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Which came first, \mathcal{M} or \square ?

Bengt Odenstedt, On the Origin and Early History of the Runic Script. Typology and Graphic Variation in the Older Futhark. Uppsala 1990. (Acta Academiae Regiae Gustavi Adolphi. 59.) ISBN 91-85352-20-9. ISSN 0065-0897.

The interest in the oldest runes has been immense ever since their first decipherment by J. H. Bredsdorff in 1839. No less than four works have been published which deal with the language in the inscriptions with this original, 24-letter runic alphabet, the so-called *futhark* (Burg 1885, Alexander Jóhannesson 1923, Krause 1971, Antonsen 1975). The number of articles and even monographs, dealing with a single or a few of the more than 200 inscriptions, is vast, and there is at least one fullfledged attempt at a corpus edition: Wolfgang Krause's *Die Runeninschriften im älteren Futhark* (1966).

The main part of the runological work has been directed towards interpreting the inscriptions. The basis for an interpretation must be the reading of the runes, and there can be no reading without a discussion of the graphs that occur. A great deal of the research that has not primarily been concerned with the language of the inscriptions, has instead focussed on the origin of the *futhark*, i.e. questions such as where it was invented, when, and by whom. None of these questions can ever be answered without a full understanding of the individual forms of the runes: these forms should give us the clue to the question on which alphabet(s) the runes were modelled.

It is thus quite clear that a monograph on the origin and early history of the runic script with special reference to the typology and graphic variation in the older runic alphabet is potentially of the greatest interest, but it is indeed a considerable task to be undertaken.

Within the last few years, two books have appeared with this ambition: Richard Morris's *Runic and Mediterranean Epigraphy* in 1988, which will not be reviewed here but which must be mentioned in the discussion occasionally, and Bengt Odenstedt's *On the Origin and Early History of the Runic Script. Typology and Graphic Variation in the Older Futhark* in 1990. These books approach almost the same problems from two very different angles and can thus be said to supplement each other.

Bengt Odenstedt's book has two aims. It is primarily an attempt "to give a detailed account" of "the various *forms* of the older runes" and "the chronological and geographical distribution of the different allographs in Scandinavia, on the Continent, in England and Frisia" (p. 9). Having studied this, Odenstedt claims to be able to establish the fact that among "the runic forms [---] there is actually considerable graphic variation" (*ibid.*). Furthermore, he states that rune forms can be used to date inscriptions, if used with caution. The second purpose of the book is to discuss the origin of the runic script, which is done in the last chapter of the book and will be discussed towards the end of this review.

The book consists of 21 chapters, whereof 20 are devoted to the study of rune forms. A preface and Chapter 1, the introduction, are followed by 17 chapters, each of which deals with an individual rune with a more complicated form. The uncomplicated runes are treated briefly in Chapter 19 which precedes a summary of the results in Chapter 20. The organisation is clear and lucid and so is the table of contents, even though one notes that certain subsections are listed (e.g. 1.3 Material) but not others (e.g. 2.1 [The *u*-rune] In the period 175–400). Obviously it was felt that a listing of the subsections under each "rune chapter" would be repetitious.

Odenstedt's study is based on the runes that occur in altogether 178 inscriptions. The ambition is to include all (epigraphic) runic writing c. 175–c. 750 A.D. This means that inscriptions of South Germanic as well as Frisian and Old English provenance are included, which is one of the great advantages of the book (see also the convincing discussion p. 140 ff.). On the other hand Odenstedt's use of the geographic and "ethnic" terms is imprecise. He states that, with the exception of the Øvre Stabu spearhead, "all the oldest inscriptions [...] have been found in Denmark" (p. 12), which is either incorrect (cf. Thorsberg in Germany and Gårdlösa in Sweden) or must here mean 'on former Danish territory'. Likewise, one wonders how the "ethnic and geographical distribution" (p. 16) was defined.

The time limit 175–750 presents some problems. The only early inscription excluded is the Meldorf fibula, which Odenstedt thinks is unrunic (pp. 53 f., 148 f., 169; see also Odenstedt 1983). As for the exclusion of transitional inscriptions such as the Sölvesborg, Sparlösa, and Ällestad stones and perhaps the Rök stone, the matter should at least have been discussed. Odenstedt has included the Etelhem fibula, of disputed reliability, but at least with a justification (note 30 p. 177). The inscriptions are (p. 17) subdivided into five groups: "A: Scandinavian and Gothic inscriptions dating from 175 to 400; B: Scandinavian inscriptions dating from 400 to 750; C: Continental inscriptions dating from 400 to 750; D: (i): English inscriptions dating from 400 to 750; (ii): Frisian inscriptions dating from 400 to 750." Each chapter on rune forms presents the material under these categories.

