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Runology has traditionally attracted few scholars, most of whom have been interested in the runic inscriptions in Scandinavia. Continental and English runology has played a minor role. This, of course, primarily depends on the small number of inscriptions to be found south of the Nordic countries, but also on the lack of official status that runology has won for itself outside Scandinavia, especially in Britain. Even if the non-Scandinavian inscriptions are relatively few, they are often both as important and as interesting as their northern counterparts. Unfortunately, there is no corpus edition of the runic inscriptions on the British Isles. It is thus a pleasure to record the appearance of a major work dedicated to the runic inscriptions of England and the Continent, emphasizing English inscriptions. *Old English Runes and their Continental Background* totalling almost 650 pages answers a long-felt need for a fuller treatment of these inscriptions.

The attractive volume is the result of a symposium which took place in Eichstätt, Germany, in 1989. The organizer of the conference and editor of the book, Alfred Bammesberger, deserves commendation. However, the good impression is slightly affected by the inconsistent and often unwieldy reference system (sometimes pages are alloted an average of ten spacious footnotes), the irregular use of italics and bold face, and the unsatisfactory reproduction of some of the runic items. The volume

contains a General Index and an Index Verborum, both compiled by Alfred Wollman. They lack some entries but nevertheless are very helpful.

The batting order is most fittingly headed by Ray Page who in a lucid and substantial article "Anglo-Saxon Runic Studies: The Way Ahead?" recounts his three decades in the field. He is undoubtedly the leading scholar within Old English runology today, and it is from him we have long expected a corpus edition. Here we are told the reason why it has not appeared, even though it was completed in 1969. For two reasons Page himself is "very relieved" that it did not. Firstly the material itself is now much larger which must lead to a reevaluation. Secondly Page's own opinions have changed due to the research done in the meantime. "Thus I am relieved that I am not formally identified with what I thought in 1969", he writes. It is lamentable that Page did not publish his corpus. Such work is best done by sceptical scholars, of which category Page is a member. It is therefore to be hoped that the sometimes too cautious Ray Page will do away with his reluctance.

Page goes on to expound on two observations advanced by René Derolez. The first concerns the representativity of the extant inscriptions. Derolez has convincingly shown that they constitute less than 1 % of the total number of inscriptions (whether English or other early inscriptions from Scandinavia or the Continent). Derolez also contends that stability in runic practice proves regular and continous use of the runes in England. Page is not so sure of this because of the irregularities occurring in the inscriptions. I believe that Anders Bæksted has a better interpretation of the nature of the extant material, which certainly is very small but nevertheless quite homogenous. Bæksted thinks that the complete corpus of all produced inscriptions would show varieties of those now known but probably not totally different *types*. We are not ever likely to find commercial, political, literary or other longer and more "technical" texts. This argument nullifies much speculation about the nature of yet unfound material.[1] The irregularity of the English runic inscriptions is not, however, an argument against the continuity of runic practices, since this is also typical of the much larger body of Scandinavian inscriptions from the Viking Age. Page reaches the conclusion that we must "try to make sense of what we have, or simply give up the study altogether", which is a constructive starting-point for any type of historical research.

The second of Derolez' observations deals with the relationship between "manuscript runes" and epigraphical runes. Page has previously regarded them as more or less independent entities, an opinion he has now reconsidered, partly because of new finds, such as the *futhorc* pin-brooch from East Anglia and the Blythburgh writing table which form links between epigraphic and learned traditions. But Page reminds us of another important dimension: the influence of the Roman alphabet, used in parallel and intermingled with English runes. "There is interplay between these various scripts", Page contends. Roman orthographic practices are never far away, except possibly for the earliest English inscriptions. Page continues by pointing out that "diagnostic" rune forms are not a good indicator of provenance. Page also wants to investigate the emergence of the (new) **o**-, **æ**- and **a**-runes in the light of the *os*-rune on the Undley bracteate, possibly made in the Schleswig-Holstein area, not

[1] Some runologists presuppose the existence of vast numbers of inscriptions (not yet found) on perishable matter that would be of a totally different sort, due to the fact that for example letters of trade or love are likely to be incised in such materials instead of metal or stone which is suitable for more monumental or commemorative occasions. Parallels have been drawn to the hundreds of inscriptions on wood found in Bergen, Norway, even though they stem from another period and a different situation.

within what is commonly accepted as Anglo-Frisian boundaries. Page rejects the idea that a south Danish rune carver executed the inscription.

