR 239) endast omfattar DR 211 Nørre Nærrå samt den sent tillkomna Maltstenen (DR NOR1988;5). Dessutom finns en liten restgrupp som på grund av brist på utslagsgivande runformer inte entydigt kan föras till någon av grupperna. Hit hör bl.a. den nyfunna Faaborgstenen på Fyn som endast utgörs av mansnamnet **oslakr** *Āslakr* (Imer 2010, 149 f.).

Finstastenen svarar givetvis i första hand mot inskrifter av Helnæstyp, vilket jag tyvärr inte påpekade särskilt i min artikel 2007, men som jag senare har framfört muntligt i olika sammanhang där stenen har diskuterats. När Fischer förnekar att det skulle kunna finnas några beröringspunkter mellan Finstastenen och den tidiga gruppen av danska runstenar hänvisar han ofta till Gørlevgruppen, vilket givetvis ger en skev bild. De flesta av hans övriga invändningar är också irrelevanta. Att den graftyp som svarar mot ⟨a⟩ på Finstastenen har formen **ᚠ** **a** och inte **ᚠ** **A** spelar ingen roll, eftersom **ᚠ** och **ᚠ** kan stå vid sidan av varandra i samma inskrift. Dessutom har redan Helnæsstenen som nämnts genomgående **ᚠ** för ⟨a⟩. Varför man nödvändigtvis måste förvänta sig närvaron av chiffer motsvarande Gørlevstenens **ᚱkmiissssttiilll** för att tänka sig ett samband med den danska gruppen har jag svårt att förstå, men jag vill passa på att påpeka att Finstastenen är ett fragment och att vi därför bara har en del av

⁴ Möjligt skall också ett litet runstensfragment med teckenformerna **ᚱ** och **ᚠ** från Björkö i samma socken (SHM inv.-nr 14926:1; U 9, *SRL*, 6: pl. 3) räknas hit. Inskriften står i ett rakt textband, och fragmentet skall enligt uppgift ha påträffats i Svarta jorden i Birka, vilket talar för en datering till tiden före ca 970.

inskriften. (Gørlevstenen 2 som Fischer anför hör inte alls hit, eftersom den har daterats till 1000-talet, se vidare Moltke 1985, 239 f.)

Inte heller det allittererande *stændr stæinn*—det enda som vi faktiskt med absolut säkerhet kan tolka på Finstastenen—pekar enligt Fischer på ett samband med den danska gruppen, trots att det ordagrant förekommer på två av dessa stenar: [...--**oṛstain** | **sasi**] ... *stændr stæinn sāsi* (DR 189†), **aftruulfstotr** | [**st**]**ainsasi** *Aft Rōulf stændr stæinn sāsi* (DR 192; jfr också **stotr** : **aftfiri** *Stændr aft Firi(?)* på DR 333). I stället vill han finna de närmaste parallellerna till Finstastensens formulering i Ällerstadstens (Ög KJ59) **raisidoka stainar** och Rökstensens bekanta ingress: *Aft Vamōð standa rūnar þar*. Dessa påminner ju något om formeln på Finstastenen, men nog borde väl en ordagrann överensstämmelse tyda på ett närmare samband än det som bara erinrar. Rökstensens ingress har dessutom subjektet *rūnar*, inte *stæinn*, vilket tyder på att den är sekundär och anpassad till att verbet i följande sats inte är *gæva* eller *ræisa*, utan *fā* 'skriva; måla'. I botten ligger förmodligen en formel motsvarande den på DR 192. Slutligen skriver Fischer att det inte finns några belägg i Uppland för "Helnæsgruppens maskros-**m** eller Gørlevgruppens **o**-runa". Nu är det ju så att Fischers "maskros-**m**" dvs. **Ṁ**-runan är den som karakteriserar just Gørlevgruppen (Helnæs har som bekant **Ṁ**) och att **o**-runan har samma form (**ṫ**) i båda grupperna. Finstastenen bör som nämnts i första hand jämföras med Helnæsgruppen och någon **Ṁ**-runa är därför inte att förvänta i denna inskrift. Däremot stämmer det att vi inte har några exempel på **Ṁ** och **ṫ** i Uppland från 800- och 900-tal, men det kan bero på att det bara finns ytterligare en enda sten med långkvistrunor från det aktuella tidsavsnittet i hela detta landskap, nämligen den ovan omtalade U 10 från Adelsö.⁵ Inskriften har inte kunnat tolkas i sin helhet och innehåller som nämnts ingen **m**-runa. Däremot förekommer intressant nog en **o**-runa med samma form som på Finstastenen (**ṫ**).

Jag diskuterade denna runform på Finstastenen i min artikel 2007 och framhöll då att detta var det enda drag som egentligen avvek från den danska gruppen (Källström 2007, 53). I senare inskrifter med långkvistrunor har **o**-runan vanligtvis formen **ṫ**, men jag pekade på att varianten med dubbelsidiga bistavar förekommer i relativt ålderdomliga inskrifter med kortkvistrunor, som på en av runpinnarna från Hedeby (DR EM85;371A), Slakastenen i Östergötland (Ög 117) och Hogastenen i Bohuslän (Peterson 1992). Det verkade därför inte omöjligt att denna runform skulle kunna

⁵ Det bör dock nämnas att denna runform finns belagd en gång i Södermanland, nämligen på den ålderdomliga Kolundastenen (Sö 113) i Stenkivista socken. Inskriften består här till övervägande del långkvistrunor, men **s**- och **r**-runorna är av kortkvistyp.

uppträda i en inskrift som ännu hade behållit ett par runor av äldre typ. Att samma runform också finns på U 10 talar för ett samband mellan dessa stenar. Den säkert tolkade delen av inskriften (**af**tubisontistajnsar *Aft Öpi standi stæinn sār*) anknyter till formlerna på Finstastenen och den danska gruppen. Däremot framgår det klart av runformerna att U 10 måste härröra från en annan runristare. Exempelvis har **s**-runan en klart avvikande form — den är genomgående bakvänd och relativt smal — och **r**-runan är av kortkvisttyp (1).

Vi saknar ännu många upplysningar om de två första århundradena av den yngre runradens utveckling. Materialet är förhållandevis litet och ett nytt fynd kan mycket väl förändra den bild som vi har just nu. Samtidigt finns överensstämmelser som sträcker sig över stora avstånd, inte minst beträffande antalet tecken och vilka grafem som finns representerade i de olika varianterna av den vikingatida runraden. Intressant är att runformer som man normalt associerar med den äldre runraden inte bara förekommer i den danska Helnæsgruppen utan också i vissa inskrifter med kortkvistrunor. Ladogableckets spegelrunor är till största delen baserade på denna variant av den yngre runraden, men ett par av tecknen verkar hämtade från det äldre systemet: **ᚱ** och **ᚲ**. Dessa borde om speglingsprincipen är konsekvent genomförd motsvara **h** resp. **m**. Inskriften på Sparlösastenen (Vg 119) har karakteriserats som en blandning av långkvist- och kortkvistformer, där två — ***a** och **ᚱ m** — har behållit den form som de hade i den äldre runraden (vid sidan av de yngre formerna **ᚠ**, **ᚡ** och **ᚢ**). Tyvärr finns inte någon graftypp för ⟨h⟩ bevarad i denna inskrift, men eftersom ***a** används för ⟨a⟩ har runan rimligtvis haft den äldre formen med två huvudstavar. Ett par **m**-runor har även antagits på den bekanta Birkaamuletten (Nordén 1937, 160–68), vilket Fischer nu anser sig kunna avfärda efter att ha studerat fotografier. Läsningen av dessa tecken har tidigare delvis ifrågasatts av Lisbeth Imer (2007 ”Tekst”, 226 f., ”Katalog”, 20), och det är inte otänkbart att de **ᚱ**-lika formerna kan ha uppkommit genom korrigeringar av felristningar (egen granskning av originalet 2009). Jag tror dock inte att båda **m**-läsningarna helt kan avskrivas, utan att denna — faktiskt ännu otolkade inskrift — kräver ytterligare undersökningar. På Ulvsundablecket från Bromma, som Fischer av någon anledning också nämner, har man däremot aldrig läst någon runa av denna typ. Inskriften består nämligen uteslutande av kortkvistrunor (se den referens han anför, eller hellre den mer utförliga behandlingen i Nordén 1943, 146–54 samt pl. 2).

Även om det endast rör sig om ett fåtal inskrifter är det värt att notera att det just är några speciella runor av äldre typ som förekommer: **A**, **H** och **M**, dvs. runor som fortfarande ingick i grafemuppsättningen i den yngre runraden.

De helt säkra exemplen på inslag av runor som saknade motsvarigheter i den 16-typiga runraden är däremot lätt räknade. Egentligen rör det sig bara om Ingelstadhällens **ᚱ**-runa, som också förekommer i en speciell funktion (som ideograf för namnet *Dagr*). Detta är en allvarlig invändning mot Fischers idé om existensen av ”passiva, vilande allografer” från den äldre runraden under vikingatid.

Vän av ordning invänder här att jag medvetet måste ha undanhållit ett exempel. Finns det inte en **d**-runa av äldre typ på Roesstenen (G 40) på Gotland och har inte denna runa samma form som på Rökstenen? Det stämmer att en sådan runa finns, men kan vi vara säkra på att det just är en **d**-runa? Denna inskrift återges ofta som **iupin : udrrak** : (t.ex. i Krause och Jankuhn 1966, 235), men identifieringen av de enskilda runtecknen är långt ifrån entydig. Det är till och med osäkert om det rör sig om en inskrift med äldre eller yngre runor. Läser man den som tillhörande den 24-typiga runraden får man en kombination av teckenformer som jag tror annars är okänd och där en ålderdomlig variant som **ʝ r** får samsas med novationer som *** a** och **ʝ k**. Den tolkning som normalt brukar anföras ”Denna häst drev Udd” (se *SRI*, 11: 55f.) förutsätter inte bara en oväntad ordföljd, utan också en del unga språkformer. Roesstenens inskrift skulle dock även kunna läsas som yngre runor och innehåller i så fall en kombination av runformer som har sin närmaste motsvarighet i Helnæsgruppen (och då i första hand på Snoldelevsstenen DR 248), en möjlighet som redan Carl Marstrander (1952, 171f.) har noterat. Det är då också motiverat att pröva om inte **ᚱ**-komponenten i bindrunan faktiskt skulle kunna vara en **h**-runa. Läst på detta sätt får inskriften följande lydelse: **iupia : humrak** :. Någon helt självklar tolkning av de uppkomna runföljderna anmäler sig visserligen inte, men fördelningen mellan vokal- och konsonantrunor gör att teckenföljden mycket väl kan täcka en utsaga på nordiskt språk.

I den senare delen av sin artikel vänder Fischer blicken mot kontinenten, där han tror sig finna paralleller till skriftsituationen i Norden. På det runristade spännat från Freilaubersheim (KJ 144) i sydvästra Tyskland förekommer två **d**-runor med formen **ᚱ**. Dessa har länge varit unika i det epigrafiska runmaterialet från kontinenten (Odenstedt 1990, 121), men enligt Fischer skall en motsvarande **d**-runa även finnas på ett runristat silverbeslag från Eichstetten. Man får väl anta att denna uppgift grundar sig på en egen undersökning, eftersom tidigare publicerade läsningar inte upptar någon **d**-runa från denna inskrift (jfr t.ex. Sasse, Alt och Hollack 2001, 80 f.; Graf 2010, 156–62). Det intressanta med fynden är att denna variant av **d**-runan här ser ut att ha en tidigare datering än de anglosaxiska motsvarigheterna.

Med utgångspunkt i framför allt en handfull nyupptäckta griffelinskrifter

med runor på fornhögtyska samt de uppteckningar av runraden som sedan gammalt är kända från åtskilliga kontinentala manuskript hävdar Fischer att det har existerat ”en bred folklig tradition” av runkunnande i Sydtykland under 800-talet. Han menar också att den runrad som uppträder i en variant av den s.k. Isrunatraktaten i handskriften St. Gallen Stiftsbibliothek 270 (Derolez 1954, 90–94) inte är av anglosaxiskt ursprung, som man tidigare har trott, utan ”en fornhögtysk runrad som utökats med inlånade anglosaxiska grafer”. Förvånade i sammanhanget är att den som satte ihop denna runrad (på några ställen när) lyckades placera in de ”inlånade” tecknen på exakt de platser där de brukar stå i den anglosaxiska runraden. Runornas namn är i viss utsträckning som i fallet *Tag* för **d** anpassade efter det fornhögtyska ljudsystemet, men det råder väl knappast något tvivel om att de är av anglosaxiskt ursprung, eftersom minst hälften har behållit sina anglosaxiska namn oförändrade (Derolez 1954, 130 f.). I den redaktion av runraden som finns i St. Gallen Stiftsbibliothek 270 har namnen på **f** och **f̄**-runorna dessutom blivit förväxlade, så att **f** (motsvarande den anglosaxiska **æ**-runan) heter *ac*, och **f̄** (motsvarande den anglosaxiska **a**-runan) har fått namnet *asc* (< forneng. *æsc*). Om det här handlade om ett levande inhemskt teckensystem som supplerats med tecken från det anglosaxiska runalfabetet hade man nog väntat sig lite mera ordning och kanske till och med att en så central runa som **f** skulle uppträda med ett fornhögtyskt namn som kunde ledas tillbaka på germ. **ansuz*. Eftersom en del av runnamnen har förtydskats, så har både runraden samt den alfabetiskt ordnade varianten begåvats med dubbla tecken för vissa ljud, exempelvis anges både **↑** och **⚡** kunna stå för /t/.

Detta kan kanske vid första påseende uppfattas som en parallell till att både **↑** och **⚡** skulle ha använts för /d/ på Finstastenen, men är de egentligen jämförbara? Handlar det inte om två olika skriftsystem som befinner sig i två helt olika faser och är på väg åt olika håll? I Norden hade den 24-typiga runraden reducerats till en runrad med endast 16 tecken, där många av tecknen var mångfunktionella (t.ex. **↑** som tecken för /t/ och /d/). I den nämnda St. Gallenhandskriften handlar det om ett inlånat anglosaxiskt runalfabet, där översättningen av en del av runnamnen har resulterat i att runtecken med olika utseende och ursprung plötsligt har fått ett och samma ljudvärde (**f** och **f̄** för /a/, **f̄** och **⚡** för /o/, **↑** och **⚡** för /t/ etc.). I Norden var de 16 tecknen egentligen för få för att täcka ljuden i språket, i den kontinentala handskriftstraditionen har de i stället blivit alldeles för många.

Isrunatraktaten rör i huvudsak olika typer av lönnskrift och det är väl troligt att runraden (och framför allt den alfabetiskt ordnade varianten) här hade samma funktion. De rätt fåtaliga runskrivna griffelinskrifterna samsas i handskrifterna med hundratals liknande inskrifter med latinska

bokstäver där olika typer av lönnskrift har tillämpats (se Nievergelt 2009, 17–22, 77–187). Nog förefaller det rimligare att det är i denna egenskap som runorna kommit till användning i detta sammanhang. Griffelinskrifterna må vara skrivna på folkspråket, men de har tillkommit i en klostermiljö och är knappast uttryck för ”en bred folklig tradition”, som Fischer vill göra det till. Något bidrag till diskussionen om övergången från den äldre futharken till den vikingatida runraden i Norden ger de väl egentligen inte.

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Bracteates and Runes

Review article by Nancy L. Wicker and Henrik Williams

Die Goldbrakteaten der Völkerwanderungszeit — Auswertung und Neufunde. Eds. Wilhelm Heizmann and Morten Axboe. Ergänzungsbände zum Reallexikon der Germanischen Altertumskunde 40. Berlin and New York: Walter de Gruyter, 2011. 1024 pp., 102 plates. ISBN 978-3-11-022411-5, e-ISBN 978-3-11-022411-2, ISSN 1866-7678. 199.95 €, \$280.

From the Migration Period we now have more than a thousand stamped gold pendants known as bracteates. They have fascinated scholars since the late seventeenth century and continue to do so today. Although bracteates are fundamental sources for the art history of the period, and important archaeological artifacts, for runologists their inscriptions have played a minor role in comparison with other older-futhark texts. It is to be hoped that this will now change, however. If so, it will be thanks largely to those German runic scholars who during recent decades have dedicated themselves to studying inscriptions on bracteates.

Due to continual increase in the material, bracteate corpuses have been assembled repeatedly. In the first universal compilation of runic inscriptions, Johan Liljegren (1833, 255 note b) mentions that over twenty have been found but that their runelike symbols are of unknown character and content. This was, of course, before the decipherment of the older futhark. The most recent bracteate inventory is that of *Die Goldbrakteaten der Völkerwanderungszeit: Ikonographischer Katalog* (hereafter *IK*), parts 1–3, published in seven volumes 1985–89 under the auspices of the immensely productive Karl Hauck (1916–2007). The catalogue has been supplemented by a volume on production problems and chronology by Morten Axboe (*IK*, 4.1) in 2004, one by Alexandra Pesch on bracteate groupings in 2007 (*IK*, 4.2),

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and now the volume being reviewed,¹ which combines twelve independent contributions on bracteate matters with a catalogue and plates of new finds. Henceforth, we shall refer to the latest publication as *Auswertung*. As of December 2010 the total number of separate bracteate models is at least 622, representing no fewer than 1003 individual pieces (*Auswertung*, 893).

There is no doubt that the publication of *IK* is a tremendously valuable contribution to bracteate studies, not least through its careful descriptions and useful illustrations, the latter comprising photographs and drawings of every item. The volumes of *IK*, 1–3, are out of print, but they are available on the Internet (unfortunately not in OCR format, i.e. the text is only scanned as a picture, not searchable). The supplementary material presented in *Auswertung* forms a valuable addition, made more useful by the index of find-places included (pp. 1012–24), although a similar index of present repositories (usually museums), provided in previous volumes, is lacking.

In this review article we will concentrate on the iconography and archaeology of the bracteates (Nancy Wicker; pp. 152–82) and the texts thereon (Henrik Williams; pp. 183–207). Evaluation of the specific topics will be offered in each of the two sections.

Iconography and archaeology

Nancy L. Wicker

While this review, due to the nature of the journal in which it is published, focuses on the runic texts found on bracteates, one of the most important aspects of Hauck's project is its insistence that such texts should be considered as part of the artifacts on which they are located rather than being treated as independent entities — as has sometimes been the case. In addition, this corpus publication has underscored the importance of considering bracteates both with and without inscriptions, also in runic publications, which as a rule have ignored bracteates lacking texts. The larger context of bracteates embraces the iconography of pictorial details as well as the archaeological find circumstances of the pieces. Hauck was the leading figure in the iconographic analysis of bracteates and assembled an interdisciplinary team of scholars who shed light on aspects of bracteates that lay outside his own wide range of knowledge.

¹ Although the present volume is clearly identified as “IK 4,3” once in the bibliography (p. 808) the designation is evident nowhere else in the book itself.

The chapters of the volume that deal with iconography and archaeology cover the following:

1. Deeds of Óðinn: signs and codes of bracteate iconography and methods for their interpretation, by Karl Hauck (60 pp.),
2. The picture formulae of gold bracteates and their variants, also by Karl Hauck (92 pp.),
3. A history of bracteate research, by Charlotte Behr (77 pp.),
4. A network of “central places”: elite contacts and cooperation between early medieval centers in the light of the gold bracteates, by Alexandra Pesch (47 pp.),
5. The chronology of bracteates with inscriptions, by Morten Axboe (18 pp.),
6. Iconography, social context and ideology: the meaning of animal-ornamented shields in Early Anglo-Saxon England, by Tania Dickinson (52 pp.),
7. Catalogue description of new finds, and the catalogue of new finds, by Morten Axboe with assistance from Charlotte Behr and Klaus Düwel (109 pp.).

Bracteate iconography: deeds of Óðinn

The work begins with two chapters by Karl Hauck, even though Charlotte Behr’s history of bracteate research would provide a better pedagogical introduction. In fact, in both the English and German summaries (pp. 704 f., 687 f.), Behr’s chapter is placed before Hauck’s. Any reader unfamiliar with Hauck’s theories and his academic style should read Behr’s summary of bracteate research to gain some understanding of the field and obtain an overview of Hauck’s work before tackling his own contributions. Here, however, we will begin with Hauck.

Hauck’s first chapter in the volume, “Machttaten Odins: Die Chiffrenwelt der Brakteaten und die Methoden ihrer Auswertung” (pp. 1–60), is introduced by Pesch in the summaries where she explains that it was intended for inclusion in volume 12 of the *Reallexikon der Germanischen Altertumskunde* as a *Stand der Forschung* article, demonstrating his methodology for investigating bracteates; however, reasons unknown, it was never finished. Pesch completed the references and redid the plates (p. 705). The chapter is well illustrated with drawings so that the reader can follow Hauck’s close argumentation about the imagery, although the order of plates is sometimes haphazard. Thus a great deal of flipping back and forth is required to find

the appropriate images. Furthermore, the reader must remember that the drawings sometimes explicitly accentuate features that Hauck wants to emphasize — and, of course, drawings are never “objective”.