The corpus is mainly taken from Krause 1966 (the English inscriptions are generally found in Page 1973), but other sources are used as well. With few exceptions, Odenstedt seems to have covered the area well. He does, however, exclude some inscriptions or individual runes from his investigation: when good photographs or modern drawings could not be obtained (pp. 14, 16f.); when a rune could not “be seen clearly in the photographs” (p. 14); all bracteates except for the ones with a *futhark* inscription “because they are frequently impossible to interpret and often contain a number of highly individual or distorted runic forms”; all English coins; inscriptions of disputed authenticity (p. 17).

The first two and the last of these omissions are understandable but, at the same time, Odenstedt does not carry out his own intentions.

I would dispute the quality of some of “the excellent photographs” (p. 14; see also below), but more important is the observation that Odenstedt seems to count the drawings of, for example, the Gummarp and Bratsberg stones, as well as the Gallehus horn, as “modern”, when these were in fact produced in the 17th, early 19th, and 18th cent., respectively. The “drawing” of the Saude stone is really a mid-17th cent. attempt to copy the inscription (only known from this one source) with printed letter types, which Odenstedt consequently takes as the basis of his study.

It is also obvious from a study of the photographs that many more runes should have been excluded, i.e. difficult or even obliterated passages on, for example, the Himmelstalund rock inscription, the Årstad, By, and Valsfjord stones, and the Næsbjerg fibula (see further below). By their inclusion Odenstedt’s results are made less convincing as he bases them on rune forms that cannot be distinguished properly.

Despite the elimination of inscriptions of disputed authenticity, the (Kleines) Schulerloch rock inscriptions are included (see p. 28 with note 7, p. 121; p. 131). He seems to agree that they are forgeries, but why then discuss their rune forms?

It is difficult to understand why Odenstedt has chosen to leave out almost all of the bracteates. These contain not so few interpretable inscriptions and the number of clear, and hence usable, rune forms in the unintelligible inscriptions is vast. They would have been no more difficult to study than inscriptions of the same kind on other types of objects. This objection is even more serious because of the fact that the bracteates make up a large group of *datable* inscriptions – a discussion of these would have been extremely valuable in this study. The existing corpus of inscriptions is so small that one should only exclude a part of it for very good reasons. To leave out a substantial group will seriously imperil the results.

Finally, if English coins are barred from the study, why are three Frisian *solidi* retained? It may also be noted that Odenstedt has included the Undley

and the Welbeck Hill bracteates and the Svarteberg medallion (the latter included in the bracteate corpus of Hauck 1985).

“Runic inscriptions should ideally be studied *in situ*, but it goes without saying that this was impossible in a survey of this scope”, according to Odenstedt (p. 14). Yes, not all could be inspected personally, but it is uncertain whether Odenstedt has seen even a fraction. Personal inspection of the inscriptions is of the utmost importance for the would-be runologist (cf. Barnes MS p. 15). Just visiting the three Scandinavian capitals gives the scholar a chance to see more than two score of the extant inscriptions listed in Krause 1966, including some of the most important of them all.

Odenstedt has chosen to rely on secondary reproductions, the high quality of which he asseverates (see above). I maintain not only that he has tried to read more into these photographs than is possible, I also cannot agree that they are of a very high quality. Krause 1966, Odenstedt’s main source, is outdated – not primarily because it is a quarter of a century old, but because it was out of date even when it was published. The work of Krause is a late fruit of a runologist at the end of his life who had been blind for decades, which forced him to use the assistance of his wife, Agnes, in “reading” the inscriptions. Needless to say, much of the work in the 1966 edition is directly or indirectly dependent on the first edition of 1937. In his later work, Krause will change a reading without giving any reason at all, even of a lost item (e.g. the Vadstena bracteate). Furthermore, the pictorial material in the 1966 book is frequently quite old. More than three score of the photographs are directly taken over from the 1937 edition. One example: the Nordendorf Bügelfibel 1 (still extant) is reproduced from the picture published by Henning 1889! The majority of all the extant inscriptions are illustrated with reproductions more than half a century old.