Other subjects touched upon in the article are the question whether runic inscriptions appear in Wessex or not, the importance of runic coins, runic inscriptions in East Anglia, and the value of studying find-reports of extant inscriptions and the early accounts of lost or damaged ones. Finally Page adds a warning against the "concentration on purely linguistic aspects" of the runic material and elaborates somewhat on the failings of that approach since it "will draw attention from features [...] that may have a more significant effect on an inscription's form." Page admits that his attitude is primarily rooted in his "ignorance of linguistic theory". As I am not so convinced of his linguistic innocence, I believe that Page would better warn against the single-minded linguist, just as any other scholar working with runes from a narrow-minded perspective. Properly applied, linguistics is at least as necessary to runology as is archaeology or archival studies. Sometimes it is better to speak simply, for example, of "graphemes" than runic letters, if nothing else for the sake of clarity. On the whole, Ray Page asks questions rather than trying to answer them. On the other hand they are good questions, and he has a sure instinct for what needs to be done within English runology. His article is a good starting point for a book on Old English runes.

The archaeologist Catherine Hills received a special invitation to the symposium to supply "The Archaeological Context of Runic Finds", which is also the title of her article. The first part is a somewhat elementary but useful account of archaeological methods, their value and failures. The runologist will probably consider the chief task of the archaeologist to supply her or him with precise datings of the runic objects as well as the exact provenance. Hills convincingly shows why this cannot often be the case. Nevertheless, the context of the inscriptions is very important: "if a runic inscription is to tell us more about the society which created it, we need to know as much as possible about its context," Hills writes, and she continues: "The primary context of an inscription is the object on which it has been written." Other aspects of the context include circumstances of discovery and deposition. The cultural context reminds us of the problem of provenance: is the object native or imported, and is the inscription original or added later? As a starting point for a discussion of different kinds of contextual problems Hills then takes John Hines' catalogue of early Anglo-Saxon inscriptions. She discusses the Loveden and Spong Hill urns, the Caistor-by-Norwich astragalus, and finally the Undley bracteate. Concerning the three former Hills puts forward a few more arguments in favor of their Anglian character, which strengthens Hines' theory that the Angles brought the runes to England. The Undley bracteate which Hines believes originated in Schleswig-Holstein is given a radically different provenance by Hills. She thinks it was made in England, quite possibly by (or by order from) someone of Scandinavian extraction or with such contacts. Hills' arguments on this point are well worth consideration in future discussions of the Undley bracteate. I find her hypothesis convincing and regard her observation as one of the most important in the book.

John Hines' article "Some observations on the Runic Inscriptions of Early Anglo-Saxon England" is exactly what it claims to be. Hines reiterates some of his earlier findings, but more importantly he argues that there is a marked difference between the early runic material and the later, already tentatively suggested by Ray Page. Obviously, something happened in the first half of the seventh century. Most of the earliest inscriptions are found in the Anglian parts of England. This is one of the reasons why Hines believes that the Angles brought the runes to England. The putative origin of the Undley bracteate is another reason, unfortunately not reevaluated