Hauck begins with three assumptions: (1) that bracteates have their origin in Late Antique Period images and texts, which are thus useful for interpreting bracteate iconography, (2) that in the absence of contemporary northern European sources, texts dating much later than bracteates, as well as other categories of material such as gold foils (Swedish *guldgubbar*), can be used to interpret bracteate images, and (3) that runic inscriptions on bracteates are connected to the images they bear. Although Hauck cites the importance of Northern aristocratic contacts with Late Antique culture and the background of bracteates in medallions and Germanic medallion imitations, he relies most heavily on the “Second Merseburg Charm” (see below) and Eddic sources to interpret specific imagery on bracteates. He maintains that the main themes of bracteates were healing, regeneration, and protection from evil, demonic forces. Their traditional material culturally linked the elite groups from the so-called central places of the North, and their images he considers to be “concretized” precursors of the later mythographic texts by Snorri Sturluson (p. 39). Whether one accepts Hauck’s specific interpretations or not, the bracteate corpus is a valuable source for approaching the oral culture of the North.

Hauck maintains that the main figure on Type C bracteates is Óðinn, basing this on the pair of birds that are depicted on four of sixty-eight bracteates with aviforms (the number known in 1995), which he identifies as the ravens Huginn and Muninn. In the more numerous cases where only one bird is shown, Hauck explains that an “abbreviation principle” is in force necessitated by the difficulty of depicting many images within the small size of the picture field. One bird can thus represent the pair. The images are indeed tiny, but the size argument becomes a crutch on which Hauck leans whenever an image is not as complete or detailed as he might wish. In his discussion of the transfer of Mediterranean topoi of power to Northern iconography, Hauck argues that over half of the impressed gold foils from Sorte Muld on Bornholm include the scepter of Jupiter (p. 10); yet the images are miniscule and indistinct in form, and no specific attributes of the supposed scepter can be discerned. Hauck also turns to Late Antique iconography to maintain that the “hand of power” of the divine emperor portrait is repeated on bracteates and indicates that the figure is a god. However, we should also consider that the hand might belong to an earthly ruler, since the Roman emperor was a secular leader as well as divine.

After establishing (to his own satisfaction) that the main figure on Type C

bracteates is Óðinn, Hauck interprets the various images as a series of events that foreshadow the death of Baldr, based on his proposed association of bracteate images with the ninth-century Old High German text known as the “Second Merseburg Charm”. Although this charm formed the basis for much of his early writing on bracteates, here (p. 4) he provides only a bare footnote to two of his earlier works (Hauck 1970 and 1992). In *Auswertung*, it is Charlotte Behr (p. 223) rather than Hauck who explains that the charm describes how Baldr’s foal sprained its foot, fell, and was healed by Óðinn with a magic spell. Hauck returned to the charm in several installments of “Zur Ikonologie der Goldbrakteaten” (articles with this subtitle and a serial number) and assumes that the readers of this volume are familiar with both the charm and its importance for his interpretation of bracteate images. Hauck claims that many Type C bracteates show Óðinn carrying out the ritual attested in the Merseburg charm, which consisted of blowing his healing breath into the horse’s ear and reciting words of healing, a practice that he traces to a fourth-century veterinary procedure for equine healing called subcutaneous air insufflation. Hauck’s use of these sources to interpret bracteates is criticized in detail by Kathryn Starkey (1999, 387–89).

Hauck proposes that various bracteates show different key points in the Baldr narrative described by the Merseburg charm and that a bracteate hoard discovered in a posthole at Gudme on Fyn in Denmark relates the entire Baldr story (*Auswertung*, 12–16). He interprets the Gudme set of nine bracteates as a necklace or collar, even though there is no evidence that the pieces were all displayed together and in spite of the fact they were not discovered resting in place on a body in a grave (cf. Hauck 1998a). Even the Type D bracteates, which do not exhibit humanoid figures, are woven into Hauck’s explication of the narrative. In summary, the parts of the myth common to bracteates and the “Second Merseburg Charm”, in Hauck’s interpretation, are:

1. the animal’s bent leg, which indicates that the foal is injured, as seen on several bracteates, including IK 106 Lilla Istad;
2. Type D bracteates (such as IK 455.2 Gudme), which show the role of a demon in causing the fall of Baldr’s foal;
3. the foal, which is shown falling (IK 392 Gudme) or even dead (IK 149 Skåne);
4. Frigg/Freyja (IK 391 Gudme), who arrives to assist the foal; this anomalous bracteate type, referred to as the Fürstenberg type by Mackeprang (1952, 103), depicts a woman *en face* and is otherwise found only further south, in Germany;

5. Óðinn, who carries out a ritual of blowing his healing breath into the horse's ear and recites words of healing, as depicted on many Type C bracteates.

I have challenged Hauck's interpretations on art historical grounds by pointing out that the so-called injured leg may be bent (or perhaps more accurately, gracefully curved) to fit into the available space of a round composition, and that likewise the mouth of the anthropomorphic head touches or is close to the animal's ear or neck for compositional reasons (Wicker 2003, 536). On a tiny bracteate, the man's mouth is necessarily placed near the horse's neck and the upswing leg is merely an elegant solution to the lack of space. Accepting all of the details as purposeful illustration of the Baldr story requires the reader to accept that each goldsmith making a bracteate knew exactly which part of the story of Baldr was to be depicted. In fact, Hauck does not really address the questions of how knowledge of specific iconography was disseminated and how artisans worked. Pesch (this volume, see below), however, deals to some extent with the way imagery as well as the actual dies for making images may have been spread.

To support further his belief in the existence of healing iconography on bracteates, Hauck focuses on a single example, IK 26 Börringe, that has the runic inscription **laukar** 'leek' located along the foreleg of the animal (*Auswertung*, 5). Relying on Wilhelm Heizmann's research on Old Norse literature and folk knowledge of medicinal uses of the leek, Hauck combines the occurrence of the name of the plant with the notion of healing on bracteates. He assumes that the inscription itself is connected to the healing of the leg and subsequently uses this example as one of the lynch-pins of his argument that runic inscriptions are linked to the images on bracteates. While the use of the leek in healing is well known and the interpretation of **laukar** as 'leek' universally accepted (cf. Starkey 1999, 390), I reject Hauck's insistence that the location of the inscription is related to the leek's medicinal efficacy. Besides appearing along the horse's leg, the inscription follows the perimeter of the gold disk, the typical location for inscriptions on coins and medallions, which were the models for bracteates. Thus, I would maintain that Hauck exaggerates the significance of the location of the **laukar** inscription. The word *laukar* appears on various bracteates in two places where Hauck claims that ravens (certainly birds, but are they necessarily ravens?) often occur (*Auswertung*, 6); yet these birds are also placed along the edge of the bracteate stamps — where inscriptions were typically placed. To substantiate his argument, Hauck further identifies a curious branched symbol on IK 571 Dannau as a leek plant that is placed

along the animal's leg (p. 5), even though it is highly stylized and most viewers would be hard pressed to recognize it as a plant. The reason Hauck focuses on **laukar** is apparently that he cannot identify other inscriptions that bolster his belief in the connection between text and image.

Besides interpreting many Type C bracteates in the light of the “Second Merseburg Charm”, Hauck connects certain Type B bracteates to the sacrifice of Baldr. These pieces depict three standing anthropomorphic figures, which Hauck calls “three gods”. The Late Antique model for these pieces is an image of the emperor with Victoria and Mars. Hauck proposes that Baldr, rather than Óðinn, is the central figure in the Northern version of this composition (p. 18), although in his view Óðinn is the main figure in most other bracteate types with anthropomorphic images. Hauck identifies six closely related variants of this scene that display different phases of the myth of Baldr's sacrifice, but to do so he often needs to invoke his “abbreviation technique” (pp. 18–22). He proposes that Loki is shown in a “skirt” similar to the one worn by the figure on the Fürstenberg-type bracteates (cf. IK 391 Gudme) because he disguises himself as an old woman while tricking Frigg into revealing the vulnerability of Baldr to mistletoe. The six variants include details such as:

1. Óðinn's ravens (one or two) in different sizes and forms (IK 51.3 Gudme, IK 51.1 Fakse, IK 39 Denmark, and IK 165 Skovsborg),
2. Høðr holding a stone to throw at Baldr (IK 165 Skovsborg),
3. Loki carrying the mistletoe (IK 51.3 Gudme, IK 20 Zagórzyn [Beresina], IK 165 Skovsborg, and IK 39 Denmark),
4. the mistletoe striking Baldr (IK 51.1 Fakse),
5. a demon's head below Óðinn (IK 39 Denmark),
6. the skirted (or kilted) Loki with wings (IK 51.3 Gudme and IK 51.1 Fakse).

Even though Hauck traces the form of the wings to the Roman numismatic prototype with Victoria, he compares this portrayal of Loki to the Viking Age “death angel” or helper reported by Ibn Fadlan (p. 21).

Other bracteates purportedly contain scenes (pp. 18–21) related to additional points in Snorri's story of Baldr in *Gylfaginning*. Hauck maintains that a piece with two standing anthropomorphic figures and a small animal, IK 6 Års, depicts Baldr and Óðinn with the former's dead foal. He further interprets a tiny dot on IK 50 Esrom Sø as a bracteate held in the hand of Baldr, who is accompanied by a larger figure interpreted as Óðinn, thus connecting this bracteate to Óðinn's healing of the foal. In a scene depicted on IK 101 Kongsvad Å, which he relates to the sacrifice of Baldr,

Hauck identifies the preparation of the mistletoe, and the detail does indeed resemble a stylized branch with berries but no leaves. A scene showing a man and woman side-by-side on the medallion imitation IK 86 Inderøy is interpreted by Hauck as a representation of Baldr's trip to Hel. On IK 79 Hjørunde, a small figure with arms and legs is identified as Baldr, who stands next to an enigmatic arrangement of three sets of lines placed at right angles to each other. These Hauck creatively identifies as the funeral pyre of Baldr. He then labels the ring held in the hand by several bracteate figures as Draupnir, which is placed upon the funeral pyre.

A detail Hauck returns to several times (pp. 7–9, 24 f., cf. also his second chapter, pp. 110–22), is a “small round object” that appears on various bracteates, sometimes large enough for Hauck to interpret it as a ring (IK 165 Skovsborg) and other times smaller so he sees it as a bracteate (IK 189 Trollhättan and IK 50 Esrom Sø). He maintains that the ring Draupnir was sometimes replaced in bracteate imagery by an amulet with a divine image (thus, a gold bracteate); there is, however, nothing to justify the assertion that this tiny dot specifically represents the image of a god. We should note that the “small round object” ranges from less than one millimeter in diameter to something as small as a dot, so any specific identification is purely speculative. Hauck presents the ring/bracteate as a symbol of regeneration.

Next Hauck attempts to demonstrate the existence of Ragnarøk representations in bracteate imagery. He begins with the portrayal of the Fenris wolf on the obverse of the unusual two-sided bracteate from IK 190 Trollhättan, where the wolf is shown biting the hand of Týr (p. 29 f.). Examining details on other bracteates that help him interpret these images, he notes that the figure on IK 250 Fure sports the same kind of hairstyle as Trollhättan. He also compares the frontal image on Trollhättan to the Fürstenberg-type bracteates that seem to show females, but he maintains that the garment on Trollhättan is a “kilt” as worn by Mars, thereafter pointing out that the Trollhättan image is abbreviated since it omits the wolf's fetters, which are to be expected in a scene that portends Ragnarøk. A second supposed Ragnarøk representation, on IK 166 Skrydstrup, consists of a crowded scene of six creatures (an anthropomorphic figure identified by Hauck as Óðinn, a bird, a “dead” horse, a stag, and two snakes) and two runic formula words; the piece is somewhat smaller, and in Hauck's view omission and simplification of details should be expected. He connects the word **laukar** here to the larger theme of regeneration (p. 31). In a final attempt to identify Ragnarøk imagery, he identifies the *en face* figure on Fure IK 250 as Óðinn, which he asserts is two-eyed here because this bracteate depicts the rebirth of the world after Ragnarøk (p. 33).

Hauck also tries to discover picture formula equivalents to the ritual and cultic names of Óðinn. He begins with IK 7 Års, where the anthropomorphic figure has a boomerang-like weapon and wears a double neck-ring (cf. *IK*, 3.1: 245 [pl. 129]), and the animal has a dotted horse-tail (*Auswertung*, 35). He points to the tail as characteristic of Óðinn's self-sacrifice (*Hávamál*, st. 138f.) and part of his ritual costume. In his second chapter in *Auswertung*, Hauck reports that he has found a similar tail on the IK 66 Gummerup bracteates and has "corrected" the drawing printed in *IK*, 1.3, where the tail was not included (p. 81f.). From the photographs published in *IK* and also some I have taken, it appears that there is a line of dots and a slight buckling of the thin golden disc — but no tail. Creative examples like the seemingly invented "tail" on IK 66 Gummerup do not inspire confidence in Hauck's methodology. Yet he uses the "tail" to make further assumptions, connecting it to what he calls Óðinn's "self-naming" as a horse in the runic *ek F[ā]kar* on the IK 340 Sønderby/Femø bracteate (p. 36). Before making general comments about this chapter, I will continue to Hauck's second chapter.

Oral tradition and picture formulae

Hauck's second chapter, "Die Bildformel der Goldbrakteaten in ihren Leitvarianten" (pp. 61–152), was written specifically for *Auswertung*, essentially completed in 1995, long before this volume appeared.² The contribution repeats a great deal of information from the previous chapter, referring to and illustrating many of the same examples and adding little that is substantially new. Rather than going over old ground, I will here focus on the additional data. Taken together, the two chapters are valuable as a distillation of Hauck's seminal works, which are not easily accessible (both hard to find and to read), but due to the myriad references to his own publications, those earlier works are still necessary if the reader wishes to see the first-hand evidence.

One of the few new aspects here is the insistence that early medieval illuminated manuscripts, especially the Carolingian Period (early-ninth-century) Stuttgart Psalter and Utrecht Psalter, preserve earlier pictorial traditions and can help us to interpret bracteate imagery, since both were borrowed from Late Antique iconography. In particular, the iconography of rulers, representations of lions and snakes, and flattened, non-illusionistic depictions of attributes and details are highlighted as a "picture reservoir"

² The subtitle of this piece is "(Zur Ikonologie der Goldbrakteaten, LV)". However, an article with this designation was already published in 1998 (Hauck 1998a).

of the oral culture of the North (p. 64). For example, Hauck maintains that the Late Antique *topos* of a horse lying on its back, intended to indicate that it is dead, was used in Carolingian psalters as well as on bracteates (p. 103). It is true that figures and objects are sometimes placed up against the edge — but not necessarily the “bottom” edge — on the small, round face of bracteates; it is, however, impossible to be certain whether a horse with its back along the side of a bracteate represents a dead animal or simply follows the curved edge of the piece. Drawing attention to the close connection between pictures and texts in the two psalters, Hauck suggests that bracteate images and inscriptions show a similar correlation (p. 65).

Hauck admits that Type A bracteates imitate Roman medallions and *solidi* closely but points out that they, in addition to being imitative, include details that can be connected to the larger themes of healing and regeneration. Here, and on other types of bracteates, he tries to tease out which picture details were borrowed from the South and which come from the North. For instance, in his view the Roman numismatic motif “Victoria crowning the Victor” (p. 76) provides a formal model for Type B bracteates showing “three gods”, yet the picture details are Northern. Further connections between the Roman world and the North are demonstrated by the use of the leek, which was employed for healing horses in the Roman Empire and about which Óðinn also had medicinal knowledge (p. 78). Furthermore, Hauck insists that — in emulation of the Roman numismatic practice of inscriptions that relate to images — the words and pictures on bracteates are also connected, as on the bracteate IK 26 Börringe discussed above, which has **laukar** written along the horse’s injured leg.

As in the previous chapter, the bracteates showing “three gods” are examined in great detail, and here reference is made to Late Antique gestures. Hauck discusses the importance of the Gudme/Lundeborg area for contact with the South and exposure to the idea that Roman coins showed images of gods and of the divine emperor (p. 80). The figure that Hauck identifies as Óðinn on this group of bracteates is connected formally and iconographically to numismatic and sculptural images of Mars, the war god (p. 83). In his view, this clinches the argument that Óðinn was the war god of the North (p. 89).

One of the details not discussed in great detail in the previous chapter is a scene from the final phase of Baldr’s sacrifice in which he stands on a stage or altar (IK 51.1 Fakse, IK 165 Skovsborg, IK 66 Gummerup, IK 39 Denmark), similar to the manner in which Mars stands on a platform on Roman coins. When the figure that Hauck identifies as Loki stands on the same kind of platform (IK 20 Zagórzyn), he proposes that it indicates a place

for a speaker (p. 106). Yet the “platform” on which Roman figures often stand is not an altar or specific structure but merely a ground line. It also separates the pictorial scene from the mintmark or *officina*, which indicates the place where the coin was minted.³

Hauck returns to the “small, round object” in this chapter and states that it is the iconographic equivalent of the gold ring Draupnir, which he considers a core symbol of the sacrifice of Baldr (p. 110). He proposes (pp. 121f.) that the ring as a symbol of regeneration, as shown on medallion imitations during the Late Roman Iron Age in Scandinavia, was replaced on Migration Period bracteates with an image of the gold bracteate itself (IK 51.3 Gudme and IK 189 Trollhättan, for instance), yet he conjectures that the ring and its symbolism returned after the bracteates’ relatively short span of popularity.

Summing up: Hauck’s contributions in these two chapters show a remarkable mind that was both creative and imaginative. It is unfortunate Hauck was not able to finish the text as he intended, but we do have the version that Alexandra Pesch has made available. These contributions condense a great many of his detailed proposals for the understanding bracteate iconography, but the chapters repeat one another extensively. Each is well illustrated, in fact, with many of the same illustrations. In a book of over 1100 pages, it is difficult to comprehend why the decision was made to include both.

Hauck’s detailed analyses exhibit impressive knowledge and deft control of the material. His arguments may impress many readers, but accepting his proposals requires great leaps of faith. There are major flaws in his work — in the assumptions made, the occasionally arbitrary handling of the material, the lack of concern with physical aspects of bracteates, and the unwillingness to consider contrary views. Here are some of my concerns:

1. One of the most surprising things for anyone who has read Hauck’s earlier work is that the “Second Merseburg Charm” is barely mentioned in *Auswertung*. Perhaps Hauck assumed that “everyone” knows about and accepts his claims that bracteates present visually what is preserved in this Old High German account (which was, as far as we otherwise know, totally unknown in the North). He does not even try to defend his premise here, and the assumption that his recognition of details of the charm in bracteate imagery is water-tight allows him to make further suppositions based thereupon. Hauck would like to use the bracteates to posit an early dating for the charm, but he also uses an early dating of the charm as evidence for Baldr in the fifth century, thus employing circular reasoning

³ For instance, browse *Roman Coins* (Kent 1978).

(1971, 142). In the early years of his research on bracteates (Hauck 1970, 403), he was apparently swayed by the proximity of Merseburg to Obermöllern (40 km distance) in his attempts to explain the anomalous imagery on the bracteate IK 132 Obermöllern, which he interprets as the horse falling after being injured (*Auswertung*, 4). Once Hauck had identified the supposed significance of the Merseburg charm for the story of Baldr, he attempted to correlate every dot and squiggle on bracteates with the text.

2. In his analysis of details Hauck sometimes mishandles the material. I have already discussed how he “improved” the drawing of IK 66 Gummerup to show a horse-tail, and he also mentions how improvement of the drawings for *Auswertung* allows him to emphasize certain details by depicting them individually beside or around the perimeter of the drawing of the bracteates themselves (pp. 81f.). These details include a spear, a sword, a bracteate in a hand, the ring Draupnir, an altar platform, whole and broken mistletoe, a woodpile, a dragon’s head, a small beast, a snake, a demonic reptile, and the god’s foot (pp. 51–58 and 133–52). Hauck may interpret the smallest mark as representing some element in a key Eddic passage. However, identifying a simple curve as an abbreviated bird makes a mockery of the “abbreviation principle” (p. 141, fig. 9.4). Hauck relishes the minute specificity of details, but either ignores the lack of substantive evidence or explains away as “abbreviations” any omission of details (such as one bird instead of two ravens).

Hauck admits that hoofs show great variation and can be mainly ornamental (p. 69), as on IK 147 Rynkebygård; yet at other times he insists that a particular hoof is “bent” (thus, injured), as on IK 106 Lilla Istad. Sometimes the vague features are built up into crucial elements of his analysis, as when a simple dot can become a bracteate with a “divine” image (see above). Determining which minor variations are iconographically significant and which are not can seem an arbitrary exercise. One of the objections I have to Hauck’s interpretations is that his readings are so fluid. Sometimes he identifies a bird as Óðinn’s raven but at other times he proposes that it is Loki in disguise as a bird obtaining the fateful mistletoe (p. 23); sometimes Óðinn is shown in a healing role (Type C bracteates) and sometimes as the war god (IK 7 Års). The critical reader begins to wonder whether Hauck’s unified interpretation of bracteate iconography can be relied on at all. To a certain extent, he undermines himself with forced attempts to make everything fit together and by insisting on the tiniest of details; if those elements in reality are absent, his argument collapses.