This “admirable edition” (pp. 10, 13) of Krause 1966 also forms the basis for Odenstedt’s dating of the inscriptions. Since his entire work is directed towards studying individual rune forms and their subsequent development in time, a detailed and thorough discussion of the grounds for dating the inscriptions would be highly necessary and would have been expected. There is none. The dating problem gets no section or subsection of its own, but is dwelt upon lightly in a 10-line paragraph under subsection 1.4 “List of inscriptions investigated” (p. 17). Odenstedt’s attitude is there stated clearly: “The dates given are invariably those in editions and articles quoted for each particular inscription (in the majority of cases Krause-Jankuhn 1966). Opinions about dates may of course vary slightly, but this is of minor importance in a survey of this kind – the overall chronological picture should be sufficiently correct” (*ibid.*). Even if we did grant the statement that opinions about dates vary only slightly (which is certainly not correct; cf. Antonsen 1975:10 ff.), this is not the main problem. Odenstedt’s statement is based on

two presuppositions, the first being that the datings given are “sufficiently correct”, the second being that if they are, they can be used in a study of the kind Odenstedt is carrying out. I would seriously question the former, and absolutely deny the latter.

Restricting the discussion about dates to Krause 1966, it must first be made clear that this work utilizes three types of dating: archaeological, linguistic, and runological. The archaeological aspect includes the circumstances under which an object was found and a comparison with related material, be it item-connected or art-historical. Sometimes these datings are sound, even though one must remember that it is the object itself, and usually not the inscription it carries, that can be dated archaeologically. Since the rune forms on the object will be dated accordingly, it is of course important that the archaeological dating really is well-founded. The competence of Krause and his archaeological assistant, Herbert Jankuhn, is not convincing; one example should suffice. The Kylvær stone is in Krause 1966:12 ff. dated to the period 350–475. Haavaldsen (MS p. 3 ff.) has recently shown that Krause’s dating has no scientific foundation; she concludes that the grave by which the stone was found cannot be dated more precisely than to the period 0–400. If further examples are needed, the reader of this review is referred to Eriksson 1970–71, who summarizes his results: “It is argued that most of the nearly 20 graves of Roman Iron-Age date on which our present dating of the earlier group of runic inscriptions in Scandinavia is based are not very reliable sources” (*op.cit.* p. 37).

It is not unusual for Krause 1966 to indicate a substantial space of time for the archaeological dating of the object. In these cases he uses a linguistic or/and runological dating as a supplement and a specification. Inscriptions that are dated within a period of time exceeding a century include the Nydam arrows (200–500), all the Vimose finds (100–300), the Kragehul spearshaft (300–c. 550) and the Køng statuette (400–600). The Kylvær stone’s more specific dating to c. 400–450 is derived from the form of the j-rune. The Vimose fitting, the Vimose plane and the Kragehul spearshaft also have received wide datings that are then made specific by the use of certain rune forms.

Many objects are dated solely on the rune forms that occur in their inscriptions, or in related inscriptions. Runes that are commonly used for dating include the a-, e-, j-, k-, r- and s-runes, respectively. The very fact that Krause describes the form of the j-rune on the Dahmsdorf spear as “altertümlich” and the same rune on the Skåäng stone as “jünger”, whereas they are much of the same shape (Morris 1988:111), should be a warning against his use of rune forms as grounds for dating. Many of the rune stones are dated only on the grounds of rune forms, and often the pronouncements on these seem arbitrary to me. Now, if the runes were designed to be carved

in wood, and horizontal lines thus avoided, why is the form \mathcal{M} considered younger than the variant with a horizontal bar, \mathcal{N} ? The Strårup neck-ring is dated to “gegen 400” exclusively on its “alertümliche” e-rune of the latter type (Krause 1966:97; see also the Garbølle box *op.cit.* p. 73). The Nordhuglo stone, on the other hand, is dated to “am ehesten um 425” because of the \mathcal{M} -type and its j-rune (*op.cit.* p. 147). It is quite obvious that, to Krause, any inscription with the \mathcal{M} -type cannot be older than 400 A.D. (Many other examples of this kind could have been presented, had there been sufficient space.) The point is argued nowhere in detail, and seems very much *ad hoc* to me. Moltke (1985:107) maintains the exactly opposite view concerning the e-rune, but Odenstedt on his part makes no comment on this problem at all. He simply notes that, with one exception, all cases of e-runes with the horizontal bar occur in pre-450 inscriptions (p. 99) and expresses some surprise at the “coincidence” (p. 98) that the “original” form, \mathcal{M} , does not occur before c. 400 A.D., which he thinks may be by accident (p. 101). He even goes into great detail in his attempt to explain the development of this rune to (or from) its “unrunic” (p. 98 ff.) shape.