3. Hauck’s insistence that medallion imitations, bracteates, and gold foils all show divine pictures (p. 77) allows him for the most part to ignore

uses of bracteates in society other than as amulets focusing on healing and regeneration. What, for example, of ostentation, gifts, tribute, or wealth/inheritance? Even Michael J. Enright, a follower of Hauck, wonders (1988, 405) whether some consideration might not “be given to the social as well as to the religious reasons for wearing bracteates? ... an amulet not only says something about the religious beliefs of the wearer but may also say something noteworthy about social status and concepts of aristocratic display”. I would suggest that a closer investigation of the Late Antique tradition of wearing looped medallions might reveal more about how bracteates were used.

4. At times, Hauck ignores the practicalities of bracteate manufacture, for example by disregarding Axboe’s assessment (p. 82) that the “mark” Hauck interprets as a horse’s tail on IK 66 Gummerup is simply “residue” from the manufacturing process. Unlike Axboe (and the author of this part of the review), Hauck did not personally examine many bracteates, and he seems unaware or unable to accept that there are technical properties limiting how specific some details can be. Although he occasionally mentions the wearers or makers of bracteates, his work generally reflects an inward-looking world of ideas that has little connection to external factors.

5. Hauck’s insistence that all bracteates, Types A, B, C, and D, deal with a unified subject matter revolving around Óðinn and Baldr and a unified theme of healing and regeneration has stood largely unchallenged. He posits the codification of a belief system controlled by leaders based in central places that is not verifiable. Thomas DuBois (1999, 42) is adamant that “the non-Christian belief systems of the Nordic region seldom if ever underwent the processes of open codification that characterized Christianity” and maintains it is “clear that Nordic paganism was subject to extensive local variation”. Fredrik Svanberg (2003, 102) questions the idea that there was a homogeneous culture during the Viking Age, noting that “manifestations of ‘religion’ vary a great deal between different parts of Scandinavia, different gods seemingly being favored, different kinds of monuments made, different religious rituals applied”. If this was the situation during the Viking Age, it is implausible that there was a codified, dogmatic religion during the Migration Period revolving around Óðinn and Baldr as Hauck propounds. Indeed, Mats Malmer (1977) argued that the gods on bracteates could be Ullr or Njǫrðr, as well as Óðinn or Þórr.

I can entertain the idea that the bracteates showing “three gods” might have something to do with Baldr and that IK 190 Trollhättan shows the Fenris wolf biting the hand of Týr, but I cannot accept the specificity of all the details that Hauck identifies. It sometimes seems as if he tries to find

elements on bracteates corresponding to every detail that Snorri mentions. In particular, I simply cannot support the thesis that most of the Type C bracteates have connections with the “Second Merseburg Charm”, which is separated from them by 400 years and has left no trace in Nordic mythology.

6. In these two chapters, references to more detailed argumentation are often to Hauck’s own works. The reader seeking to follow up such references will need to obtain access to the various numbers of Hauck’s “Zur Ikonologie der Goldbrakteaten”, but these were published in scattered proceedings, *Festschriften*, journals, and the like over the years. Those who lack access to these works must take his pronouncements on trust. While Hauck’s interpretations are fully documented by reference to his own works, and other references listed are almost without exception supportive of his views, Hauck rarely acknowledges interpretations that are contrary to his own and apparently assumes that his are the only ones that are valid.

7. The lack of critical weighing of others’ views is indicative of a larger problem. Readers unaware of the existence of opposing voices will have difficulty finding the apparatus that would enable them to delve more deeply. Hauck impresses many scholars with his immense learning, and they rely on him as the authority on all matters pertaining to bracteates, sometimes perhaps even suspending their own critical faculties. Hauck himself changed his mind many times throughout his thirty-five years of bracteate research, and it is commendable that he was not too proud to revise his opinions, although he was not one to renounce publicly his earlier beliefs. For instance, he began by writing a great deal about the ‘breath symbol’ (*Atemchiffre*) in which Óðinn blows on Baldr’s foal (Hauck 1970, 1971, 1972, 1980); later, however, he began to refer to the ‘speech symbol’ (*Sprechchiffre*) in which Óðinn whispers into the ear of the animal (Hauck 1998b, 48). It is not clear whether he rejects the earlier idea or whether he has already told that story and does not need to return to it. It is interesting that he discusses neither *Atemchiffre* nor *Sprechchiffre* in this volume — a fact as surprising as that he barely mentions the “Second Merseburg Charm” here. In her chapter, Charlotte Behr points to Hauck’s admission that his research went through growing pains in the early years (p. 221, note 386), which is a way of accounting for the various revisions found in the course of publication of the sixty-one parts of the series “Zur Ikonologie der Goldbrakteaten” lasting from 1971 through 2003.

Many people have accepted Hauck’s Óðinn-Baldr thesis uncritically, believing that since his writings dominate scholarly discussion, his interpretations must be correct. Those who are swayed by his massive erudition and the difficulty of his syntax sometimes fall prey to another fallacy. Finding

his scholarship difficult, they blame themselves for lack of understanding, and that leads them to accept his theories unquestioningly. Hauck's two chapters can be mined for details of his method and ideas, but they offer only a short-hand version of his position in 1995. This is not a reference work and certainly not the final word on bracteates.

Bracteate research history

While Charlotte Behr states that the goal of her chapter, "Forschungs-geschichte" (pp. 153–229), is not a complete presentation of the history of bracteate research, she does in fact come very close to accomplishing this daunting task. Her contribution to the volume puts bracteate studies into context, and this seventy-seven-page historical assessment assists the reader who is not intimately familiar with Hauck's writings to become well enough informed to read his later works that depend so heavily on previous publications (his own and those by others). Behr's research as part of the Hauckian team was on the minor symbols on bracteates (Behr 1991), and her meticulous detail in that publication is characteristic of her work. Since that time, she has become the primary representative of the Hauck group in England, publishing the new finds discovered there (Behr 2010).

Behr does not simply sum up over 300 years of bracteate studies chronologically but organizes her discussion according to changing paradigms of research. She begins with the earliest antiquarian interest in the late 1600s, moves through the blossoming of a more scientific approach to typology beginning with Christian Jürgensen Thomsen in 1855, continues with a veritable "who's who" of Scandinavian archaeology in the nineteenth and twentieth centuries (p. 160), leading to an excellent discussion of the 1960s controversy between Mats Malmer and Egil Bakka about artifact types in archaeology (pp. 191–95) and a brief mention of contextual archaeology. She acknowledges the continuing and overarching concerns with the pictorial elements and runic inscriptions on bracteates (p. 153), and distinguishes the desire to classify and date the objects from interests in their religious, political, and social use (p. 154). Also highlighted are the advances made by archaeologists, runologists, art historians, historians of religion, and place-name specialists through interdisciplinary research into the understanding of central places and the ritual functions of bracteates.

Since this review deals primarily with runes and bracteates, I will focus on Behr's synopsis of the investigation of runic inscriptions on these objects and on Hauck's iconological interpretations, although questions of chronology (pp. 165–69, 189–91, 195f.), of who wore bracteates — men or women? (pp.

204–10), of production and central places (pp. 210–14), and of the function of bracteates (pp. 217f.) are also of vital importance to runologists. In a very short section on “bracteates and runes” (pp. 169f.), Behr points out that Wilhelm Grimm, who identified the major figure on bracteates as Þórr, saw already the connection between text and pictures on bracteates that Hauck later recognized. While most of Behr’s writings are reasonably objective, she here uncritically gives credence to the Hauck team’s assessment that inscriptions and images are indeed connected (pp. 389f.), an assertion that is by no means universally accepted (see below under Williams’s general comments). Behr highlights the bracteate from Tjurkö (IK 184), with its text *Wurtē rūnōr an walhakurnē Heldar Kunimundiu* ‘Heldar wrought runes on “the Welsh corn (= the golden bracteate?)” for Kunimundur’ (p. 404), as crucial to the debate about whether bracteate inscriptions and images are not only connected but also self-referential. She finishes this section on bracteates and runes by mentioning the systematic treatment of the runic material by George Stephens in *The Old-Northern Runic Monuments of Scandinavia and England* (1866–1901), which, despite the many problems with Stephens’s runic interpretations, set the stage for later research on the function of bracteates and runic literacy, or illiteracy, at the relevant period (p. 170).

Behr summarizes nearly every interpretation of bracteate iconography. While a number of scholars examine the relationship between bracteates and their Roman prototypes — both the Imperial imagery and the Latin inscriptions on medallions seem to have been imitated in the North — most interpretations of bracteate images and texts (pp. 182–85) depend upon later sources, especially Eddic literature. Behr traces the evolution of early twentieth-century ideas that laid the groundwork for Hauck’s later interdisciplinary research on iconography. Key among these studies are Knut Stjerna’s investigation into the connections between bracteate imagery and *Beowulf*, Axel Oxenstierna’s identification of the image of a man with his hand in the mouth of an animal on the Trollhättan bracteate (IK 190) as a representation of Týr and the Fenris wolf (cf. *Gylfaginning*, 33), and Detlev Ellmers’s determination that the anthropomorphic bracteates of Types A, B, and C all deal with Óðinn, and that the horse accompanying Óðinn on Type C is a sacrificial animal, which then appears by itself on Type D (*Auswertung*, 183–86). Also important for the iconographic interpretation of bracteates is Bernhard Salin’s observation that small symbols such as the swastika and triskele did not have a fixed meaning on bracteates. Behr notes (p. 206) that Hauck (as discussed above) identifies the “small, round object” in the hand of the figure on the Type A Trollhättan piece (IK 189) as

a bracteate, whereas the same sort of circle located in other places did not have the same correlation. In light of this variation, it is curious that Behr and the Hauck team insist that the figural images (unlike the symbols) did indeed have a stable reference. Behr's assertion that bracteate images were understood in the same way in all the areas where they were current (p. 176) is unsupported and apparently uncritically taken by her on trust. It is difficult to assume such constancy across vast distances and over a long time, and it would seem more likely that there was regional and chronological variation in the understanding of bracteates, particularly since there is little or no evidence that there was one consistent Óðinn cult in Scandinavia during the Migration Period, as mentioned above.

Behr speculates that Hauck's interest in the interpretation and social function of Type D animal-style bracteates was influenced by the theoretical focus of the 1980s on contextual archaeology, although Ian Hodder, who initiated that branch of study, would hardly recognize Hauck's methods as akin to his. Hauck used sources in different ways and interpreted them differently from the manner scholars in disciplines such as archaeology and runology would do. His concept of placing bracteates in context was to insist that Type D pieces were part of the same mythological context as Types A through C. Other researchers have focused instead on trying to understand the social function of the animal ornamentation of the Migration Period rather than the anthropomorphic figures (e.g. Kristoffersen 2000).

Bracteates discovered outside Scandinavia (*Auswertung*, 196–204), especially in Anglo-Saxon and Continental burials, are crucial to the overall chronology of bracteates and to the determination of how they were worn and used. Behr notes that few Nordic researchers have paid adequate attention to these outliers. Elisabeth Barfod Carlsen, however, relies on them in her reworking of the dating of Type D bracteates, which turns the generally accepted chronology upside down by considering the most “degenerate” ones to be the earliest. Her chronology has not been accepted by Morten Axboe (2007, 62–64), but it has been given some credence by John Hines (2005, 477).

Among the positive side-effects of the research by Hauck's team is the growth of interest in bracteates discovered outside of Scandinavia as well as the expansion of studies beyond individual researchers' modern political boundaries, which I have suggested have sometimes been a deterrent to such research (Wicker 2010, 68). Anders Andrén considers that bracteates found in Kent and Pannonia, as well as serving as an identity link to Scandinavia, played a political role; Behr acknowledges the importance of the Scandinavian connection but matter-of-factly states that bracteates were

of moment for the Óðinn cult in Kent (*Auswertung*, 200) — expressing not a trace of doubt about the existence of such a cult at the time. The focus of bracteate studies in England has indeed been on Kent, but a bracteate from Undley, Suffolk, breaks the mold with its apparently meaningful inscription (though the meaning is disputed; pp. 201f.). The probable importance of bracteates from other areas of England has recently been underscored through numerous metal-detector finds (Behr 2010).

An interesting connection with the Continent is through the Fürstenberg-type bracteates, which exhibit an *en face* female figure (*Auswertung*, 202–04). Although one example of the type was found at Gudme (IK 391), the rest were discovered to the south, in Germany, and all seem to have originated on the Continent. Interpretations of the figure range from the Virgin Mary (Ellmers; with connections to a Byzantine numismatic image) via a woman “weaving” prophecy with textile utensils (Enright) to a seeress (*vǫlva*) connected to Frigg/Freyja (Pesch). The skirt worn by this figure resembles the “kilt” that Hauck identifies on some of the bracteates showing “three gods” (e.g. IK 51.3 Gudme and IK 51.1 Fakse). Pesch’s identification is thus consistent with Hauck’s comprehensive interpretation of all bracteates as connected in one way or another to Baldr and Óðinn, and thus also to Frigg.

Hauck makes only a few claims about how bracteates were used and who wore them. He declares that men used and wore bracteates, just as medallions were worn by Roman men. In support of this assertion, he cites both IK 189 Trollhättan, which he believes is self-referential, showing a male figure holding a bracteate, and other pieces, which he says show bracteates borne at the neck (*Auswertung*, 206). He also claims that the supposed Gudme necklace mentioned above was worn by a man since no fibulae or beads were found with it. However, it should be recalled that the Gudme bracteates were not found in a burial, and it is simply hypothetical to assert they comprised a necklace. The very few bracteates known from men’s graves were found either in the mouth as Charon’s coins or alongside the body, not lying in place on the chest as if worn as a pendant (Wicker 2010, 74). In fact, nearly all bracteates from burials have come from women’s graves. Behr (*Auswertung*, 208f.) cites several authors who have discussed women or feminine factors and bracteates. Among them are Marta Lindeberg, who identifies female elements in both runic inscriptions and iconography, and Brit Solli, who discusses the androgynous ambivalence of Óðinn as shaman in terms of queer theory; in contrast, Morten Axboe (2007, 111), in a rather forced argument, maintains that a “skirt” and long hair are not female. Hauck and some members of his team appear determined to assert that bracteates

were of, by, and for men. It seems symptomatic that only female researchers have considered the use of bracteates by women.

A major question concerning bracteates is whether their main function was religious or political (*Auswertung*, 218). One approach to understanding how bracteates were used is to examine the relationship between bracteates and their Roman prototypes since both the Imperial imagery and the Latin inscriptions on the medallions seem to have been imitated in the North. Behr (pp. 218f.) points to Näsman's suggestion that local elites in central places gave away bracteates to demonstrate their power, in emulation of the way Roman medallions were used. The Roman use of medallions also lends credence to the Hauckian idea that men wore bracteates. Overall, however, the Roman connection has been insufficiently examined and under-theorized. Looking backwards in time has not been as common as fast-forwarding to medieval texts separated from bracteates by hundreds of years.

Behr devotes a generous section to Karl Hauck and the iconology of the gold bracteates (pp. 220–29), which she begins with an explanation of the need for an iconographic catalogue and for an interdisciplinary project to examine bracteates thoroughly. She discusses how Hauck in his work employed Aby Warburg's distinction between "iconology" and "iconography", whereby the former deals with the interpretation of subject matter, the latter more directly with the identification of formal aspects of images (pp. 221f.). Behr admits that Hauck's work is "not unchallenged" (*nicht unwidersprochen*, pp. 220f. and note 385), referring in particular to challenges from Kathryn Starkey, Edgar Polomé, and me (Wicker 2003) from the viewpoints of literary studies, the history of religion, and art history respectively. It seems curious that the only three critics Behr mentions are Americans (Polomé was Belgian, but his entire scholarly career was in the U.S.). Is it that outsiders are able to think more freely, or that they have less at stake politically and academically by questioning the canon? Another dissenter, Wolfgang Beck (2011 [2003], 267–75), questions Hauck's interpretation of the "Second Merseburg Charm", but Klaus Düwel and Wilhelm Heizmann (2009) criticize Beck and defend Hauck (see Williams below).

Many of Hauck's ideas are explained by Behr, who even discusses some of his earlier works not cited in his own contributions in this volume. In early writings, Hauck referred to Óðinn on Type C bracteates as the "wind god" (Hauck 1972). He first proposed this interpretation in 1969 and elaborated on it in several subsequent works, including a short article from 1971, which is not cited in the extensive bibliography of *Auswertung*. It is curious that the term *Windgott* is not used by Hauck in this volume nor by Behr,

although she summarizes (p. 223) Hauck's related conjectures about the "Second Merseburg Charm" and his discussion of Late Antique knowledge of Asclepius (the Greek god of healing) and Christ as *medicus salvator*, referring though to one of his later works (Hauck 1980).

Behr discusses Óðinn's possible role as a shaman on medallion imitations and various bracteates, including IK 132 Obermöllern (p. 224), and lays out Hauck's argument that the images on Type D pieces belong to the same overall mythological scheme as those on the other bracteates (Hauck 1977). Detached human legs and ears depicted on Type D pieces are interpreted by Hauck as the result of dismemberment as part of a shamanistic initiation ritual (*Auswertung*, 225), and he relates some Type B bracteates (e.g. Allesø, Bolbro and Vedby, IK 13.1-3) — not mentioned in his two contributions in *Auswertung* — to shamanism as a representation of self-regeneration in divine ecstasy. Behr also cites Hauck's assertion that IK 184 Tjurkö shows a shaman as a visionary communicator (Hauck 1988). Edgar Polomé (1994) criticized Hauck's interpretations of Óðinn as a shaman on bracteates, and a larger question is whether shamanism even existed in the Migration Period. Finally, Behr returns to Hauck's insistence that his identification of Óðinn on bracteates is based not only on iconographic details but also on the self-naming of the god in their runic inscriptions (p. 228). He claims that Óðinn's officiating at the sacrifice of Baldr was a legitimating ritual of Migration Period aristocracy (p. 229). Ultimately, it was very important for Hauck to demonstrate that all bracteates were part of the same mythological worldview revolving around Óðinn.

In the last paragraph of her history of research on bracteates, Behr notes that current ideas may or may not stand the test of time, especially as fresh discoveries inspire new interpretations. In addition, changes in research paradigms may also direct attention to different interpretations (as contextual archaeology has moved the emphasis in bracteate studies toward the social function of bracteates). Although she evinces due respect for Hauck's enormous contribution to bracteate studies, Behr exhibits — for a member of Hauck's research team — a healthy dose of skepticism, and puts his ideas into context. In a work published since *Auswertung*, Behr (2011) has propounded an entirely secular interpretation of a newly found bracteate that depicts a man with a drinking horn (Scalford IK 635).

Behr's work is a first-rate history of bracteate research incorporating a thorough survey of all the relevant literature. The length of the volume's composite bibliography (170 pp.) is largely due the comprehensiveness of her contribution. Behr has digested an enormous amount of material and has done a great service to bracteate research by compiling this thorough

interdisciplinary synopsis of changing paradigms of bracteate research through the years.

Network of central places

Alexandra Pesch's chapter, "Netzwerk der Zentralplätze: Elitenkontakte und Zusammenarbeit frühmittelalterlicher Reichtumszentren im Spiegel der Goldbrakteaten" (*Auswertung*, 231–77), builds upon her previous contribution to Hauck's project, namely her monograph (2007) on the groupings of bracteates into "formula families" based on similarities of basic picture forms. That work is a solid and sensible improvement over attempts by others (such as Malmer 1963) who created typological classifications of bracteates that tell us more about the researchers than bracteate typology. After completion of her earlier work on smaller groupings, Pesch can here consider the larger context of bracteates. In the long initial part of the chapter, she reviews archaeological evidence for so-called central places, and in the final section, she proposes understanding Nordic Animal Style I as a "corporate design" or "brand" and reflects on possible contacts between centers as illuminated by bracteates.

"Central places" (also known as "productive sites") served many purposes — *inter alia* economic, political-administrative, military, religious-ideological, and residential. Central places are locations where people exchanged ideas and goods. Thus it is assumed that these places served as distribution centers for bracteates and also for the dies used to make them. At the simplest level, the discovery of concentrations of bracteates may allow the detection of central places; yet central places may also indicate where bracteates are likely to be found, thus risking a circular argument, as Pesch admits. However, it is not merely the discovery of bracteates that has permitted the identification of central places; there is copious archaeological, place-name, and historical evidence, too. Interdisciplinary research, in part carried out by Hauck's bracteate team, has led to the recognition of these sites. Pesch notes that most central places disappeared and were forgotten (*Auswertung*, 233). In many cases, those that have been identified have been suggested by sacral names and corroborated by metal-detector finds that have then led to the discovery of other archaeological traces such as large hall structures. Finds of large (≥ 50 mm in diameter) bracteates and also numbers of bracteates greater than the personal jewelry of an individual (which might indicate a private hoard) are particularly indicative of central places (p. 236).

Before discussing individual sites, Pesch sets the stage by proposing that bracteate styles were the expression of a group rather than an individual, and

she considers how images were created and copied from a pool of possible imagery, with variants reflecting differing capabilities of goldsmiths (pp. 238 f.). An important point Pesch makes is that bracteates were not produced in less expensive silver (other than a few examples in England) or bronze, as fibulae were (p. 240). Thus it must have been crucial that bracteates were made of gold, and they must have been made in secure places under the protection of a political-military leadership that could guarantee the safe-keeping of gold, and also control the imagery. Yet it is not clear how craft workers moved around and spread bracteate designs or actual dies (see Wicker 1994b). In her 2007 work, Pesch examines bracteates with related designs that reflect a decentralized copying process, and in *Auswertung* she asserts that bracteates made with the same die reflect individuals in direct contact with each other (p. 241). It follows that these people could have been cult specialists or elite leaders who delivered authorized iconographic scenes on dies, runemasters who produced inscriptions to order, or even itinerant craftsmen working independently.

Pesch discusses five central places that are relevant for the study of bracteates, each with a slightly different “flavor”: Gudme/Lundeborg, Uppåkra, Sorte Muld, Ravlunda, and Sievern. She also tentatively mentions other sites that exhibit certain qualities characteristic of central places but are inadequately investigated at the present time. Many of these sites have been discovered in recent decades, after Hauck began his research on bracteates. In particular, discoveries initiated by metal detectorists are rapidly changing the landscape of bracteate studies. Pesch provides a map (p. 244) marking conjectural central places with suggestions as to where many “formula families” may have originated. There is a great deal of information embedded in this very useful graphic. Even without familiarity with her groupings, the viewer can at a glance visualize where Types A, B, C, and D are most commonly found, noting for instance the preponderance of Type D in Norway and Jutland.

The neighboring sites of Gudme and Lundeborg on Fyn in Denmark are paired, with each serving a different purpose. *Gudme*, meaning ‘home of the gods’, indicated a sacral place and had a large ceremonial hall, whereas Lundeborg was a production site for gold objects. An extremely high-quality, large Type B bracteate found at Gudme, IK 51.3, is a typical indicator of a central place. Among the twenty-two bracteates found at this site are the earliest in Axboe’s seriation (2004) as well as many early types in Pesch’s “formula families”, so it has been suggested it was possibly the place where the first bracteates were created and produced (p. 246; there are, however, later Type D examples found here, too).

All of the administrative, religious, economic, etc. functions seen at the paired sites of Gudme and Lundeberg are found together in one location at Uppåkra in Skåne, Sweden. This was an important trading site going back to the Roman Iron Age and continuing into the early Viking Age, yet of the eleven bracteates found there none belong to the later Type D. To explain this, Pesch (p. 250, citing Margrethe Watt) proposes that a later cult replaced the use of bracteates here with gold foils (*guldgubbar*). Although bracteate production at Uppåkra seems to have begun later than at Gudme/Lundeberg, a Type A bracteate (IK 610) with a previously unknown runic inscription was found at the former site, and Pesch suggests that it was created there (p. 250) — an exciting yet completely speculative proposal.

Like Uppåkra, Sorte Muld on Bornholm, Denmark, had a long existence from Roman through Viking times, and here too gold foils in great quantities eventually replaced bracteates. There are no Type A or Type D bracteates among the twelve pieces found at Sorte Muld, but Pesch claims that the presence of three examples of a Type B bracteate showing “three gods” (IK 595 Fuglsang/Sorte Muld) and supposedly depicting the sacrifice of Baldr indicate that this was a cult site (p. 252). Pesch suggests that Bornholm examples of her C12 “formula family” (2007, 210–15) reflect contact with Poland and Bornholm’s status as a “bridge to the Continent” (*Auswertung*, 253). In addition, she proposes that the bracteates with the inscription **ota** known from Skåne, Blekinge, and Bornholm could have been created at Sorte Muld.

At Ravlunda, Skåne, on the Baltic coast, the recovery of evidence of metal-working has been going on over a long period. A bracteate from Ravlunda (IK 144.1) has an intriguing punch identity with another bracteate with a different central stamp from Öland (IK 279 Holmetorp), a relationship that has led to some hand-wringing and thoughts about how tools such as punches and dies were produced and shared (Axboe 1994, 74; Wicker 1994c, 147). The bracteates from Ravlunda exhibit ties to the Danish islands as well as Öland and the Swedish mainland. Pesch suggests that all of the bracteates found at Ravlunda were imported rather than being produced there (*Auswertung*, 256).

Sievern on the Elbe-Weser delta came to the fore with Hauck’s 1970 monograph, *Goldbrakteaten aus Sievern*, which elaborates on a hoard find from this location. Near the site are cemeteries, walls, and a palisade (p. 256). Pesch suggests that Sievern was an intermediate station where Danish bracteates were copied in preparation for distribution as far away as Frisia and England. Among the fourteen bracteates found at Sievern is IK 156, with a runic inscription, which Pesch links in a “formula family” with IK 76 Wurt Hitsum and IK 323 St. Giles Field (p. 258).

Pesch suggests possible sites of additional central places and also general areas within which such sites might in the course of further research be located — in Scandinavia, and also in England and on the Continent (pp. 261–67). In particular, Pesch points to Uppsala and Helgö as likely places for the production of bracteates, especially since gold fragments have been discovered at Helgö (p. 265). Besides numerous sites in Scandinavia, other likely places include the Dutch coast and Nebenstedt in Lower Saxony, which is suggested as an intermediary link to Thuringia where the Fürstenberg-type bracteates are centered (p. 267). Other possibilities are along the Elbe and Saale to the south, the Danubian area, and Kent in England (where twenty-six bracteates had been found when Pesch wrote her chapter). The situation is changing rapidly and unevenly with metal detecting regulated differently in various jurisdictions. In particular, the number of English bracteates has mushroomed during the past decade, and Pesch appropriately cautions that our concept of central places could change radically with further discoveries (p. 269). I would like to add that metal detecting in Poland is rapidly changing our understanding of bracteates there.

According to Pesch, one of the key functions of central places from the fourth century onwards was to serve as a forum where members of the elite could come into contact with each other, where Roman luxury goods were distributed, and where the imitation of Roman images and ideas could take place. Although central places have not been dated to earlier than the third century, she mentions that common burial customs indicate contacts among the elite during the preceding two centuries; she envisages at least passive knowledge of Latin at embryonic central places (p. 270), and that the runic script arose in these precursors. Bracteates that both imitate Roman imagery and show a Northern pictorial vocabulary were also created in this milieu, regardless of whether the Scandinavians saw themselves as followers of the Romans or as adversaries (p. 271). Pesch suggests that Nordic Animal Style I could be considered the equivalent of today's corporate branding with a common identity expressed across a vast area via a simplified and standardized pictorial code (p. 272). This is a thought-provoking approach to "identity", a much-invoked buzzword of the past decades (see e.g. Pohl and Mehofer 2010).

Finally, Pesch considers the bigger question of how Animal Style I spread. She proposes that high-level control over imagery and inscriptions was exerted during cultural contacts at, for example, assembly (thing) gatherings, and that such dealings were peaceful since none of central places discussed earlier are fortified (*Auswertung*, 274f.). She insists that the images on bracteates were divine and did not depict individual persons as on Roman

coins, but she does not substantiate this assertion, instead suggesting that elite cult specialists or “priests” similar to Celtic druids regulated the imagery (p. 275). However, it seems just as likely that high-level control over pictorial and runic details could have been exercised by political leaders who wanted to depict themselves, as Roman emperors did. Despite the cultic overseeing that Pesch envisages, she admits that bracteates changed, albeit gradually, mirroring local religious and political circumstances. Not all pictorial types have surfaced at all central places that have been identified, whether because of the accidents of preservation or because different images predominated in various locations. Thus we may question how tight the supervision by so-called priests and runemasters really was, and whether bracteates were perhaps venerated as much for their gold as for their specific images. Despite the apparent importance of Gudme/Lundeborg, the leaders at this site may not have had cultural dominance over the vast network of central places. Connections between central places provided a means of communicating iconographic details, but variation in imagery indicates that artisans did not follow models dogmatically.

Pesch’s solid and accessible investigation of central places and their role in the spread of bracteate motifs showcases some of the best work that has come out of the interdisciplinary bracteate team. Her contribution should make it possible both to extend her concept of formula families to more bracteates (and new finds) and to place future research on additional central places in the context of cultural and political networks in northern Europe.

Inscriptions and bracteate chronology

Although Morten Axboe is an archaeologist, his chapter “Die Chronologie der Inschriften-Brakteaten” (pp. 279–96) is obviously of great importance for the study of runic inscriptions and as such will also be commented on by Henrik Williams (see below). Axboe’s (2004) detailed work on the production and chronology of bracteates was presented as a monograph in the same series as *Auswertung* and Pesch’s monograph. The short contribution in *Auswertung*, with a list of inscriptions in seriation order (pp. 290–95), focuses only on pieces with inscriptions. However, it does not deal with all runic bracteates but merely with those discovered before November 1988, which is most disappointing for a work published in 2011. Since he uses details of the anthropomorphic head, Axboe is limited to those depicting humanoids and those with relatively large heads. Thus he excludes the Type F bracteates with inscriptions (IK 241.1 Väsby and IK 241.2 Åskatorp) and the Type B examples, with inscriptions, of the sort that Hauck refers to as

the bracteates showing “three gods” (IK 51.3 Gudme, IK 165 Skovsborg, and IK 39 Denmark), since the heads on these examples are too small to see the details that he considers significant (p. 281). In his monograph he investigated a total of 342 different dies (models) of bracteates, of which 125 have inscriptions, including both imitations of Roman capitals and runes; the latter subset is the material presented in the *Auswertung* chapter.

By means of correspondence analysis, Axboe in his monograph organized the bracteates he investigated into four groups, H1 through H4. His seriation is based on typological details of the ears, eyes, and hairstyles of Type A, B, and C pieces. He then arranged them in an internal chronology, which should not be confused with an external chronology calibrated to absolute dates. It is important to point out that his sets are ambiguous rather than mutually exclusive since H3 and H4 overlap and cannot be clearly separated. Martin Rundkvist (2006) has criticized Axboe’s methodology on this point while also commending him on the basic tenets of his research. Axboe explains that one of the difficulties in dating bracteates is that a Roman coin available in the North might inspire an earlier as well as a later bracteate, thus reflecting continuing input from Mediterranean formal iconography (*Auswertung*, 279).

From his internal chronology, combined with analyses of closed finds and use-wear, Axboe estimates an external chronology, with group H1 dating to the third and H2 to the last quarter of the fifth century (p. 281). He places the end of bracteate production shortly after the month-long darkness that occurred A.D. 536–37. (Unfortunately, there is a typographical error on p. 281 so that the darkening of the sun is listed as happening in 336–37.)

Axboe’s datings depend only on details of the male heads and are completely independent of the inscriptions. Almost all of the twenty-two bracteates in his group H1 have some kind of inscription, with many imitations of Roman capitals. There are only four bracteates from three dies that imitate specific, traceable Roman coins, two different ones of Constans (337–50) and one of Valens (364–78). Inscriptions within a runic band that ends in a bird’s head (IK 110 Lindkær, IK 140 Overhornbæk, IK 312.1 Overhornbæk, and IK 312.2 Vendsyssel) appear early in his seriation because they emulate Roman models in the placement of the inscription around the perimeter (p. 285). Bracteates from group H1 contain no lexical runic inscriptions; inscriptions that are semantically interpretable begin in group H2 and continue through H3 (p. 289). It is revealing that formula words appear across bracteates of all groups except H4 (p. 286, fig. 3; they are garbled in H1). Axboe places one bracteate with an inscription at the transition from H3 to H4 (IK 44 Djupbrunns), and one in his H4 group (IK

158 Sigerslev, non-lexical). He dates the end of bracteate runic inscriptions to around A.D. 540 (p. 289). It will be interesting to see how the discovery of the first Type D bracteate with an inscription found at Stavnsager in Denmark in the summer of 2012 will affect our interpretation of the corpus (<http://runer-moenter.natmus.dk/nye-guldbrakteater-med-runer/>).

Axboe mentions the possible use of abbreviations in formula words (p. 285), which to some extent mirrors the use of the “abbreviation principle” that Hauck invokes when a bracteate does not display details he expects. For both images and inscriptions, there is disagreement about how much a researcher should be allowed to “correct” what he perceives as mistakes. Both Axboe and I (Wicker 1994a, 77) have cautioned that some apparent mistakes in bracteate inscriptions may have occurred due to the difficulty of executing bracteate dies in mirror-image of the intended outcome, not simply due to the illiteracy of those designing or producing the runic inscriptions.

Axboe’s concise contribution summarizes findings about a subset of bracteates (a total of 125 with inscriptions) extracted from his analysis of a larger set of the bracteate corpus (342 examples with large heads), nearly one-half of the total number (622) of known bracteate models (*Auswertung*, 902). Axboe cautions readers adequately that his illustration of lexical runic inscriptions in seriation order (p. 288, fig. 4), as also his seriated list of all inscriptions according to groups H1 through H4, should not be construed as giving an absolute chronology of bracteate inscriptions. However, it is very tempting to ignore the warning and take the list at face value. Axboe’s chronological investigations are meticulous and provide a great deal of information for further research. It is to be hoped that he will continue his work to include all bracteates — not just those found by 1988 that display clear humanoid heads.

Anglo-Saxon animal-ornamented shields

Tania Dickinson draws attention to an interesting but little-known category of material in her chapter “Iconography, Social Context and Ideology: The Meaning of Animal-Ornamented Shields in Early Anglo-Saxon England” (pp. 635–86). Hers is the only contribution in *Auswertung* other than that by von Padberg that does not deal explicitly with bracteates, and it is difficult to understand why this outlier was included, beyond the fact that the author was a member of the interdisciplinary bracteate team. Dickinson has already published this body of material, in 2005, and with the exception of minor revisions made in 2008 completed her *Auswertung* manuscript in the same year (p. 635, note 1).

Metal mounts with animal ornamentation attached to twenty-one Anglo-Saxon shields found in burials do have some parallels with bracteate imagery. However, other sorts of material, namely gold foils, Vendel Period dies and pressed plates on helmets — perhaps Gotlandic picture stones too — can also be compared to bracteates. Yet there are no chapters in *Auswertung* on these subjects, even though we learn from the foreword (p. viii) that Hauck left an unfinished manuscript on gold foils intended for inclusion in the final volume. Dickinson compares the imagery on shields to that on bracteates, making a case for a shared iconography based on Hauck's interpretation of bracteates. The iconographic comparison that merits the most discussion is whether a creature on the shields is a fish and if so, whether it is a pike (pp. 644–48). Dickinson compares the “pike-like” beings on the shields to Hauck's discussion of reptiles and snakes on Type B and C bracteates, suggesting that all these creatures represent opposition to the gods (p. 646). To make such an appraisal, one must first presume a shared visual vocabulary and then establish a compatible chronology.

Dickinson admits it is problematic to assume “that similar images have the same meaning even when in different contexts” (p. 636); thus we can question whether the designs on Anglo-Saxon shields are relevant to our understanding of bracteates and vice versa. She expresses some doubts herself and refers to Jane Hawkes's (1997, 314) warning that images are “malleable; they can express things in ways which allow for their common form to be retained and shared among members of more than one community, whilst not imposing upon them the constraints of uniform meaning”. To admit the possibility of variable meanings is to repudiate Hauck's very insistence that bracteates present a unified, coherent body of material representing the same religious content even through widely distributed and varying pictorial images. Dickinson herself seems hesitant, finding in her comparison of the materials only “striking analogies ... which might open a route to interpretation” (*Auswertung*, 636).

Dickinson's (p. 641) dating of the shields depends upon Barfod Carlsen's (2002) chronology of bracteates, which turns the traditional dating of the Type D examples upside down. Barfod Carlsen's dating has not been accepted by other members of Hauck's bracteate team, neither by Behr (*Auswertung*, 196 note 239) nor by Axboe, as mentioned above. Dickinson rather tortuously argues that the alternative dating of certain Type D bracteates as the earliest “need not invalidate Karl Hauck's arguments” for their connection with open-jawed animals on a group of Type B bracteates (p. 641). The fact that Dickinson employs Barfod Carlsen's chronology for bracteates instead of Axboe's is curious, since the latter was part of the

bracteate team and his work is included in this volume. The impression is reinforced that *Auswertung* has been treated more as the proceedings of a conference, consisting of diverse contributions, than as an integrated summation of bracteate research.

One of the most thought-provoking observations by Dickinson is advanced in her discussion of the public display of shields. She notes that the animal ornamentation on metal mounts would have been visible only at close range and suggests that the images were perhaps used to identify warriors (p. 651) during the heat of battle, as an aid while distinguishing the dead after a battle, or in “protecting” a grave chamber after death (p. 653), ultimately pointing to the apotropaic function of animal ornamentation on shields as defensive weapons. One can similarly question how visible bracteates were — whether worn by men or women — and what role the imagery on them played in recognition of group and individual identity. This is an issue I have previously discussed (Wicker 2005), but the subject has received little attention in bracteate studies.

Catalogue of newly found bracteates

As an introduction to the “Katalog der Neufunde” (*Auswertung*, 891–999), Morten Axboe summarizes some of their highlights and some quirks of the earlier volumes. Among the latter is the fact that certain pieces were included due only to the special interests of Hauck, even some that fall outside the technical and chronological constraints of the corpus, such as the IK 232 Daxlanden fibula (p. 895). Axboe notes that three bracteate dies have now been found (IK 572 Postgården, IK 609 Essex, and IK 637 Morley), yet the short English summary (pp. 718 f.) mentions only the first two. There are inscriptions on twenty-six of the new bracteates from twenty-one different stamps (including five stamps that were known previously). Completely new inscriptions are found on eighteen bracteates from thirteen different stamps. At the end of 2010, a total of 1003 bracteates were known from at least 622 dies, plus seventeen unique medallion imitations. It is very fitting that culmination of this project occurred just when the number of pieces crossed the 1000-mark!

The catalogue itself (pp. 905–99) follows the pattern of the previous volumes, but Morten Axboe and Charlotte Behr have made a few changes, mainly to simplify its use and shorten it (p. 898). The description of the quadruped, for instance, is now summarized concisely in narrative fashion, rather than in sixteen formatted lines. Most of the new entries include a reference to Pesch’s formula families (abbreviated as “FF”) at the beginning

of the catalogue entry. The descriptions of the head of the anthropomorphic figure as defined by Axboe for his correspondence analysis of details are now used, and Axboe helpfully points us to the English translations of these type descriptions in one of his earlier publications (Axboe 1998, 141–43).

The catalogue numbering continues according to the system established in the first volume so that new examples from already-known stamps (models) have the same number with a decimal subspecification. Bracteates from new stamps are given numbers running from IK 570 onwards. Stamp-identical pieces from differing find-places are distinguished through the use of a decimal subspecification, whereas “identical” examples from the same site are not differentiated. Lumping die-identical pieces together is not a problem for Hauck’s iconographical descriptions of bracteates, but it is problematic when concerns turn to technical issues about the manufacture of individual pieces. From this standpoint, it would be preferable if each exemplar had a unique identifier. Occasionally there are discrepancies such as IK 51.1 Fakse and IK 51.3 Gudme, which were not stamped from the same die even though the same initial number would indicate that they were, had the system been applied consistently. Similarly inconsistent, the stamp for the bracteate with inscription IK 47.1 Elmelund was not the same as for the die-duplicates IK 47.2 Broholm and IK 47.3 Enemærket. In the case of two new die-identical bracteates from the central place Uppåkra, IK 591.1 and IK 591.2, the use of the decimal subspecification indicates that these pieces were found at two distinct localities within the large settlement site (*Auswertung*, 897).

The original numbering system followed the alphabetical order of find-places, starting at “A” in each of the *IK* volumes. Volumes 1 and 2 thus contain bracteates of Types A, B, C, and F and also medallion imitations arranged alphabetically in each; when looking for a particular example, it is therefore unclear in which of the first two volumes one should search. Volume 3, on the other hand, presents all Type D bracteates in clear alphabetical order, plus an appendix of new finds up to 1988. *Auswertung* presents near on 100 new bracteates more or less in the order in which they were discovered, so inevitably any semblance of alphabetical order is lost.

Nearly all the bracteates in the first three volumes were autopsied by a bracteate specialist (usually Lutz von Padberg or Morten Axboe), whereas some of the newly found pieces have not been examined by any member of the bracteate team (p. 899). Physical inspection of artifacts is essential, yet most scholars studying different aspects of bracteates cannot examine every bracteate in person. Instead, they must rely on descriptions by those who autopsied them as well as drawings and photographs. I trust the hands-

on examination by an expert such as Axboe, but Hauck did not always agree with his findings, for example considering what Axboe recognized as manufacturing residue to be an iconographic detail, a “horse-tail” (see above), and then “improving” the *IK* drawing to match his interpretation.