If Odenstedt had checked Krause’s datings on this point, he would have understood why all horizontal bar-type e-runes seem to occur pre-450. This is one of the very means by which Krause has dated these inscriptions! Another clear example is found in Krause’s use of the j-rune for dating purposes. When Odenstedt (p. 69) mentions three “later” examples of inscriptions from c. 400 and four from 400–450, and claims that there “*are consequently no indisputable examples of j-runes in reduced size after 450*” (p. 71), he fails to see that the form of the j-rune is Krause’s main basis for dating these very inscriptions.

Krause’s dating methods are shaky: archaeologically they will have to be studied in detail before a final judgement can be passed; runologically it is already quite obvious that they are unacceptable. His most serious fallacy is his frequent arguing in a circle.

Odenstedt should not have used Krause’s dating uncritically. Worse, by basing his study on rune forms in inscriptions that are dated by Krause on their rune forms, Odenstedt argues in as bad a circle as does Krause. The result must be that his study is impossible to use for these purposes – that is, none of the conclusions that Odenstedt has drawn about the development of the rune forms can be trusted. When he makes claims about the detailed development of, for example, the k-rune and its chronological occurrence (p. 49), it is hard to accept these statements for reasons presented above, or to make a serious effort at reviewing them.

Passing on to the dating chart on pp. 18–23, one may note that it is chiefly based on the one given in Krause 1966:313–317. This means that Krause, in a discussion of the dating of an inscription, will indicate its date within a

period. But in his chart he makes a compromise. Thus the Nydam arrows are dated to a time range between 200–500. Krause’s “compromise” is “um 400” (*op.cit.* p. 314), which is the only date given by Odenstedt (p. 19), without further comment. The problem here is that the periodization 175–400 vs 400–750 is the watershed in Odenstedt’s grouping of his material. The Nydam arrows are placed by him in the former category, but, as follows from what has been said above, are possibly to be placed in the latter. Odenstedt gives no reason why the dividing line should be drawn at the year 400. Inscriptions vaguely dated by Krause to “um 400” are arbitrarily included in the group of inscriptions earlier than 400.

As described earlier, Odenstedt has his material from photographs, the several thousand runes of which he has copied (p. 14). We are not told how, nor by what means they were reproduced in his book, but the result is hardly satisfactory. Since the runic forms are crucial for Odenstedt’s study, they have to be dealt with in some detail in this review.

It is easy to see that Odenstedt’s copies are not exact, and probably not meant to be. It is also understandable that, for example, the strangely shaped r-rune on the Britsum stick with both staff and arm made up by four parallel lines has been simplified to consist of three lines each (p. 38; cf. Odenstedt 1989:155). But it is not possible to penetrate the general principle behind Odenstedt’s copies. He claims to have classified “the runic forms [...] into main types, but I also note minor and individual variants of these types” (p. 15). That would perhaps explain why the single-line r-rune, which also occurs on the Britsum stick, is given a form with the end of the arm turned outwards, when in fact it is parallel to the staff.

A slight modification of the actual rune forms must be permitted, but in many cases I find something which appears to be something else. One example consists of three u-runes on the Kylver stone that are claimed to be “symmetric” and copied identically as such, \wedge (p. 27; cf. note 6 p. 175), whereas all three look different from each other to me and should have been assigned to a main type λ , which is not admitted by Odenstedt (but by Kai-Erik Westergaard according to Odenstedt p. 26). Possibly he is right to reckon only with the symmetrical type, besides the variants of the \lrcorner -type, in the earliest period. The two are equally common in the 22 inscriptions that are claimed to be the earliest. Later \lrcorner becomes dominating, and is discussed by Odenstedt as a possible “original” form, but is discarded in favor of the symmetrical type, because the \lrcorner -type “is incompatible with a fundamental principle of runic writing, the avoidance of curved lines” (p. 30). (This is a matter to which I will return.) Incidentally, the form \lrcorner constitutes the only significant difference between the older *futhark* as established by Odenstedt (p. 146), and the one by Antonsen (1975:1), which is not even referred to by Odenstedt in this context.