Bracteates in the first three *IK* volumes were published at a scale of 4:1 (or 3:1 if exceptionally large), which is an enormous improvement over the 1:1 illustrations by Mogens Mackeprang (1952). Since the format of the *Auswertung* volume is smaller than that of the *IK* volumes, illustrations here are at 3:1. Drawings for the first three volumes of the catalogue were uniform, all executed by Herbert Lange, but more variability is apparent in the new illustrations, which are made by different draftsmen. It is a fallacy that photographs are more objective than drawings; they are dependent upon light source and direction and can be as misleading as drawings. Photographs for this volume are of variable quality, in many cases provided by museums and individuals, in particular for the new finds from England. It is commendable that the decision was made to include as many new bracteates as possible, even if no photographs could be obtained.

Physical autopsy of the artifacts is crucial for understanding the production of bracteates and workshop connections. Hauck did not originally plan to include technical details, but after Axboe — who had personally examined most of the bracteates — joined the team (*IK*, 2.2: viii.), he was invited to add his comments about technical details as part of the artifact description. An addendum lists that information for the first volume (*IK*, 3.1: 241–302) and adds photographs for particularly interesting details on the reverse of bracteates (*IK*, 3.2: pl. 128–31). In *Auswertung*, such comments are also incorporated. However, given the huge resources devoted to this entire project, it is unfortunate that reverse images of all bracteates in *Auswertung* were not provided, as is standard with numismatic material. In fact, all of the over 1000 pieces in the corpus should have been thus illustrated. In addition, examples stamped with the same die and found at the same site have not been uniquely documented, and there are indeed differences in details of the punched borders, loops, and wire edges. I understand that *IK*, 1–3, and *Auswertung* constitute an iconographic catalogue and not a technical catalogue, but I believe that this was a missed opportunity. Not in our lifetime will a project document all of the bracteates again, and we may never see another such catalogue in printed form. It took twenty-two years following the completion of volume 3 of *IK* in 1989 for *Auswertung* to appear. The question now is how long will it be before the entire catalogue becomes available on the Internet — not just as scans of the printed pages but as a searchable database updated with each new bracteate that is found.

This is a solution John Hines has called for (2005, 477), so that we do not have to wait another quarter of a century for the next installment.

Iconological conclusion

The foreword to *Auswertung* introduces the eleven members of the interdisciplinary team (Hauck plus Axboe, Beck, Behr, Dickinson, Düwel, Heizmann, Müller, Nowak, von Padberg, and Pesch), and the reader understands that they were each allotted a chapter to supplement the final installment of the catalogue. (As noted above, Axboe and Pesch have already published monographs as the culmination of their efforts with the team.) Yet the contributions are of varying relevance to an overall evaluation of bracteates and give the work as a whole the idiosyncratic character of an odd collection of articles put together by a committee.

It is unfortunate that the catalogue supplement could not have been published as a separate, smaller work, with the history of research by Charlotte Behr and the massive bibliography. A smaller volume would have been more affordable and more accessible, besides being physically easier to use. The other contributions could then have been published as another *Festschrift* to Professor Hauck (cf. earlier ones in 1982 and 1994), this time by the interdisciplinary team that he had assembled. I can imagine that the decision to send the volume out in its existing form was difficult and most probably driven by the constraints of publishing economics.

Despite problems that I perceive in Hauck's vision of bracteate iconology and the unrealized nature of what Hauck wanted the *Auswertung* to be — a catalogue plus a distillation of his iconology of bracteates that would have updated and superseded his published installments of “Zur Ikonologie der Goldbrakteaten” — the tome that has been produced is extremely useful if only in gathering together all of the divergent material it contains. All in all, perhaps the most positive aspect of Hauck's bracteate project was that it provided the opportunity for individuals to work as a multidisciplinary team and become more aware of the impact of each other's disciplines — archaeology, runology, iconology, name research, history of religion — so that they could carry out genuinely interdisciplinary research. I hope that there will never again be runologists who ignore all bracteates without inscriptions nor archaeologists who know nothing of runic inscriptions, as has been the case in the past.

Texts⁴

Henrik Williams

Of all bracteates known at the end of 2010, no fewer than 222 from 153 distinct dies bear texts, predominantly with runes or runelike characters, according to a calculation by Klaus Düwel and Sigmund Oehrl (*Auswertung*, 296). Latin or Latin-like characters occur chiefly on Type A bracteates whereas runic legends appear most often on Type C. Altogether there are 143 distinct runic inscriptions. Thus runes on bracteates constitute nearly one-third of the some 450 inscriptions with the older futhark⁵ (excluding the so-called Anglo-Frisian material), even if this proportion decreases somewhat when one considers the number of preserved individual runes.

Not only is the runic corpus on bracteates of considerable dimension, it is relatively well dated. Even though the suggested chronology of rune-bearing bracteates has varied with different proponents, the timespan is no more than 150 years, and Axboe (see Wicker above) has proposed an even narrower dating, to A.D. 450–540. Many other older runic inscriptions are no more closely dated than to within a timespan of several centuries.

Despite the magnitude and relatively exact dating of bracteate runes, this corpus has received too little attention and is sometimes ignored altogether. Bengt Odenstedt, for example, in his study on the typology of and graphic variation among the older runes (1990) chose to exclude all inscriptions on bracteates not consisting simply of the rune row “because they are frequently impossible to interpret and often contain a number of highly individual or distorted runic forms” (p. 17; he did, in fact, include some bracteate inscriptions without justifying their inclusion).

It is true that a disproportionate number of (seemingly) non-lexical inscriptions and aberrant graphs appear on bracteates. Why this should lead to the rejection of the “not so few interpretable inscriptions and the number of clear, and hence usable, rune forms in the unintelligible inscriptions” (Williams 1992, 194) is a mystery, one which Odenstedt (1993) made no effort to dispel; instead he abjured the responsibility to deal with the “scribblings of a monkey” (p. 7). It is also unclear why “highly individual or distorted runic forms” should be exempt from examination. Here, in the margin of runicity, there may be important discoveries to be

⁴ I would like to thank Klaus Düwel, whose generous gifts over the years of off-prints from his rich *oeuvre* have facilitated my work on this review article significantly.

⁵ See <http://www.runenprojekt.uni-kiel.de/beschreibung/9/fundliste.pdf>.

made. As I have pointed out (Williams 1992, 194): “The existing corpus of inscriptions is so small that one should only exclude a part of it for very good reasons.”

Odenstedt is by no means alone in his contempt for bracteate inscriptions, nor is he the first to consider them to be inferior products. Erik Moltke distrusted deeply all runic work by metalsmiths, whom he by definition considered to be illiterate (e.g. 1985, 114, 124). Why this particular category of craftsmen should lack reading and writing skills more than their contemporaries in other trades is unclear to me. There are, after all, some well-executed runic texts on bracteates, which must ultimately have been produced by metalsmiths. The many badly executed runic legends may certainly be accounted for by the existence of illiterate smiths, but there are perhaps other avenues of explanation to be explored.

Recent decades have seen important contributions to bracteate runology by Klaus Düwel, Gunter Müller, and not least Sean Nowak in his 920-page dissertation (2003). The last has not received the attention it deserves even though it is available on the Internet. Like much German bracteate runology it is not an easy read, but it contains enormous amounts of hard data and valuable observations, and cannot be overlooked when discussing early runic inscriptions.

For runologists interested in the older material, it is a great pleasure to welcome yet another substantial contribution to the study of runic bracteates and especially of their inscriptions. In the volume being reviewed there are no fewer than five chapters devoted to runic texts on bracteates, covering more than 300 pages, not counting relevant parts of the bibliography and catalogue nor the discussion of writing in the iconographic sections of the volume. Among the last is a most useful overview by Morten Axboe of bracteate texts, grouped chronologically (pp. 290–96).

The five runic contributions deal with:

1. Problems of reading and interpreting the name stock of the bracteate corpus, by Heinrich Beck (19 pp.),
2. The transition from letter magic to name magic in bracteate inscriptions, by Gunter Müller (58 pp.),
3. Semantically interpretable inscriptions on the gold bracteates, by Klaus Düwel and Sean Nowak (99 pp.),
4. Letter magic and alphabet sorcery in the inscriptions on gold bracteates and their function as amulets, by Klaus Düwel (49 pp.),
5. Formulaic words on gold bracteates, by Wilhelm Heizmann (77 pp.).

I will also comment on:

6. Forms of reaction by polytheism in the North to the expansion of Christianity as reflected by the gold bracteates, by Lutz von Padberg (32 pp.).

In such a book, one would have expected a different form of organization: first an overview of all bracteate inscriptions, interpretable or not, and then chapters on names, formulaic words, and magic, in that order.⁶ For an introduction one must instead turn to Morten Axboe's short contribution on the chronology of bracteates with inscriptions. It is, however, important to remember that Axboe's list is incomplete. The reason is not just that new bracteates keep being found (see below) but also that he did not include all known bracteates with runes in his seriation (see Wicker above). For these reasons, at least five semantically meaningful inscriptions are left out of his list (cf. p. 287).

There is much in the present volume that is impressive and of great interest, as I hope to show. I will also, however, offer extensive critical commentary after an initial survey of each contribution, as well as in my conclusion. Since the chapters have been authored independently I shall review them separately and offer my assessment consecutively.

Names on bracteates

Heinrich Beck's chapter, "Lese- und Deutungsprobleme im Namenschatz des Brakteatencorpus" (pp. 297–315) starts with three assumptions (p. 298), firstly that iconographic expression and runic message are related, secondly that since runic items are found on only one-fifth of all bracteates the inscriptions contribute an extra dimension to the iconographic/iconological interpretation (by Hauck, and on which the linguistic interpretations are dependent), and thirdly that the bracteate corpus constitutes its own genre which is to be understood as a unified whole.

The delimitation of the onomasticon or name stock investigated by Beck seems to be derived from what have been interpreted as names in *IK*, complemented with specific additions by Gunter Müller and Ottar Grønvik. Unfortunately, there is no list of the names Beck accepts and why. He refers (*Auswertung*, 297) to Düwel and Nowak's contribution where

⁶ In the German and English summaries (pp. 694–99 and 710–15, respectively) the order more logically starts with the Düwel and Nowak and Düwel chapters. Why this differs from that of the actual disposition in the book I do not know.

ten meaningful inscriptions are included, and presumably all or most of Beck's names should be found therein. He does mention IK 163 Skonager 3 *Niuwila* and the related IK 43 Darum 5 *Niuwil*, IK 42 Darum 1 *Frohila*, IK 161 Skodborg *Alawinir*⁷ and *Alawidir*, IK 149.1 Skåne *Gakar* (following Ottar Grønvik's interpretation of this as a byname, which would make it an addition to the onomasticon), and IK 184 Tjurkö 1 *Kunimundur* and *Heldar*. It should, however, be pointed out that the ten inscriptions studied by Düwel and Nowak are those of interest for the history of religion (cf. *Auswertung*, 396) and thus do not encompass all runic texts on bracteates containing names; in his contribution, Gunter Müller (p. 325) lists no fewer than eighteen names or namelike personal designations.

Beck's conclusions concerning runic charms on bracteates are three (pp. 314f.):

1. Runic sequences identified as names should be interpreted as bynames. This means that *Kunimundur* is probably not a given or "first" name but a designation for the 'protector of the family'.
2. The iterations in bracteate runic inscriptions, as well as bynames of the type *Gakar*, onomatopoeically 'cackle', speak in favor of a ritual element.
3. Inscriptional contents move between the poles of threatening statements (with iconographic back-up) and invocations for averting danger.

It is extremely difficult to get a grip on names in the bracteate corpus using Beck's study — much recent onomastic work has been ignored, most sensationally Lena Peterson's lexicon (2004), which includes all names in the oldest runic inscriptions (including four probable and four possible names on bracteates mentioned by neither Beck nor Düwel and Nowak). The fact that Beck does not state explicitly which names are included and which excluded makes it even harder. Since lists of newly found names and of now discarded names posited in previously published *IK* volumes are nowhere to be found, there is no way of knowing which names are actually thought to exist. In addition, the picture of the onomasticon is muddled by Beck's inclusion of topics not related to the *Namenschatz* ('name hoard') in the sense a name scholar would understand. One example is the lengthy

⁷ In this review, I consistently use *w* instead of *v* even where the author(s) may have used the latter. On the other hand, I have chosen *r*, **r**, and **ī**, in accordance with the usage in *Auswertung*, although I personally prefer *z*, **z**, and **ç**, respectively.

discussion (*Auswertung*, 308–11) of the Undley bracteate, which contains no names at all.

Beck in his title promises to discuss problems of reading in addition to those of interpretation, but as far as I can see he mentions only one, **g|ōla** versus **guoba** on IK 76 Hitsum, and here Beck (p. 311) simply notes the alternative readings. Klaus Düwel (1970, 286, and in *IK*, 1.2: 149) interprets the former sequence as *Glōla*, a diminutive of the nominalized verb *glōa*, which he sees as the name of a runemaster. The latter reading emanates from Morten Axboe (cf. Müller 1986, 460 note 41), but is given no interpretation, neither by Axboe nor by Beck. Elmar Seebold (1996, 195 f.), who is not mentioned in this context, suggests the reading **groba** ‘that which belongs to a grave or burial’. My point here is threefold: firstly that we are not given a definitive reading, secondly that we are not told if any of the readings result in credible words, and thirdly, if the latter is the case, whether such a word or such words might constitute names. This is indeed primarily a “problem of reading” but Beck does not enter into it and thus I cannot see what “Lese[probleme]” is doing in the title of his contribution. Maybe that is why it has been translated “The Problem of Names in Pictorial Codes and Runes on the Gold Bracteates” in the English summary (*Auswertung*, 712), with no mention of reading problems. That labels the actual contribution well but is not a very accurate rendering of the heading in German.

Names and bracteate magic

Another chapter dealing with names on bracteates is by Gunter Müller, “Von der Buchstabenmagie zur Namenmagie in den Brakteateninschriften” (pp. 317–74). Surprisingly, it is not written for the present volume, but is a reprint of a twenty-three-year-old journal article (1988), with some insignificant additions. The original article is in many ways excellent, and whoever has not read it already should take the opportunity to do so now. But it stands to reason that more than two decades of runology and other scholarship has changed the basis of knowledge significantly and rendered Müller’s study partly out of date.

In the introduction (pp. vii f.) we learn that it was Müller who was originally recruited to deal with the names on runic bracteates, but that his scholarly career took a different turn and Heinrich Beck was drafted in to revise the treatment of the onomastic material. One would have expected this to be mentioned in Beck’s contribution and reflected in its structure, which it is not; it should have been an updated version of Müller’s earlier

work, and Müller's own chapter should perhaps have preceded Beck's rather than following it as is now the case.

It would be unfair to review Müller's chapter on an equal footing with the other contributions, and one wonders why the editors included it without correlating it to the rest. I shall, however, make occasional references to it in the following.

Interpretable bracteate inscriptions

The chapter by Klaus Düwel and Sean Nowak, "Die semantisch lesbaren Inschriften auf Goldbrakteaten" (pp. 375–473), is not only the longest of the runic contributions but also the most valuable since it deals with semantically interpretable inscriptions (which they term 'semantically readable'). In my opinion, it could have been published on its own as a separate booklet. It is well written and with few exceptions well structured and up to date.

Düwel and Nowak (p. 380) rightly reject Elmar Seebold's (1991, 460–91) chronology of bracteate runic forms, and they discuss intelligently problems of how to come to grips with the more difficult bracteate texts (*Auswertung*, 382–88). They also debate (pp. 388–96) the communicative situation of bracteate inscriptions: Who is communicating and what is the relationship between text and picture? Traditionally, the first question has been answered by positing a runemaster, a runic magician, or just a plain magician as the "sender" of the message. But Düwel and Nowak assert (p. 389) that new interpretational perspectives have opened up since the images on bracteates have been shown to depict deities. They claim (pp. 389 f.) that Karl Hauck has made it seem more and more probable that bracteate pictures present Óðinn in various mythic and ritual constellations, and consequently that an attempt may be made to interpret the accompanying inscriptions as designations of that divine ruler or to understand him as speaker or recipient of such messages. Only from this point of view can a connection between text and image be established according to Düwel and Nowak. (Concerning reservations as to Hauck's iconographic interpretations, see Wicker above.)

On pp. 394–96 there is an enlightening demonstration of just how difficult it is to reach consensus on what a certain word means, even when the reading is clear. The sequence **farauisa** on IK 98 Køge/Sjælland 2 is taken as an example, interpreted as either *Fārawīsa* 'who knows the dangerous' or *Farawīsa* 'travel-wise'. These names can be made to fit either the runic magician or Óðinn (cf. the Odinic name *Gangráðr* '[literally] pace-clever'). It is good to keep in mind the complications of interpreting even the

seemingly most straight-forward runic bracteate inscription (as is also most always the case with other types of older-futhark inscriptions).

According to Düwel and Nowak (p. 401), the use of a verb in the preterite is typical of statements by secular runographers in other older runic inscriptions whereas the use of present tense verbs demonstrates the elevated, priestly function of the writer. The bracteates the authors study (pp. 398 f.) seem to conform to such a division. IK 184 Tjurkö 1 and IK 241 Äskatorp/Väsby use “practical” verbs in the preterite: *wurtē* ‘wrought’ and *fāhidō* ‘I colored (wrote)’. The present tense of verbs on other bracteates suggests that their texts — on the basis of the iconographic interpretational perspective and in combination with the particular placement of the inscription on the piece — may be understood as statements made by the god being depicted (p. 401). There are, however complications with this theory (see below).

Düwel and Nowak present (pp. 402–57) ten bracteates with runic inscriptions that are readable, interpretable, and syntactically comprehensible, although some fulfill these criteria better than others. It should be remembered that the list contains only texts of relevance to the history of religion (p. 396). Additionally included is IK 374 Undley (pp. 452–57, as an appendix to the “actually” semantically comprehensible inscriptions), as well as some ten bracteates, such as IK 260 Grumpan, with the rune row or parts thereof (pp. 457–66). The presentations are throughout excellent and solid, with heavy emphasis on Odinic aspects. Most interpretations will not be commented on here. Although the number of linguistically valid texts is greater than those presented, all of the longer texts are indeed found on the list.

IK 184 Tjurkö 1 with its thirty-seven runes belongs to the longest, as well as to the readable and semantically least problematic bracteate inscriptions (p. 403 f.). Its text is an exception in many ways and is by consensus taken to be *Wurtē rūnōr an walhakurnē Heldar Kunimundiu* ‘Heldar wrought runes on “the Welsh corn (= the golden bracteate?)” for Kunimundur’. It is probably cast in verse (p. 404; cf. Marold 2012, 80), and also otherwise has a unique position in the corpus (*Auswertung*, 405). Even though it appears we have a workman’s formula on the piece, Düwel and Nowak question whether Heldar made the actual runes, and consider it more likely (pp. 406–08) that he is the runemaster and Odinic priest, and that his name (etymologically related to both Old Norse *hjaldr* ‘warrior’ and *hildr* ‘combat’) may be compared with names of Óðinn containing elements dealing with battle. Kunimundur may then be Óðinn himself, ‘the protector of the family’.

The IK 11 Åsum and IK 340 Sønderby/Femø bracteates are iconographically very close and their inscriptions also partly similar. The latter has been read

ekfakarƿ and the former **ehēiakarƿahi**. Düwel and Nowak (pp. 430 f.) accept the proposal that these texts stem from a common original and that the name should be interpreted as *Fākar*, since *akar/ākar* is meaningless. *Fākar* is taken to be the etymon of Old Norse *fākr*, a poetic word meaning ‘horse’, and is compared to Odinic designations referring to the equine world (p. 433). An Odinic connotation is also accepted for **glīaugiru iurnrl** *Glīaugir wīu r[ū]n[ō]r l[aukar](?)* on IK 128 Nebenstedt 1, but other possibilities are also discussed (p. 438): the word may refer to the supernatural powers of the runemaster; to his performance in a priestly function during a magic cult act; to his cultic imitative representation of a god; to his bearing of an Odinic designation; and, finally, it may represent a divine self-revelation. Düwel and Nowak claim that the iconographic understanding of the figure with oversized eyes as an image of Óðinn allows for a new interpretation: *Glīaugir* is the name under which the god depicted on the bracteate carries out the consecration of the runes, which are meant to work as protective and curative defense against demons.

On IK 189 the full text is *Tawō laþōdu* ‘I prepare an invitation’. This short message may be explained in an almost unlimited number of ways. Düwel and Nowak (p. 442) agree with Gunter Müller’s interpretation of the image on the bracteate as representing “bracteate magic”, something instituted by the god himself, and that the text means that the depicted god makes an invitation; linguistically nothing contradicts this and iconographically much speaks in favor of it, according to the authors.

Inscriptions containing the rune row have traditionally been interpreted as having a magical context (cf. pp. 462–66). Düwel and Nowak take a different approach and see the complete rune row (as well as parts thereof, as *pars pro toto*) as containing every sound and character of all imaginable lexical items, including the healing words of the “Second Merseburg Charm” (see Wicker above).

In the concluding section the authors are concerned with the philological reconstruction of the original texts (*Vorlagen*) underlying two small groups of perhaps semantically interpretable inscriptions, but that will be considered in my general discussion below.

Düwel and Nowak (p. 375) justly point out the puzzle-like quality of bracteate texts; the parts of an inscription should not be interpreted in isolation but rather incorporated in the overall picture. This is an excellent principle, if applicable. Readability is defined by them as the successful identification of bracteate characters with individual runes, from whose “ideal” form the characters may deviate to a greater or lesser extent. In certain cases Düwel and Nowak (pp. 377–79) claim that runes may be positively identified even

when defective, viz. when not conforming well enough to any form-typical shape. In some cases, such as IK 156 Sievern **r̥wriḷu**, the interpretation (in this case as *r[ūnōr] wriḷu*) is said to be undisputed (p. 377). That may be so, but it is still only guesswork. It is even more problematic when readings are changed to fit with the presupposed interpretation, as when, for example, the first rune in IK 392 Gudme 2 **kuḷar** (with older < **k** placed on a vertical, thus with the form *Y*) is read as *Y f*, with a missing branch, because that is what it was “meant” to be (p. 377) and the inscription presented without reservation as **fubar** (e.g., p. 460). This confuses transliteration with normalized transcription. If the interpretation were certain, **fubar** would perhaps have been acceptable, but since the suggestion that this sequence represents the beginning of the rune row is no more than a possibility, a strict transliteration is called for.

There are other “words” on bracteates for which no satisfactory meaning has been suggested, such as IK 386 Wapno **sabar**. Perhaps here we find the *incantationes*, the magic formulas, Hrabanus Maurus claims that contemporary pagans in the North used their letters to record (cf. Grønvik 1996, 6). Or maybe we are dealing with the war chant Tacitus called *barditus* and that Frands Herschend (2005, 96–103) suggests that we find in non-lexical sequences in older runic inscriptions. I am certainly not saying that either of these hypotheses is correct, only that all possibilities must be taken into account before deciding on what we choose to proclaim as the most plausible interpretation.

As noted above, Düwel and Nowak rely heavily on the iconographic analysis of Karl Hauck. There are arguments in favor of such an approach. Even if bracteate texts could very well perform other functions than that of healing or protecting amulets, there is limited positive evidence of what those might be. In other older runic inscriptions we frequently find functionaries such as the *ḫewar*, *gudija*, and *erilar*. Only the last, however, appears in bracteate inscriptions, and then only once in the published material, on IK 241 Åskatorp⁸/Väsby, *Fāhidō wīald Wīgar ek erilar*. But here we have reason to pause. In 2009 two bracteates were found at Trollhättan, one with Roman letters (IK 638) and one with runes (IK 639), making the latter the second runic bracteate known so far from this locality (cf. IK 189). The inscription on IK 639 has not been fully interpreted yet, but is read **·eēkrilar·marīḫeubarh aitewraitalaḫo** and tentatively interpreted by Magnus Källström (2011) as

⁸ Throughout *Auswertung* the antiquated form *Eskatorp* appears (Pesch 2007, 435, has the correct form). This is understandable — who can keep track of every changing place-name form? — but in this case unfortunate since there is an Eskatorp in the province of Skåne which may be confused with the proper find-place in the province of Halland.

Ek erilar Mari-/Māriþeubar haitē, wrait alapō. Obviously, we have here a second example of *erilar*. It is clear we cannot assume that the types and contents of inscriptions we know today in any way preclude the existence of other types of text with completely different contents.

The new find IK 639 also complicates the hypothesis that verbs in the preterite are typical of runemasters performing a secular function whereas present tense verbs demonstrate their elevated, priestly function. On IK 639, verbs are used in both the present and preterite tense. Looking again at some of the present-tense verbs on bracteates, particularly IK 340 Sønderby/Femø *f[āhi]* ‘I color (write)’ (cf. IK 11 Åsum **fahi**), IK 189 Trollhättan (1) *tawō* ‘I prepare’, and possibly (see above) IK 156 Sievern *writu* ‘I write’, it is hard not to conclude — at least initially — that they too are rather “practical”.

One might think that IK 184 Tjurkö 1 with its text *Wurtē rūnōr an walhakurnē Heldar Kunimundiu* would be the pattern by which other, more “corrupt” texts would be judged, since here for once we are dealing with a complete sentence consisting of six words in a variety of syntactic relationships. This metalsmith, at least, was not incompetent (cf. above). Superficially the text seems to have nothing to do with Óðinn or the healing of Baldr’s horse. And as the only almost unproblematic text it might suggest it would be unwise to press such an interpretation on other, more problematic texts. This is not the approach taken by Düwel and Nowak. Instead of accepting that we are dealing with a person of high status in society, which even Karl Hauck thought, they propose (*Auswertung*, 406) that we should consider interpreting the names on IK 184 as referring to priests or to Óðinn (or possibly Baldr), given that other bracteates have been construed this way. Düwel and Nowak (p. 405) remind us that Gunter Müller once pointed out that the serial production of bracteates would make unlikely the appearance of a commissioner’s name in an inscription.⁹

Letter and alphabet magic

Klaus Düwel also has a chapter of his own, “Buchstabenmagie und Alphabetzauber: Zu den Inschriften der Goldbrakteaten und ihrer Funktion als Amulette” (pp. 475–523). However, it too (cf. Müller above) is a reprint of a twenty-three-year-old article (1988, with a brief postscript), and the same

⁹ This is also a strong counter-argument against the interpretations by Ottar Grønvik which involve the assumption that the bracteate message deals with very individual cultic events. For example, he takes IK 1 Ågedal to mean ‘Bondwoman, ruddy, in yuletide strength, may lead the horse to pasture’, supposedly part of a longer poem recited as a preparation for a sacrifice and burial at Ågedal (1996, 96).

reasons offered for not reviewing Müller apply here. Nevertheless, since this work on letter magic and alphabet sorcery is more relevant to runologists some important points must be mentioned.

Düwel challenges Erik Moltke's assertion that bracteate runographers were mostly illiterate (cf. above). Düwel (pp. 477 f.) asks if bracteate inscriptions really are corrupt and consist of meaningless character sequences, and wonders if the question of their intelligibility is at all appropriate. To answer these questions he investigates how Roman letters on medallions were adapted to bracteate inscriptions and replaced by runes, the function of medallions and bracteates, and the background of amulets in Late Antiquity. He also makes a structural comparison between the iconographic principles of Late Antique magic objects with inscriptions and Migration Period runic bracteates. The gradual shift from/of letters to runes is of particular importance (pp. 484–87) and is traced in detail, as well as ordered chronologically. The results, however, are affected by Morten Axboe's new datings, as Düwel points out in his postscript (p. 523).

Düwel also discusses (p. 513) the characteristics of magic words and names: (1) the obscure word as an adequate, "comprehensible" form of the desired result, thus the search for the suitable word in a glossolalic process, and (2) the obscure word as a protective disguise of the effective force in magic. The formal principle of making something arcane involves many regular methods of formation, which are also partly applicable to the process of glossolalia (i.e. the production of ecstatic, unintelligible utterances; cf. p. 519, note 181). Düwel (pp. 513 f.) lists fourteen such ways: acrostics, alphabets, anagrams, variation of initial sounds, insertion of alien letters, contractions, *notarikon* (making a new word by using another word's letters), palindromes, squares, *Schwindeschema* (arrangements of gradually disappearing sequences), suspension, substitution of syllable and letters, vowel variation, prefixed or otherwise added syllables. These phenomena are well established in classical cultures. Düwel (pp. 514–19) tries to demonstrate that most of the methods are also exemplified in bracteate runic inscriptions, although some procedural categories are only represented by one example, some by none. A seemingly certain example of the *Schwindeschema* is to be found in the varying writing of *laukar* (p. 518): **laukar, lakr, lkar, laur, lur, lr, l**.

But Düwel (p. 519) also wisely warns us against abusing the rules. Not every runic sequence may be subdivided into examples of arcane practices; such an interpretational procedure should be attempted only when the arcane character is evidenced by its systematic use within a limited set of objects and when the elements stand in a convincing relationship to iconographic elements.

Finally, Düwel (pp. 519–21) discusses glossolalia and tentatively designates as such meaningless sequences of vowels and consonants, for example **iaaeiau** on IK 70 Halsskov Overdrev and **rm!hþ** on IK 148 Sædding/Slotsgården. He rejects (p. 521) suggestions of the use of number magic on bracteates but finds that a structural comparison between magic inscriptions in Late Antiquity and texts on runic bracteates demonstrates that the latter too had the function of magical communication with superhuman powers in order to procure protection or to defend against harm. This would unambiguously confirm the amulet function of bracteates.

Düwel's survey is exemplary, and although I cannot agree with all of his results, this contribution to the subject will surely stand for a long time.

I would pose the question, nevertheless, of whether runes might be an "interpretation" of the Roman letter forms, rather than a representation of their linguistic contents. Recent work by Morten Axboe on chronology (see above) shows that there is no continuous development from Roman gold coins and medallions over Germanic imitation medallions to bracteates. This makes it doubtful whether the Roman letters were really understood or only copied, which would explain the very few meaningful sequences and the many garbled forms. It also makes it questionable whether runic words such as *laþu*, *laukar*, and *alu* are really parallels of Latin *dominus*, *pius*, and *felix*, respectively, as claimed by Anders Andrén (1991, 256). We have after all no evidence of Latin literacy among the smiths making imitation medallions and bracteates. Wilhelm Heizmann (*Auswertung*, 529 f., cf. 589) suggests that certain runic words, such as **salusalu/alu**, **ehwu/ehu**, **ota**, and the sequence **aug**, may be phonetic equivalents of *SALUS*, *EQVUS/EQVIS*, *VOTA*, and *AUG(ustus)*, respectively, but equivalents lacking a semantic connection. Even this is doubtful in my view, as is the assumption of any Latin literacy among those in the medallion and bracteate audience (cf. Nowak 2003, 671 note 11).

As for the *Schwindeschema*, I note that it is never recorded in one and the same inscription, as would be expected from its classical predecessor, nor is the disappearance really gradual: one would then have expected **laukar**, **lauka**, **lauk**, **lau**, **la**, **l**, of which only the first and last forms are (presumably) attested.

Finally, a word on amulets: Düwel's unequivocal determination of bracteates as amulets is hard to falsify since so much depends on what is meant by an "amulet". Would a rabbit's foot, a crucifix, a relic, or a club badge all be amulets? They are each carried with the objective of obtaining some sort of boon, but with very different motives and mental justifications. These artifacts represent everything from sheer superstition and magic

manipulation of natural or supernatural forces to religious symbols and aids or emblems of loyalty and group membership. Let us also not forget that things may be multifunctional.

Bracteate formulas

The last chapter devoted to bracteate inscriptions is written by Wilhelm Heizmann, and deals with “Die Formelwörter der Goldbrakteaten” (*Auswertung*, 525–601). He, too, stresses (pp. 525f. and note 5) the relationship between pictures and words when interpreting the latter, and asserts that bracteate inscriptions are unlikely to be purely secular, given that the objects are made of gold, produced in series, and seldom mention names of (human) individuals; in any case their possible function as jewelry is secondary. The bracteate concept is taken from Late Antique medallions and coins, which in the North were often worn as amulets. The names and epithets of individual emperors on medallions and coins are replaced on bracteates by various appellations for gods, primarily Óðinn. Heizmann also discusses the gradual replacement of coin and medallion letters by runes (cf. above).

The fact that only some bracteate inscriptions are semantically interpretable is also pointed out (p. 530), and it is stressed that the explanation of the lack of interpretability cannot solely be faulty copying by illiterate goldsmiths. The originator of the complex and mystical iconography on bracteates possessed great artistic creativity combined with an enormous speculative, religious talent. Heizmann (p. 531 and note 32) claims that formulaic healing words constitute the largest group within semantically interpretable inscriptions. He prefers the term *Formelwort* (‘formulaic word’) to *Einzelwort* since the latter is empty of meaning. In making this change he claims to be in opposition to Einar Lundeby and me, as well as Sean Nowak. The scholars in question, however, use the concept ‘single word’ merely to denote their object of study. Heizmann is, though, correct in championing *Formelwort* since words of this kind frequently do recur and *Einzelwort* gives the impression of a word that occurs in isolation, which is often not the case.

Heizmann (p. 532) notes that formulaic words have the following features: they consist of a small number of appellatives; their meaning is ascertainable through etymology and reflexes in later forms of the languages; they may appear alone or in groups, but commonly in the nominative singular and without syntactic context; they appear predominantly in connection with pictures of gods, which justifies assuming their content to be close to that of magic formulas and interpreting them as one-word abbreviations of such

formulas. He then goes on to discuss a number of formulaic words, **alu** (pp. 533–44), **laþu** (pp. 544–50), **laukar** (pp. 550–73), **ota** (pp. 574–77), as well as runic sequences that have been connected to formulaic words: **anoana** (p. 578), **auja** (pp. 578–81), **eh(w)u** (pp. 582–87), **salusalu** (pp. 588 f.), and **tuwatuwa** (pp. 589–93).

Heizmann (p. 544) interprets *alu* as primarily meaning ‘protection’, *laþu* (p. 550) as a coded word (*Wortchiffre*) for the summoning of helpers in animal form, and *laukar* (p. 573) as representing Óðinn’s powers of healing and regeneration. Most interesting, perhaps, is **ota**, which Heizmann (p. 576) following Düwel convincingly renders as *ōtta* ‘fear, horror’. Ottar Grønvik (1987, 155 f.) also concurred with Düwel, but further identified the word as a name for the deity depicted, which he took to be Baldr (although he also mentioned Óðinn, who in Old Norse literature is given a name of similar meaning, *Yggr*). Grønvik concluded that we have here an example of a runic inscription giving the name of the god depicted on the bracteate. I agree this is the most plausible interpretation, and Heizmann (*Auswertung*, 577) reminds us that Othinus is described as the horrendous husband of Frigga by Saxo, while Óðinn is said to cause his enemies to become *ōttafullir* ‘full of fear’ in *Ynglinga saga*, ch. 6. For all that, there is no discussion in Beck, Müller, or Düwel and Nowak of the inscription as a possible name or even as semantically meaningful.

Bracteates and Christianity

The final chapter to be considered is the contribution by Lutz E. von Padberg, “Reaktionsformen des Polytheismus im Norden auf die Expansion des Christentums im Spiegel der Goldbrakteaten” (pp. 603–34). Von Padberg readily admits (p. 606) that there are no contemporary sources indicating that Christianity was known in the area under investigation, yet he discusses at length the reaction of polytheism in the North to the advance of Christianity. The contribution has little if any relevance to runic studies. The only really concrete discussion concerns the cruciform elements found on some bracteates (pp. 612–18), which von Padberg uncritically accepts as representing Christian crosses, even though the symbol also occurs in pre-Christian iconology and thus does not necessarily indicate Christian influence.

A cruciform element appears on IK 51.1 Fakse (p. 613), for example. On this Type B bracteate three humanoids are seen, all having something in their hands or in extensions of their arms. The figure farthest to the right has a spear in his(?) left hand and a strange object proceeding from his

right, if indeed it is a hand. The object consists of three arms in a cross formation (i.e. at 90° angles to one another), each ending in a crossbar, and is connected to the “hand” by a wavering line, possibly depicting the shaft of the cross. In my view, this is a very uncertain cross. The identification is further weakened by comparison with IK 51.3 Gudme 2 of similar design (which to me seems like the “better” version); drawings of the two bracteates for easy comparison are found in Hauck’s first chapter (*Auswertung*, 46 f.). On IK 51.3, there is a proper hand in the place discussed, instead of an uncertain object. Alexandra Pesch (2007, 100) makes no mention of a cross on any of these bracteates.

According to von Padberg (*Auswertung*, 617) the first two runes in the sequence **foslau** on IK 101 Kongsvad Å — as the first and last items in the rune row — are to be equated with the *A* and *O* of the Greek alphabet as a symbolization of Christ. But, as Heizmann points out in his contribution to *Auswertung* (p. 588 note 286), the complete bracteate rune rows both end with **d**, not **o**. In addition, von Padberg (p. 618–27) wants to connect Christ the healer with the corresponding healing iconography and healing words on bracteates. All in all, this is a most speculative contribution.

Runic conclusion

Commenting on the runic contributions in a wider perspective, I would like to emphasize that the words in the older runic inscriptions, not least those on bracteates, are notoriously difficult to explain. Therefore no serious attempt to do so should be ridiculed. However, most of the runic scholars in *Auswertung* rely explicitly on the iconographic interpretations of Karl Hauck, which are referenced concisely but not evaluated critically. Whether the runologists are right thus depends on whether Hauck is.

It is quite possible, perhaps even likely, that bracteate inscriptions should be seen in a religious or ritual light. But it seems to me that this should be the result rather than the starting point of any investigation. The assumption in *Auswertung* appears to be that Hauck has proved the cultic function of bracteates and that their pictures and words must be connected. Criticism of Hauck’s understanding has been offered by various scholars, Scandinavians and Americans, as well as German-speaking writers. Critical comments have not been received constructively. At best, counter-arguments have been presented, as by Düwel and Heizmann (2009, 347–55) in a reply to criticism by Wolfgang Beck and by Robert Nedoma. Sometimes, however, defense consists solely of rejection, as when Wilhelm Heizmann (*Auswertung*, 540 note 49) calls attempts to criticize Karl Hauck’s bracteate iconography totally

unqualified, lacking professional competence, and lacking in substance, without offering any counter-arguments whatsoever.

What I think we need is an open-minded discussion of Karl Hauck's bracteate theories, acknowledging his great contributions but recognizing also the need for testing them in a scholarly manner. As for the interpretation of names and other words based on their relationship to pictures, I am not convinced that Hauck has been proved correct. Moreover, if you posit axiomatically that all names had meaning, then it is always possible to come up with some kind of cultic interpretation, no matter what the nature of the name. Even non-semantic names could be seen as onomatopoeic or cult-related in some other way. The problem here is that there is simply no way of falsifying any of the theories propounded.

To me, it is a disturbing fact that not a single name of any Scandinavian god is found on bracteates, as Heinrich Beck readily admits (*Auswertung*, 299). The explanation given is that these names were taboo and that characterizing bynames were substituted. That is, of course, entirely possible, but involves various methodological problems. Let me illustrate this complexity with discussion of just one word, the sequence **ho?ar** on IK 58 Fyn 1, a bracteate mentioned on no fewer than forty-six pages of *Auswertung*.

IK 58 is a Type C bracteate with typical ornamentation, consisting of a four-legged animal and a rider with a huge mop of hair, and also a bird. There are two other runic sequences on the bracteate, but I will disregard those for the present. Clearly separated from the other runes, placed between the head and foreleg of the animal, stands the legend **ho?ar**. The consensus reading in *Auswertung* is apparently **houar**. There seem to be two certain *u*-runes with the shape \aleph on IK 58 (one of which is reversed); the putative *u*-rune in **houar**, however, has the shape \aleph , which looks at first glance more like an **r**. It was originally read as such by Adolf Noreen and Sophus Bugge, although the latter, and following him the former, changed his mind in favor of **u** (see *DR, Text*, col. 522). *Danmarks runeindskrifter (Text*, col. 523) states that the rune in question can only be regarded as a **u**, but admits (col. 669) that the interpretation 'high' introduces phonological problems and suggests the sequence may be miswritten. Elmer Antonsen (1975, 62) is credited with the reintroduction of the reading **horar** (or rather **horaz**), and there are scholars who have followed his lead, for example Elmar Seebold (1991, 466). Lena Peterson (1994, 137) considers the reading uncertain and that the rune concerned "might very well be an **r**". One would expect the different contributors to *Auswertung* to have followed the runological expertise of Klaus Düwel and agreed on a common stance. This is not the case.

Gunter Müller (*Auswertung*, 336) adheres to the once common opinion

(Krause and Jankuhn 1966, 255) that **houar** could be a reflex of **Hauhar* ‘the high (one)’ and the etymon of the Odinic names *Hār* and *Hávi* (expressed more positively in the summaries, *Auswertung*, 698, 714).¹⁰ At one of his altogether eight citations of the transliteration, however, “[/horar]” is added.

Morten Axboe (p. 291) transliterates “**houar** oder **horar**”, and similarly Wilhelm Heizmann (p. 534) “**houar** oder **horar**”. Heizmann’s vacillation is unexpected since he firmly ruled in favor of the latter alternative more than a decade ago (2001, 329), following the lead of Heinrich Beck (2001, 67), who decided that because of its position on the bracteate the inscription had to refer to the horse, not the god (according to this interpretation we are dealing with a byname for Baldr’s horse: ‘the esteemed, the beloved’¹¹). That the related bracteate IK 300 Maglemose has the legend **ho-r** in the same position is taken by Beck (op. cit., 68) as evidence that it “without doubt” represents the same name, the dot signaling an abbreviation (no parallel to such a method of contraction is given or seems to exist; see also Nowak 2003, 305).