Other examples of runes that are reproduced in an erroneous way include the two *j*-runes on the Stenstad and Skåäng stones (p. 71), and in the same paragraph, we are incorrectly told that the *j*-rune on the Kylver stone “has this variant in small size”, and further that “this variant occurs once” in the Vadstena bracteate, where the top line is in fact obliterated. (Odenstedt does not hesitate to include other rune forms that are damaged, see e.g. p. 105.) Still in the same paragraph, as well as in four other instances (pp. 67, 70, 73, and 74), this rune as appearing on the Krogsta stone is given a completely misleading form. Reading from photographs, Odenstedt should have refrained from deviating from the reading made in his source, which becomes quite obvious when one reads his attempt at a modified reading of the ligature *ēk* on the Noleby stone (p. 98 with note 29 p. 177) and compares it with the treatment by Svärdström (1940–70:93).

It is difficult to say whether any study could really be carried out on the basis of the rune forms as reproduced in the book. To answer this question, one would have to check every single instance, viz. do Odenstedt’s work all over again. I will risk a guess, however, that most of the reproduced rune forms are sufficiently close to their original shape for it to be possible to discuss them. It must be added, however, that not so few statements by Odenstedt, too numerous to list here, regarding the form of a certain rune, or the (non)existence of a certain variant, have proven incorrect when checked.

Odenstedt thus sets out to study the variation in rune forms as they occur in the inscriptions. He does not discuss why that variation should be relevant, whether indeed any kind of variation is relevant. Does, for example, the minute variation in the shape of the *r*-rune recorded by Odenstedt (p. 35 ff.) have any significance? Not surprisingly, the conclusion after the six-page treatment of this rune is that all four main variants occur early (p. 39). Odenstedt thinks that this indicates “that the original runic alphabet must have undergone considerable development and modification before we meet it in the oldest inscriptions c. 200” (p. 39 f.). He also appoints the form *R* as the “original” because it dominates. Here, as very frequently otherwise, one is impressed by the very variation in would-be contemporary rune forms, always accounted for by Odenstedt as evidence of the development within the *futhark* itself.

Odenstedt has no theoretical framework within which to undertake the analysis of the rune forms. He speaks of “graphs” and “allographs” but only once, in a quote from a work by Ottar Grønvik (p. 113), of (German) “Grapheme”. All allographs belong to graphemes, and it may be that a study of the latter is the better way of discovering the origin of the runes. Odenstedt does not discuss the matter, nor does he argue the case why variation on allograph level should on the whole be significant. No distinc-

tive allographic features are named, nor is there any attempt at systemizing his observations theoretically. Antonsen's graphemic system (1975:8) seems a superior way of approaching the problem, even though it has been criticized (see Knirk 1977:178 f.; Santesson 1989:13 ff.; for graphemic analysis see also Loman 1965 and Fjellhammer Seim 1982). Antonsen's analysis of, for example, the R-rune as 1 staff/2 crooks/continuous puts the matter neatly and accounts well for the variation within the type.

On many occasions Odenstedt notes the variation between rounded vs. unrounded rune forms (e.g. pp. 30, 33 f., 54, 74, 85, 88, 96, 109, 125). He has also observed that this often occurs in the earliest inscriptions. As a matter of fact, rounded variants occur for all runes that can differentiate between rounded and angular at all. A typical way for Odenstedt to deal with this fact is exemplified in the following quote (p. 33 f.):

Because runes were originally no doubt inscribed in wooden objects it is commonly assumed that their angular forms are older than the rounded ones. Assuming this theory is correct, þ [...] must be the original form of the þ-rune. However, the early appearance (c. 200) and the predominance of þ in Scandinavian inscriptions show that this variant (which was, originally at least, doubtless used only on stone and metal objects) is also of an early date. Indeed, the fact that the development þ > þ was completed by 200 indicates that a fairly long time must have elapsed from the invention of the runic script to the period of the oldest extant inscriptions (c. 200).