Karl Hauck (2002, 111) concurred with Beck’s new reading and interpretation. Klaus Düwel and Sean Nowak do not, however. They agree (*Auswertung*, 448 note 368) that the shape of the third rune alone cannot rule out the reading **horar**. Nevertheless, they (p. 376 note 6 and p. 469) opt for **houar**. The motivation is provided in a section dealing with reconstructed models of semantically interpretable inscriptions belonging to the same formula families, and is based on Alexandra Pesch’s (2007, 44)¹² groupings of

¹⁰ Grønvik (1987, 141) convincingly rejected such an etymology, although his own proposal (op. cit., 144) of a development from *Hō-warr* meaning ‘the high (noble) protector’ and designating Baldr is equally improbable since *-rr* should not be represented by **-r** alone. Later (1996, 232) he believed this to be a word /houhar/ referring to the progenitor of the family, though recognizing the unsatisfactory spelling on the bracteate. Finally (2005, 13) he abandoned the reading **houar** in favor of **horar**, acknowledging that the development of Germanic **hauha-z* via Proto-Norse **houar** to Old Norse *hār-r* or *hór-r* is extremely problematic. I do not understand why we should insist on interpretations that do not match the runic record.

¹¹ Unfortunately, we do not know the name of Baldr’s horse. In *Gylfaginning* (p. 17) eleven out of the twelve horses of the *æsir* are named, but of Baldr’s steed we are only told that it was cremated with him. None of the gods’ mounts have names that are semantically parallel to *Hórar*, however, nor do the many horses mentioned in *Þorgrímsþula* or *Alvinnsmál/Alvissmál* (*Skáldskaparmál*, 88 f.).

¹² Although in one respect I share the scepticism expressed by Svante Fischer (2009) towards Pesch’s study — in so far as it is uncritically dependent on Hauck’s “Kontextikonographie” (cf. Pesch 2007, 40) — I cannot condone his censure of the work as a whole. Pesch’s investigation into the groupings of bracteates on the basis of shared motifs is most welcome, and I have

bracteates with similar basic picture forms, although the same clustering is mentioned by Elmar Seebold (1996, 466 f.) and the group as such already by Herje Öberg (1942, 105–08). Comparing IK 58 **houar**, IK 300 **ho•r** (mentioned above), and IK 142 Randers **rahswia**, Düwel and Nowak (*Auswertung*, 472) conclude that all three emanate from a common original written **hohar**, supposedly a name for Óðinn, cf. *Hár* (see also Nowak 2003, 280 f. and note 5, 286). Since they consider that only **houar** can be graphically connected with **hohar**, the transliteration with **u** takes precedence.

Düwel and Nowak claim that the inscriptions on these three bracteates belong together because of the placement of the runic sequences in question. This shows that they are not isolated instances, nor texts exhibiting arbitrary similarities, but constitute three variations of the same original. The three bracteates are indeed closely related iconographically (cf. Pesch 2007, 152–56), but this particular family encompasses an additional six bracteates. A further two have runic writing between the head and the foreleg of the animal, but neither sequence of runes is close to the posited original ***hohar**.¹³ Furthermore, Düwel and Nowak have not accounted for the other inscriptions located elsewhere on the bracteates within the formula family. Only once are any of these even similar to one other, the exception being IK 58 and IK 300, where one of the three sequences on each piece is exactly the same, **all** (in addition to the similar sequences **ho?ar** and **ho•r**, respectively). In my view little if anything speaks in favor of a textual link between any of the other bracteates.

Instead of letting external factors decide which reading is to be preferred, the runologist can and should take a different approach. The understanding of a runic inscription can be arrived at by a systematic process of analysis, starting from the “bottom” with discrimination (of the individual graphemes), and proceeding to phonematization (of written characters into speech sounds), lexicalization (of phonemes into words), structuring (of the text), and finally the creation of propositions, i.e. how the text relates to reality (Palm 2001).

The discrimination of graphemes is not easy since the main problem in this exercise is what comparative material to use. At the very least, of course, the other graphs on the same object should be analyzed, and secondarily graphs from similar objects, in our case other bracteates. One should also consider runes in the wider corpus of the older-futhark inscriptions. In the case of

found her book immensely useful for my own purposes. She does not, perhaps, give the full credit due to her predecessors, but does nevertheless make a valuable contribution to bracteate studies.

¹³ IK 75.3 has **lurpa** and IK 163 **niuwila**.

IK 58 there is an obvious difference between what appear to be clear *u*-runes and the third rune in **ho?ar**, as well as the fourth rune in the sequence **aad?aaaliuu** on the same bracteate. In a rather neglected contribution Elmer Antonsen discusses the distinctive features of **u** and **r** (1978, 294 f.; cf. 2002, 51–71, at 64). The difference resides in the fact that the former has a full-length branch (i.e. a long nonvertical line) while the latter has a “crook” (a sharply bent line, here from top to base), and he consequently – due to the bend – chooses the transliterations **horaz** and **aadraaaliuu**, respectively. He further points out that an *r*-rune of this shape is also found on the Aquincum clasp (KJ 7). Bengt Odenstedt (1990, 37) has found it in two further inscriptions. In my view this argues strongly in favor of the reading **ho?ar**. And it should be pointed out that Düwel and Nowak themselves (*Auswertung*, 410) read a similar graph on IK 98 Køge/Sjælland as an *r*-rune.

There are, however, also graphs of very similar shape (with perhaps less sharp bends) that have been read as *u*-runes. Odenstedt (1990, 26) mentions an example on the Bülach clasp; it is found in a sequence usually transliterated **du**, but the interpretation is uncertain and cannot be used to support the choice of solution to the uncertain reading (cf. KJ 165). Another example (and there may be more) is found on IK 128 Nebenstedt 1, where the established reading of the beginning of the inscription is **gliaugir**.¹⁴ However, in the sequence **rnr** on the same bracteate (for *rūnōr* – a doubtful interpretation in my view) the graph for *r* has a distinct bend and is thus kept separate in the context of this bracteate from the *u*-rune. Internal discrimination is of primary importance, and the sequence in question on IK 58 should be read **ho?ar**, if this is in any way amenable to interpretation.

As several scholars have already pointed out (cf. Antonsen 1978, 295), there are exact correspondences to a word *hōrar* in later Germanic languages: Gothic *hors* and Old Norse *hórr* m. (possibly attested in N 353), both meaning ‘male adulterer’. This word is related to Latin *carus* ‘dear, beloved’ and has other Indo-European cognates, all with a positive connotation. Antonsen sees *hōrar* as “undoubtedly a term of endearment, or at least not a pejorative, in spite of the later development of this root to mean ‘fornicator, prostitute’”. This positive sense has been presupposed by all who accept the reading **ho?ar**. Nevertheless, it is semantically questionable to posit a favorable meaning of the kind, given that there are no traces of flattering connotations in Germanic languages. I think we need to accept the possibility of a pejorative. After all, bracteate inscriptions evidence words with a negative connotation, such as *ōtta* ‘fear, terror’ (cf. *Auswertung*, 576),

¹⁴ A reading **gliargir** is theoretically possible, of course, and interpretable too.

and the sexual sphere seems to be referred to in DR 357 Stentoft (KJ 96), DR 360 Björketorp (KJ 97, *argiu*), and perhaps in KJ 61 Kalleby (*þrawijan*).

I do not know if *hōrar* ties in with the bracteate mythology posited by Hauck and others, but it is not my task to settle this question. As a runic philologist I can only determine what is the most likely reading and whether any words exist that could be represented by such a sequence of runes. In this case I find that there is one. An indication that the interpretation *hōrar* may be correct is the sequence **ho•r**¹⁵ on IK 300 (with the dot being part of the ornamentation, as in IK 129.1 Nebenstedt 2 **llet**†**or•ri**, which is in the same formula family as IK 58 and IK 300). The sequence **hor** corresponds to Old Norse *hór* m. (gen. *hós*, acc. *hó*), a twin to *hórr* and with the same meaning (von See et al. 1997, 241f., 446), although it must have a different etymology.¹⁶ Regardless of whether the parallel to **ho•r** is valid, it is clear that the reading **hořar** and the lexicalization *hōrar* present neither runological nor etymological problems; future research will determine how this understanding may relate to reality.

Müller (*Auswertung*, 342f.) thinks that *Alawinir* on IK 161 Skodborg is a further designation for Óðinn, arguing that the *Ala-* occurs in his Old Norse name *Alfǫðr* and likewise in North-West European names of female deities (*matrones*), that Óðinn and other gods described themselves as “friends” of their protectees, and finally that theophoric names such as *Answin* and *Gudwin* contain an element meaning ‘friend’. Many objections might be raised against this reasoning: suffice it to say that this is another example of arriving at the designation of a god through simply trying to match some few pieces of the jigsaw puzzle. From Eddic and other sources we know of hundreds of epithets used for the pantheon of the North, perhaps even thousands if we add the lower echelons and the names of all mythic individuals. But as far as I know, not a single one of these words occurs in bracteate inscriptions. The would-be Odinic names, such as *Fākar*, *Gliaugir*, and *Hariūha* (further examples p. 353 with footnotes), are all thought to be derived from qualities associated with Óðinn, just as *Alawinir*.

Even if we were to accept *Alawinir* as an Odinic name, we would be left with **alawid** on the same bracteate, which does not seem to

¹⁵ It is, however, also quite possible that this sequence has no more meaning than the **tlp**†**lfhis** and **all** on the same bracteate seem to have (unless **all** is considered a corrupt form of **alu**).

¹⁶ *Hōr* would appear to be a masculine monosyllabic consonant stem with an analogical genitive -s (cf. Noreen 1923 § 412), although other vowel-ending parallels are all feminine in Old Norse: *kýr*, *sýr*, and *æř* (op. cit., § 418). If the earlier form of the word was indeed *hōr*, it would be expected to appear in Old Norse as **hór* (§ 71.4), but analogical processes within the paradigm (cf. § 72) and influence from *hórr* could most probably explain the attested shape.

be another such alias, although Müller (*Auswertung*, 345) tentatively connects it with *Víðarr*, the name of Óðinn's son. If we are to accept the endingless forms of the personal designations in the IK 161 inscription **aujalawinaujalawinaujalawinjalawid** as vocatives, which Müller (p. 342) did, a straightforward translation might be: 'Luck, Alawinir — luck, Alawinir — luck, Alawinir — good year (harvest), Alawidir', i.e. in line with the interpretation of Elmer Antonsen (1975, 77). There is nothing necessarily theophoric in these names. The concept of 'luck' was after all tremendously important in ancient times, and so were the crops. But Müller (*Auswertung*, 342) is unwilling to accept the idea that bracteate inscriptions could be directed towards humans; they have to be an invocation of the gods. This is in line with his choice of the last two among the three possible interpretations of names on bracteates (p. 337): They may represent the owner or recipient, the runemaster, or the gods connected to the pictorial contents. He did not, in my view, provide sufficient proof that the third alternative is the most likely, or even probable, in longer inscriptions. For single-word inscriptions he favored runemasters (p. 351).

Whereas Wolfgang Krause clearly preferred magic connotations and an exclusive cast of runemasters, the dominating school of interpretation today (at least in Germany) sees almost everything in a cultic light. My view, however, is that there may well be alternative explanations for the onomasticon on bracteates. There could be other societal structures that would account for the names (and other words) found on these objects. One hypothetical explanation would be that these are texts emanating from sodalities of different kinds, for example groups of warriors, or perhaps allegiances of other types, which mention a leader of some sort and the followers or allies of such a person. Bracteate inscriptions could then contain names of a chieftain, the individual to whom the bracteate was presented, or of another being that it was for some reason important to mention (divinities would of course fall within this sphere). Particularly suggestive here is the word *gaganga* 'follower' which may occur on the Undley bracteate (see Bammesberger 1991, 398–400 with references), as well as on the Kragehul lance shaft. This suggestion is only meant to demonstrate that there are other possible approaches to the runic inscriptions on bracteates.

Any study of the older runic inscriptions will of necessity be extremely difficult and its outcomes uncertain due to the limited nature of the linguistic material, its ambiguity, and our limited understanding of the activities and mentalities of the period concerned. Since many runic sequences, usually written in *scriptio continua*, may be divided up in two or more ways and almost every one of them given multiple interpretations, and most

interpretations several implications, it is obvious that we are dealing with a jigsaw puzzle of such complexity that if you move a single piece the entire picture will change. It does not help that we are not entirely sure which pieces really belong to the puzzle and that a steady stream of new pieces keep appearing.

Herje Öberg ended his important book on the gold bracteates from Scandinavia's Migration Period with a pessimistic but at that time quite accurate footnote (1942, 271 note 1): "Ännu så länge synes dock runologien, i vad mån det gäller läsandet av brakteaternas runinskrifter, i mycket stå på trevandets stadium." ("So far, however, runology seems largely to be at the fumbling stage, where the reading of bracteate inscriptions is concerned.") Clearly bracteate runology has progressed far since then, but it is still not a fully mature discipline. To achieve such stature, it must first attain scholarly "independence", i.e. it cannot be too reliant on other disciplines. The task of runology is to present an independent analysis of what a certain runic text means, not merely to serve up interpretations that fit within a given framework. Once runologists have established the possible interpretations of inscriptions on bracteates there is nothing wrong, of course, with choosing those interpretations that harmonize with the theory external to runology that has the highest explanatory value and the fewest contradictions. I cannot see, however, that runologists have tried to subject Hauck's theories to such a systematic evaluation.

The Scylla and Charybdis of runic philology are "horse sense" on the one hand and lack of prejudice on the other. To be guided by common sense is excellent, of course. If something looks too good to be true it usually is, as the saying goes, and the runologist must pay heed to the plausibility of every interpretation. On the other hand, not everything is as it seems at first glance, and common sense usually contains a fair proportion of prejudice.

Both the championing and rejection of Hauck's hypothesis of horse healing are therefore problematic. It does seem unlikely to me that a short poem preserved in only one Old High German manuscript would offer evidence of a central cultic practice so prevalent in Scandinavia many centuries earlier that it completely dominates bracteate iconography, but leaves no trace in later Scandinavian written sources. On the other hand this is not entirely impossible, and Hauck and others have presented some intriguing analyses of the pictorial contents of bracteates. However, his hypothesis has in my opinion been accepted (and sometimes rejected) uncritically, and though I regret to say it, it is clear that all of his work needs to be checked carefully by appropriate specialists.

I am not a specialist in iconography, and do not presume to decide whether

images on bracteates represent Óðinn and/or any other Northern god, other mythic beings, human dignitaries or cultic functionaries, or something else altogether. I do, however, question the necessity of believing there existed a close link between these images and the texts appearing on certain of the bracteates in question. Such a link certainly did exist in the case of the Roman medallions that bracteates originally emulated. But just as the images changed in form and presumably also in content, so the Latin language inscriptions were transformed — at first into something almost certainly without lexical meaning to judge from the twenty-six or so models with Roman letters or imitations thereof (*Auswertung*, 290–95), none of which seem to carry any clear linguistic message. It is perfectly conceivable that the lexically meaningful inscriptions in runes that start appearing on bracteates have no connection to the pictorial contents. The way to find out whether they do is to study their linguistic contents with an unprejudiced attitude in order to see if the texts add to the pictures or not. The runic scholars in *Auswertung* seem to be confident that such augmentation is present. I cannot agree. Every single assertion of a textual-pictorial connection requires the imagination to be stretched to a degree that seems unacceptable. Sometimes even the data itself has to be adjusted to reach a certain result, as when readings are “corrected” to come up with the desired solution.

The axiom that bracteate texts and pictures are of necessity connected has, in my opinion, not been demonstrated. Such a connection does not seem to manifest itself on Viking Age runestones, where figurative art often accompanies the inscriptions. The comparison is not entirely valid, however, since time and genre differ to such a degree. But if there were indeed a firm connection between image and text on bracteates, one would have expected to find at least some clear instances. Instead, the opposite seems to be true. When, for once, we have what seems to be a very clear message on a bracteate (IK 184 Tjurkö 1), we find absolutely no link between text and image. There we can read that Haldar wrought the runes on the “Welsh corn” (the gold bracteate?) for Kunimundur. The piece shows a horned, four-legged horse(?) and the expected head with a fancy mop of hair above, and in addition a bird. It is not assigned to any family by Pesch (cf. 2007, 431), and by Öberg (1942, 76) only with doubt to his group C IV, the birdlike termination of the coiffure precluding a definite classification. But all the key elements of the iconography are there, elsewhere interpreted as Óðinn healing Baldr’s horse (see IK 58 etc. above). Yet, in the inscription we find no mention of any of the gods thought to be found on so many similar bracteates (unless we accept Düwel and Nowak’s daring interpretation of the names as referring to priests or to Óðinn, see above).

Another textual argument against the interpretation of bracteate inscriptions as supplementing the divine iconography is Karl Popper's falsifiability criterion. How would we show that bracteate pictures definitely do not represent Óðinn and his fellow gods? One method might be to bring in older runic inscriptions not on bracteates. On the possibly contemporary Mjöbro stone (KJ 99), for example, there is a picture of a horse and rider, the latter equipped with a shieldlike object and brandishing an implement of some sort (spear or sword?) and accompanied by two canines. The inscription may be translated "Frawaradar from Hå is killed" or (in my view less likely) "Frawaradar is killed at Hå" (cf. Fridell 2009, 102). Now, the pictorial representation could very easily be taken to show Óðinn with his horse Sleipnir, his spear Gungnir and his wolves Geri and Freki. And the name *Frawaradar* could equally easily be seen as a designation of Óðinn, cf. the first element in his *heiti* (poetic synonym) *Fráriðr* 'the fast rider' (Peterson 1994, 152 f.) and the last in his names *Gagnráðr*, *Gangráðr*, and *Hvatráðr* '[literally] quick-witted'. In my view no runic object has more Odinic overtones. Yet no one to my knowledge has suggested that the rider on the Mjöbro stone represents Óðinn, nor has anyone proposed that *Frawaradar* is a designation for him.

Paradoxically, the greater a scholarly achievement, the bigger a danger it is to its own discipline. The reason for this is that such a *magnum opus* will dominate for many years and, to some extent, preclude similar efforts. Any and all mistakes or bad calls of judgment in such a work will also be made more or less permanent. Scholars outside the field will tend to quote authoritative editions even when they are out of date. One example is Wolfgang Krause and Herbert Jankuhn's *Die Runeninschriften im älteren Futhark* (1966), the hitherto best edition of the older runic inscriptions. In the Scandinavian Runic Text Database there are 270 older inscriptions, of which only 95 are found in Krause and Jankuhn's book. As a corpus edition it is now very incomplete. The pictures were in some cases outdated even when it was published (Williams 1992, 194 f.) and many of the interpretations have been revised. Yet, the effort necessary to produce a new corpus edition has so far precluded its realization, although a project to do so is now under way (Zimmermann 2012, 220 f.).

When publishing such monumental works as *IK*, it is necessary to keep the above-mentioned paradox in mind and thus to ensure that whatever is published is of the highest quality attainable and as certain as possible, and thus likely to have staying power. When producing corpus editions, it is preferable to separate description and analysis from each other. Such a procedure makes the publication easier to use and ensures that the description

will have value even if or when the analysis is no longer considered valid. In this respect the volume under review could have been better organized. The catalogue and the plates alone total 238 pages. If published separately they would have been much more easy to use.

The individual contributions are too independent of each other for a volume claiming to be an “evaluation” (rather than, for example, a conference report). The main criticism to be leveled against *Auswertung*, however, is its lack of an unprejudiced systematic approach. In other words, there is still room for a structured, general overview of the gold bracteates and their inscriptions. That having been said, the sheer amount of effort that has gone into the present endeavor deserves respect. It would be a huge mistake to ignore the contributions made by the participating scholars.

A final word of caution: the bracteate corpus continues to grow and new finds may alter our concepts radically. Wilhelm Holmqvist estimated the total number of these objects once in existence to have been around 100,000 (Pesch 2007, 9 note 1). This is not an unlikely number and simply boggles the mind. The sheer quantity of the potential material should serve as a warning to tread carefully before making definitive claims about the bracteate phenomenon.

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Reviews

Solveig Möllenberg. *Tradition und Transfer in spätgermanischer Zeit: Süddeutsches, englisches und skandinavisches Fundgut des 6. Jahrhunderts*. Ergänzungsbände zum Reallexikon der Germanischen Altertumskunde 76. Berlin and Boston: Walter de Gruyter, 2011. ix + 265 pp., 45 plates. ISBN 978-3-11-025579-9. e-ISBN 978-3-11-025580-5. ISSN 1866-7678. € 109.95.

Reviewed by Martin Hannes Graf

Die 2010 am Institut für Ur- und Frühgeschichte der Ruhr-Universität Bochum angenommene Dissertation trägt einen wohlklingenden und vielversprechenden Titel. Gleichwohl sorgt er terminologisch für Irritation: Die Begriffe „Tradition“ und „Transfer“ werden nämlich in der „Einleitung“ S. 1 kurz erläutert, anschließend spielt aber zumindest der Begriff „Transfer“ in der ganzen Arbeit keine Rolle mehr. Der Terminus „spätgermanisch“—er mag vielleicht unter Archäologen gebräuchlich sein und eindeutig das 6. Jahrhundert benennen—erscheint im Text sogar überhaupt nie. Nun mag man einwenden, dass der Titel vielleicht nur der findigen Feder eines Verlagsverantwortlichen entstammt, oder dass es auf Titel ja nicht so sehr ankommt. Dem sei stattgegeben, denn der Autorin geht es tatsächlich sehr zentral um die im Untertitel formulierte Frage, in welchem Verhältnis vergleichbares süddeutsches, skandinavisches und englisches Fundgut des 6. Jahrhunderts zu einander steht, und wie dieses „Vergleichbare“ zu seinem sonderbaren Status kommt. Aber jenes Manko des Unverbindlichen, das sich schon im Titel abzeichnet, bleibt unterschwellig kennzeichnend für die ganze Arbeit: Dem intelligenten und anspruchsvollen Fragenkomplex rund um die Interpretation vergleichbarer archäologischer Kulturausprägungen folgen kaum prägnant formulierte Antworten. Vielleicht liegt dies daran, dass die Dissertation im wesentlichen als Literatarbeit konzipiert ist, und als solche gelangt sie nur punktuell zu wirklich operationalisierbaren Ergebnissen, etwa in den prägnanten „Schlussbetrachtungen“ oder „Zusammenfassungen“ am Ende der jeweiligen (Unter-)Kapitel. Da runologische Fragen (insbesondere übergreifend-theoretischer Natur) einen zentralen Aspekt der Untersuchung ausmachen und die Runenfunde auch im Katalog- und Tafelteil prominent hervortreten, sind die Lücken in der runologischen Literaturliste jedoch etwas störend. Beispielsweise kommt heute keine Beschäftigung mit den südgermanischen Runeninschriften mehr um die (von Möllenberg nicht zurate gezogene) Habilitationsschrift von Robert Nedoma (2004) herum. Dass der allgemeine Eindruck der Studie ein zunächst etwas befremdlicher ist, mag auch daran liegen, dass sie sprachlich nicht so recht überzeugt: Viele grenzwertige Erscheinungen im Satzbau sowie ungezählte Interpunktionsfehler stimmen den Leser nicht gerade wohlwollend.

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Dem eingangs angesprochenen Fragenkomplex geht die Autorin nach, indem sie die Arbeit mit einer theoretisch fundierten Beschreibung verschiedener Kulturraum-Modelle (aus Soziologie, Ethnologie und Archäologie) einleitet und zu zeigen versucht, dass die traditionellen „Einfluss“-Modelle weniger zu leisten vermögen als etwa die Modelle von Kommunikations- oder Symbolräumen. Dieser knappe theoretische Teil (S. 4–17) liegt ganz im Trend der letzten zehn Jahre Diskussion über die „Germanen“, die zu dem Schluss zu kommen scheint, dass sowohl die ethnische als auch die politische Interpretation archäologischer Kulturen zugunsten der Aufschlüsselung eher sozialer oder symbolischer Identitäten aufgegeben werden soll. Die Debatte, die nun vermehrt (und gelegentlich erbittert; vgl. Kaiser 2006) auch in der traditionellen Geschichtswissenschaft geführt wird und jüngst auch Eingang in die sprachliche Diskussion gefunden hat (Pohl und Zeller 2012), ist ganz am Puls der Zeit, und es ist Solveig Möllenberg daher hoch anzurechnen, dass sie sie sachlich und verständlich präsentiert, ohne in den neuerdings gepflegten hermetischen, kulturtheoretisch aufgeladenen Jargon zu verfallen. Dieses nüchterne, beschreibend-präsentierende Verfahren führt die Autorin auch in den folgenden Kapiteln weiter. Den Beginn macht das eigentlich zentrale und längste Kapitel des Buches über „Gemeinsame Fundgattungen“ (S. 18–116). Darunter werden die klassischen Zeugen für das zu besprechende Phänomen einer ausführlichen Befragung unterzogen; es sind dies: (a) Bügelfibeln mit rechteckiger Kopfplatte und barockem/rhombischem Fuß (der sogenannte „nordische Typ“), (b) Brakteaten und Pressbleche, (c) Ringschwerter, (d) Runen und (e) Webschwerter und Westlandkessel. Möllenberg möchte hier zeigen, dass die Gemeinsamkeiten, die sich im Hinblick auf diese Fundgruppen erweisen, nicht nur aufgrund eines wie auch immer gearteten „Einflusses“ oder im Rahmen von Import-/Export-Beziehungen erklären lassen. Vielmehr sollen die stets einer gehobenen Sphäre angehörenden Gegenstände eine Art selbstgemachter Internationalität ausdrücken, die die prinzipielle regionale Eigenständigkeit jedoch wenig tangiert. Was die Runen angeht, sind zwei Feststellungen erwähnenswert: (i) Bei mehr oder weniger isolierter Betrachtung des 6. Jahrhunderts fällt auf, dass die kontinentalen („süddeutschen“) und englischen Inschriften zahlenmäßig den skandinavischen weit überlegen sind. Für den skandinavischen Raum kann Möllenberg nur eine Handvoll norwegischer und eine schwedische Inschrift anführen, die Brakteaten sind kaum mehr der Rede wert; aus Dänemark sind keine vergleichbaren Inschriften bekannt. (ii) Unter einem zeitlich erweiterten Blickwinkel nehmen die englischen Inschriften eine interessante Scharnierfunktion ein: Zum einen stehen sie in der Frühzeit (5. und 6. Jahrhundert) unter starkem südsandinavischen Einfluss, koppeln sich davon jedoch in der Folge los, zum andern scheinen sie ab dem 7. Jahrhundert selbst die südgermanische Form der doppelt quergestrichenen **h**-Rune (**ᚱ**) zu übernehmen. Unter dem richtigen Verweis darauf, dass die thüringische Vermittlung der Runenkultur von Südskandinavien in den südgermanischen Raum als obsolet zu gelten hat, nimmt der kontinentalgermanische Runenhorizont diesbezüglich also eine Schlüsselposition ein: Wurde er zunächst von der angelsächsischen Kultur befruchtet, um ein gutes Jahrhundert später diesem wiederum zu neuen Ausprägungen zu verhelfen? Wir wissen es nicht, und auch Solveig Möllenberg

gibt darauf direkt keine schlüssigen Antworten. Eines scheint jedoch klar, das stellt die Autorin im kurzen, darauf folgenden Kapitel („Nordischer Einfluss in Süddeutschland?“, S. 117–140) heraus: Direkte Beziehungen zwischen Süddeutschland und (Süd-)Skandinavien sind in den meisten Fällen, wo man sie gerne annehmen möchte, unwahrscheinlich. Möchte man überhaupt von Beziehungen sprechen, so sind diejenigen zwischen Skandinavien und England sowie diejenigen zwischen Süddeutschland und England in jedem Fall stärker ausgeprägt als diejenigen zwischen Skandinavien und Süddeutschland. Dass als zusätzliches Scharnier das fränkische Gallien in Erscheinung treten konnte, wird allerdings nicht angesprochen; hierzu wären die von Fischer (2008, 30–32) hinsichtlich der Runenkultur herausgearbeiteten insular-kontinentalen Beziehungen am Ärmelkanal weiterzuverfolgen. Das letzte Analysekapitel des Buchs („Die Kommunikation im 6. Jahrhundert: Sprache, Schrift und Bild“, S. 159–168) spannt einen weiten Bogen von der (an sich nicht mehr nötigen, da längst erfolgten) Dekonstruktion des Maurerschen und Kolbschen Narrativs des alemannisch-nordischen Bezugsraumes, über den kulturgeschichtlichen Stellenwert von Schrift, den Gebrauchsrahmen des Tierstils I bis hin zur im engeren Sinne geschichtswissenschaftlichen Interpretation des ganzen Fragenkomplexes. Über die erneute Behandlung von Maurer und Kolb mag man geteilter Meinung sein; im Rahmen einer wissenschaftsgeschichtlichen Sichtweise hat sie sicherlich ihre Berechtigung. Es folgen lesenswerte Ausführungen über den Stellenwert von Schrift in frühgeschichtlichen Kulturen, die allerdings keine wesentlich neuen Erkenntnisse bringen. Dass Schrift jedenfalls „irgendwo zwischen alltäglichem Profangebrauch und Exklusivität angesiedelt zu sein“ (S. 164), ist bestenfalls eine Binsenwahrheit. Wichtiger ist die Feststellung, dass Schrift ein über die ethnischen Zusammenhänge hinaus wirkendes „Gemeinschaft stiftendes Moment“ (S. 165) haben konnte, nämlich über eine Exklusivität, die sich zudem auch über das Ostendieren von Tierstil I in Szene setzen konnte, in einer Art „Corporate Design“, wie es auch in der Brakteatenkultur zum Ausdruck kommt (S. 67, nach Pesch 2007). Inwieweit die geschichtlich nachgewiesenen kontinental-angelsächsischen Beziehungen im Rahmen von Exogamie und Gelehrtenaustausch im Hinblick auf das Untersuchungsthema relevant sind, bleibt bei Möllenberg blass bzw. wird nur unter Bezugnahme auf die Arbeit von Annethe Lohaus (1974) referiert. Im Fokus steht hier jedoch der durchaus als peripher zu wertende alemannische Raum, und es wäre reizvoll, ihn vor dem Hintergrund der Einrichtung fränkischer Stützpunkte im frühen 6. Jahrhundert in den Blick zu nehmen. Es scheint nämlich, dass gerade periphere Eliten für die Integration des fränkischen Reichsgefüges von einiger Wichtigkeit waren. Was im 8. Jahrhundert mit der Zusammensetzung des karolingischen Gelehrtenapparats am Hofe seinen Höhepunkt erlebte, könnte durchaus schon im 6. Jahrhundert angelegt gewesen sein. Zwar wissen wir in der Tat wenig Konkretes über die personale Mobilität im 6. Jahrhundert, besonders was den Handel angeht. Immerhin vermitteln aber die reichhaltige innerfränkische Perspektive mit Berichten etwa über Truppenverschiebungen und Reliquientranslationen, sodann die Kenntnis von Migrationsbewegungen und die darüber hinaus anhaltenden Rückkoppelungserscheinungen einige Anhaltspunkte zur Bewertung

„spätgermanischer“ Mobilität. Insofern bleibt das Ergebnis der Arbeit, das unter „Zusammenfassung und Ausblick“ (S. 169 f.) referiert wird, doch etwas einseitig. Es gilt heute als chic, Handel und Personenmobilität als Faktoren für überregional auftauchende Fundzusammenhänge abzuwerten (beides sei „in vielen Fällen schwer oder gar nicht nachweisbar“, S. 169) und stattdessen kulturtheoretisch und soziologisch begründete Modelle einzusetzen wie im vorliegenden Fall mit dem sozial konstituierten „Symbolraum“. Beides hat wohl seine Berechtigung, und die Schlüsse von Möllenberg seien dadurch auch gar nicht in Frage gestellt. Wenn jedoch trotzdem immer wieder auf die Bedingungen hingewiesen wird, die diese Parallelitäten begünstigten — das Nachwirken römischer Traditionen oder die nachgewiesenen personalen Kontakte zwischen Insel und Kontinent — so bleibt Möllenbergs Angebot doch zu unverbindlich, wenn sie auf die Frage nach dem Grund für die Parallelitäten angibt: „Es sind die Räume selbst, die diese Parallelitäten hervorbringen, sei es ganz konkret im geografischen Sinn, oder aber im übertragenen Sinn als soziale Räume“ (S. 169).

Ein umfangreicher Katalog („Runenfunde und Funde mit ‚nordischem Einfluss‘ in Süddeutschland“, S. 171–239), das Literaturverzeichnis, fünf kleine Register sowie der ebenfalls umfangreiche Tafelteil (leider sämtliche Abbildungen ohne Maßstab) runden das Buch ab.

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Lena Peterson. “En **brisi** vas **lina** sunn, en **lini** vas **unar** sunn ... En þa **barlaf** ...”: *Etymologiska studier över fyra personnamn på Malsta- och Sunnåstenarna i Hälsingland*. Runica et Mediævalia, Opuscula 15. Stockholm: Sällskapet Runica et Mediævalia, 2012. 93 pp. ISBN 978-91-88568-53-3. ISSN 1103-7970. SEK 100.

Reviewed by Staffan Fridell

Lena Peterson har med denna lilla, men innehållsrika skrift summerat och troligen satt punkt för en lång diskussion om ett antal svårförklarade personnamn på två vikingatida runstenar från Hälsingland, Malsta- och Sunnånskrifterna. De slutsatser hon kommer fram till är väl underbyggda och kan knappast ifrågasättas i grunden. Däremot finns, som alltid, ett antal randanmärkningar för en recensent att göra (den första av dessa gäller förstås den onödigt långa och krångliga boktiteln, som kommer att försvåra för både bibliotek och citerande forskare).

De två inskrifterna är ristade med så kallade stavlösa runor och Peterson ger till en början en värdefull sammanfattning av sin läsning (translitterering) av texterna. Det finns även fotografier av inskrifterna, som dock vad avser Malstainskriften är något otydligare än de foton som fanns i en tidigare artikel av samma författare (“The Graphemic System of the Staveless Runes”, i *Proceedings of the Third International Symposium on Runes and Runic Inscriptions*, 1994, s. 225).

Boken är sedan kapitelindelad efter de behandlade personnamnen. Det första diskuterar namnet **brisi** (nom.), **brisa** (gen.). Peterson argumenterar övertygande emot Sven B. F. Janssons förslag att namnet skulle motsvara ett fornvästnordiska *Bresi*, belagt från *Landnámabók*. Detta förklaras i stället som en på Irland uppkommen metateserad form av det relativt vanliga nordiska namnet *Bersi*. Namnet **brisi** är däremot sannolikt en bildning till det norsk-isländska verbet *brisa* ’läga, blossa, flamma upp, glimra, glänsa’ (finns både med kort och långt *i* med likartad betydelse). Här hade betydelsen av de norska verben (med avledningar) ännu bättre kunna beläggas om *Norsk ordbok* (vol. 1, 1966) hade citerats i stället för Ivar Aasens *Norsk Ordbog* (1873) och Hans Ross’ *Norsk Ordbog* (1895). Peterson stannar för att i första hand anta ett långstavigt *Brīsi* bildat som nomen agentis till verbet i en överförd betydelse ’glänsa, pråla, stoltsera’, men lämnar möjligheterna öppna både för en kortstavig bildning och för att namnet är direkt bildat till en motsvarighet till något av de belagda substantiven norska *brise* eller isländska *brísi*, båda med betydelsen ’eld’. Ordgruppen har en tydlig västlig utbredning i Norge, men hon menar att det välkända västnordiska språkliga inflytandet på de hälsingska dialekterna ändå kan motivera antagandet att verben med avledningar funnits även i Hälsingland under vikingatid (s. 28, 70).

Namnet **lini** (nom.), **lina** (gen.) antas vara bildat till bestämd form av adjektivet fornvästnordiska *linr*, fornsvenska *lin*, nutida svenska *len*, i en överförd betydelse om person ’mild, saktmodig’. En del av argumentationen för detta innefattar en värdefull genomgång av hur etymologiskt initialt *hl-*, *hn-* och *hr-* skrivs i nordiska vikingatida runinskrifter. Peterson drar den försiktiga slutsatsen att ett eventuellt äldre *hl-* borde ha bevarats och betecknats i Malstainskriften. Som en teoretisk — men

betydligt mindre sannolik — alternativ tolkning till *Lini* anges ett namn bildat till det poetiska ordet fornvästnordiska *linni* 'orm'.

De förmodade namnen *Lini* och *Brīsi* (på far och son) har alltså det gemensamt att de anger karaktärsegenskaper hos personer och Peterson antar att det är fråga om två (kontrasterande?) binamn i absolut användning (s. 29f.). Detta är en mycket tilltalande tanke. Namntypen är sannolikt vanligare än vad man brukar anta och man borde oftare räkna med absoluta binamn när man stöter på ovanliga personnamn i runinskrifter.

Det mest svårtolkade namnet, som därmed också får störst textutrymme, är **un** (nom., ack.), **unār** (gen.). Det är belagt både i Malsta- och Sunnånskriften, troligen på den försvunna Hudiksvallsstenen (även den med stavlösa runor), samt dessutom i fyra andra vikingatida runinskrifter från Öland, Östergötland, Södermanland och Medelpad. Kärnfrågan är om namnet innehåller långt eller kort *n*, dvs. **Unnr* > *Unn* eller **Unr* > *Unn* i nominativ. För att komma fram till en lösning gör Peterson en genomgång av frekvens, kronologi och geografi för namnen *Une*, *Unne*, *Una* och *Unna* i Norden, vilka eventuellt kan vara besläktade bildningar (till samma ord eller rot). Huvudresultatet är att det medeltida namnet *Unne* i Sverige väsentligen är sydligt och knutet till högreståndskretsar, medan *Une* under samma period är ett typiskt bondenamn med nordlig utbredning. Därav följer enligt Peterson att runnamnet **uni** (nom.), **una** (gen., ack.), som finns i fem uppländska inskrifter och en gotländsk, troligen också är ett kortstavigt *Uni*. Detta talar i sin tur för att även **un** ska förstås som ett kortstavigt *Unr* (nom.). Såväl **un** **Unr* som **uni** *Uni* antas höra samman med verbet *una* 'trivas, vara tillfreds'. Närmare bestämt menar hon "att man kunde förstå *Uni* som en svag utvidgning av ett starkt böjt **Unr*, *Unn*" (s. 58). Ordbildningsmässigt antas det senare namnet vara ett adjektiv urnordiska **wunuz* 'som trivs, känner sig tillfreds', bildat till verbet. En passant föreslår Peterson att även de västgermanska personnamnslederna fornhögtyska *Wunni*-, fornengelska *Wyn(n)*-, *Wun*-, *-wynn* skulle kunna innehålla samma adjektiv (med sekundär *ja-/jō*-stamsböjning).

Slutsatsen förefaller rimlig vad gäller **un**, men för **uni** måste man enligt min mening även som ett likställt alternativ räkna med möjligheten av en hypokorism *Unni* till sammansatta mansnamn på *Unn*-, såsom de i svenska runinskrifter belagda *Unnulf* och *Unnvaldr*. Detta alternativ nämns i början av diskussionen (s. 44, 49f.), men försvinner sedan utan övertygande motivering varför.

För att ytterligare belysa relationen mellan *Une* och *Unne* görs en genomgång av ortnamn på *-stad* och *-torp* i Sverige som kan tänkas innehålla något av dessa mansnamn som förled. Det finns flera tydliga fall av *Unastadhir*, som torde ha genitiv av antingen *Uni* eller **Unr* som förled, däremot tydligen inga **Unnastadhir*. Parentetiskt må här inskjutas att *Ånestad* vid Norrköping, som skrivs *Onastadhum* 1401, *onestadum* 1452, inte gärna kan innehålla *Une* eller **Unr*, som Peterson hävdar, utan i stället säkerligen mansnamnet *Ane*, vilket *Ortnamnen i Östergötlands län* (Ortnamnsarkivet i Uppsala, Skrifter, ser. A, 15, s. 59) också menar. Ett långt /å/ betecknas ju ofta med <o> under 1300-talets slut och 1400-talet.

Slutligen tolkas i ett kapitel relativt kortfattat kvinnonamnet **barlaf** (nom.)

från Malstastenen. Det ges två rimliga och möjliga tolkningsalternativ: *Berglōf* och *Berglæif*. Helt utan motivering väljer Peterson att av dessa prioritera *Berglōf*. I samband med **barlaf** säger hon: ”Utelämnad beteckning för frikativan [ɣ] förekommer i inskrifterna inte så sällan just efter /r/” (s. 66); ”Exempel på utelämnad beteckning för frikativan [ɣ] just efter /r/ finns det gott om i runinskrifterna” (s. 73). Jag menar att man hellre borde uppfatta sådana fall som exempel på att ett bortfall i uttalet faktiskt noteras även i skrift, i enlighet med principen ”läs som det står”.

Boken avslutas med en ovanligt lång och utförlig sammanfattning.

De fyra namnen kan, som sagt, i och med denna bok nog betraktas som säkert och slutgiltigt tolkade: **brisi** *Brīsi*, **lini** *Lini*, **un** *Unn* < **Unr*, **barlaf** *Berglōf* eller *Berglæif*. Därmed återstår inte särskilt mycket av oklarheter i tolkningen av Malsta- och Sunnåinskrifterna. Skrivningen **nur i uika** har övertygande tolkats av Evert Salberger (i *Ortnamnssällskapets i Uppsala årsskrift* 1993)—i anslutning till Otto von Friesen—som *norðr í vega* ’norröver, norrut’ och de tre ortnamnen **balas-in**, **lanakri** och **fiprasiu** är säkert identifierade som *Balsten*, *Lönnånger* och *Färdsjö* (se senast Stefan Brink i *Tor* 26, 1994, och Lennart Hagåsen i *Namn från land och stad*, 2008, 71–82).

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