He later includes the matter of a time lapse between the invention of the runes and evidence of their use (e.g. pp. 30, 39 f., 62, 69, 100) in his theory of the origin of the runic script (p. 164 ff.).

It is obvious that Odenstedt is again arguing in a circle, as was recently pointed out by Barnes (MS p. 7). In this case Odenstedt will have the comfort of being joined in this circular reasoning by most runologists (e.g. Moltke 1985:40, cf. Barnes MS p. 12). Barnes (*op.cit.* p. 7) describes "the fallacy of affirming the subsequent" very elegantly: "(a) Wood requires angularity in the characters to be carved in it; (b) we find only angular characters in the earliest inscriptions; (c) therefore the rule that characters intended for carving in wood must be angular has been implemented". Actually, presupposition (b) is not true. Curved lines occur, as Odenstedt notes (p. 159), in the earliest inscriptions. Furthermore, not even presupposition (a) is true: wood obviously does not require angular characters, since rounded examples occur on wooden objects, such as the Nydam arrows, the Britsum stick, the Kragehul spearshaft, and the Vimose plane, i.e. almost all the inscriptions on wood that actually could contain a rounded type rune. The Garbølle box contains no rounded forms, but it does contain the "unrunic" and "anomalous" e-type with a horizontal bar, which should have been impossible on wood (cf. Odenstedt pp. 98, 101).

Even though Odenstedt is clearly conscious of all this (p. 159), he adheres to the conception that the runes originally *must* have been designed for use in wood. Remember Odenstedt's treatment of ᚗ, above, and the "fundamental principle of runic writing, the avoidance of curved lines". Personally, I believe that the runes were no more (and no less) angular than any other fairly primitive script.

The final chapter of the book, "On the origin of the runic script", is a revised version of an article by Odenstedt in *Saga och Sed* 1984. In a way, it is a pity that it concludes a book which has inspired so little confidence. If read on its own, it has a quality of provoking one's imagination, even if one shares few of Odenstedt's opinions. His review of previous research is not impressive, his graphic charts sketchy and superficial compared to those in Morris 1988. Odenstedt sets out to study the origin of the *futhark* by principles for alphabet creation (p. 153 ff.). These principles seem no better than those established by Moltke (1976:32 ff., 1985:38 ff.) and definitely inferior to those applied by Morris (1988 *passim*). Odenstedt in reality limits himself to discussing the forms of the letters and their phonological value. Morris (1988:12) makes a systematic (if not always convincing) analysis of seven more factors: "the direction of writing, the use of ligatures, interpunction, the vocabulary of writing, writing techniques, types of inscriptions, and spelling conventions". The two scholars get radically different results. Odenstedt appoints Latin as the only source for the runes, which were presumably created around the year 0 (p. 168). Morris (1988:150) does not think that the runes had a "mother alphabet", since they were created so early that both the Greek and the Latin alphabets could have served as a source, but that they had "much more in common with the Greek" (*op.cit.* p. 153). Their creation could have taken place in the 5th cent B.C. (*op.cit.* p. 152).

Odenstedt does not convince the present reviewer that Latin is the sole origin of the runes. His one original contribution consists of the derivation of the ᚧ-rune from Latin ⟨O⟩ (which is an amendment of the Latin-origin theory as presented by Fritz Askeberg), whereas ⟨Q⟩ should give ᚨ. He realizes that the very opposite seems more likely, and therefore introduces the *ad hoc* rule that "small runes should be reserved for consonants" (p. 167).

Odenstedt must be given credit for bringing to life some valuable propositions previously advanced by others. One is the suggestion that the invention of the runes and their arrangement in *futhark*-order need not be simultaneous (p. 170). That would explain why the runes could have been inspired by a Mediterranean alphabet, without appearing in alphabetical order. The second is the observation by Anders Bæksted that the rather primitive nature of the hitherto known inscriptions with the older runes makes it improbable that the mass of unknown inscriptions from the same period would be of a

totally different, more advanced type (p. 171). This would rule out the appearance of inscriptions with commercial, administrative or other very practical contents.

Odenstedt concludes his book: “The art of writing was a luxury which Germanic people had seen Romans practice and which they no doubt envied and tried to imitate, with very limited success” (p. 173). Still, he has claimed that the inventor of the runes used every single Latin letter (p. 164), and actually “must of course have known Latin well” (p. 169). Why then, is the agreement between the Roman alphabet and the Germanic *futhark* so bad? Either the runic innovator did not know Latin so well, or else Latin cannot be the source of the runes.

Finally, I want to deal with some more formal points. Odenstedt writes in a clear and unaffected style which makes pleasant reading. The proof-reading is satisfactory – a dozen unimportant printing errors, but there are also some others that might be confusing: (p. 35, l. 6; pp. 41, 44, 103) 1933 > 1939; (p. 36, l. 4 f.b.; p. 94, l. 3) 81 > 79; (p. 79, l. 8 f.b.) **u**PARASBA > **u**PARABASBA; (p. 171, l. 15 and 13 f.b.) **rohoaltr** > **rhoaltr**, **fin**a > **fin**o; (p. 176, l. 4) 151 > 158; (p. 177, l. 3) 132 > 130; and (p. 180, l. 3 f.b.) 198 > 1986. Three inscriptions, designated in accordance with Krause, should rather be called something else, i.e. *Eggja* (p. 20), *Lindholmen* (ibid.), and *Mos* (p. 18). The reference to section 5.5.2 (p. 42, l. 20) is misleading. The *Skandumodu* solidus is often designated by bold face, sometimes by italics. The numbering system of the inscriptions is not always used consistently (e.g. p. 49). The ligature **ƒ** + **þ** is drawn incorrectly (pp. 84, 85, 111). A ligature or an uncertain reading will sometimes be unmarked (e.g. p. 173).

Odenstedt uses the terms *transliteration* and *transcription* in a deviating manner, since he lets the (assumed) phonetic value affect the transliteration, which should be kept as an abstract entity for each system of runic script. Thus Odenstedt transliterates **ƒ**, **ǰ** on the Chessel down scabbard mount as **a**, **o** instead of **æ**, **œ** (p. 137). The same type of problem occurs in the Watchfield fitting (p. 81), where also **β** is even discussed as if it were a **p**-rune.

The book was published at the very end of 1990. The preface is not dated, but it is clear that Odenstedt's work must have been finished for some years. The bibliography is slim – slightly more than two pages – and nothing after 1985 is mentioned. It should have been made clear in the preface that the book takes nothing into account which was printed after that date. This makes parts of the material (e.g. pp. 97, 112) and some of the results outdated. Moltke is even quoted from his 1976 edition instead of the book from 1985. It is a pity that Odenstedt did not let himself be challenged by Morris's ideas, whose dissertation appeared in 1983.

The shortness of the bibliography also depends on Odenstedt's somewhat

sparing use of references. Works that definitely should have been mentioned include Nielsen 1969 on dating, Conally 1979 on the \mathfrak{J} -rune and Thompson 1981 on transliteration. Furthermore, I find Odenstedt's disparaging attitude towards Antonsen disturbing. Antonsen's work may certainly be criticized, but Odenstedt begrudges him everything. He will frequently not acknowledge contributions made by Antonsen, and when he does, he will even deviate from bibliographical practices and refer to a later source instead of the original, when the latter is Antonsen (cf. pp. 83, 106). He quotes Antonsen incorrectly or misleadingly (cf. pp. 103, 146, 149 f., note 20 p. 176, note 34 p. 177). One also becomes suspicious of Odenstedt's learning when he criticizes Antonsen's opinion that ϕ is an innovation and not borrowed from Greek *theta*, because the Greek letter denoted /b/, whereas standard works will say that in standard pronunciation *theta* denoted /t^h/ even on this side of the year 1, but /b/ appears in (non-dialectal) popular speech not before the 1st cent. B.C. (cf. Sturtevant 1940 § 92 and Rix 1976 § 95 d). Finally, if Odenstedt had studied Antonsen with an open mind, he would have known that the reason why *spā* in the inscription on the Björketorp stone is written *sba*, is not because the **p**-rune "is avoided" (p. 79) and the "homorganic" **b**-rune "substituted" (p. 82; cf. p. 94), but because the opposition between voiced and voiceless consonants is neutralized after /s/, just as on the Myklebostad stone: **gasdir** /gastir/ (Antonsen 1980, note 18 p. 13; cf. 1975 § 4.6 and Barnes 1987:33).

The book contains no indices which makes a work of this kind more difficult to use.

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