by Hines after Hills' contribution. Hines looks at the earliest inscriptions from three angles: date and provenance, function, and language. He contests that runes occur on "exclusive items in especially rich contexts", and further that they are associated with adults of both sexes from a high social stratum. Since the runes in early Anglo-Saxon society did not serve bureaucratic purposes nor were normally intended for display, Hines concludes that this esoteric writing was precious, as was the mere possession of an inscribed object. The watershed is the "romanized continental influence". Runes are beginning to get a real use; they appear in new contexts: on monumental stones and in coinage, hence the dominance of Roman script. As an appendix Hines lists eighteen runic inscriptions dated before 650. In this context he adds a five-page treatment of some problems connected with transliterating runes. Hines identifies three major obstacles in transliterating the early Anglo-Saxon runic inscriptions: obscure runes (damaged or difficult to identify as runes at all), aberrant runes (deviant sound value), and runes with uncertain sound values. Hines makes some very interesting comments on examples within the third category, especially when he discusses the runes *æsc*, *āc*, and *ōs* and their earliest sound value. It is a pity that Hines' valuable comments are marred by an evident unfamiliarity with other runic material than the Anglo-Frisian. He does not seem to know the practices for transliterating and editing Scandinavian inscriptions. The main problem with Hines is his failure to distinguish between *transliteration* and *transcription*. To transliterate is to transfer rune by letter. The sound value of a rune has in principle nothing to do with this, but for practical reasons a common value is chosen. Anglo-Saxon *æsc*, *ōs*, and *œðil/ēðil* should therefore always be transliterated **æ**, **o** and **oe** respectively, regardless of their posited sound value in an individual inscription. The transcription is intended to show the linguistic interpretation of the runologist. Hence Watchfield's **wusæ** may very well be transcribed *wusa*, if there is enough evidence for that,[2] but the proper transliteration should at least tell any runologist which graphs are used. I think that the confusion is due to the formal proximity of most Anglo-Saxon runic inscriptions to their supposed pronunciation. A Scandinavian runologist perhaps has less of a problem here, since he or she is used to the fact that the transliteration **lukobri** is to be transcribed *Laughambri* (a place name). It should be kept in mind that a similar situation is true of the inscriptions with the oldest runes as well. The **a**-rune is used both for /a/ and (final) /æ/ in **swarta**, but transliterated consistently, and the rune **algiz* is transliterated as **R** although it probably denoted /z/ in the earliest inscriptions. The transliteration of *æsc* as **æ** for [a] in early Anglo-Saxon inscriptions is a parallel to this latter practice.

René Derolez returns to old haunts in his article "*Runica Manuscripta* Revisited" which is a sort of update of his 1954 dissertation on the subject. In it he makes some changes and additions, of which the most important is the inclusion of the Antwerp version of the *isruna* tract (a treatise on cryptographic runes, a favorite of the scribes). Derolez also brings up his own older hypothesis that "scratched" runes (written with a dry-point pen) are intermediate between epigraphical runes and their manuscript counterpart. He now seems to think they are not, but that the graffiti from the Commodilla graveyard in Rome possibly could act as kind of a bridge be-

[2] It may be noted that René Derolez (see below) mentions that in the runic alphabet in the Kassel MS. the w-rune is used for *p* (as well as in the name **losewi** *losepi*), without doubt due to influence from the Latin letter <P>. The crux here is whether Roman influence really could account for the same process in the mid 6th century Watchfield inscription.

tween the two. Here, I would like to remind the reader of Gösta Franzén's article in *Saga och Sed* 1986 on the bronze vessel with an English runic alphabet, also found in Rome, which may point in the same direction. Manuscript runes are sometimes so early and epigraphic inscriptions so late that there has to be a connection between the two, the former perhaps not being so antiquarian as Ray Page once thought (cf. above). The Belgian expert on these matters concludes his article: "the *runica manuscripta* have now become an acceptable ingredient of Anglo-saxon runic studies". One cannot but fully agree with him on this point. Derolez himself points out the bearing the manuscript runology has on our knowledge of the Old English language, including its dialects. I believe that manuscript runes will contribute more to the understanding of the epigraphical runes in the future (and *vice versa*) than they have been allowed to do in the past. In this light it is to be lamented that Derolez chose not to proceed with his edition of the Norse *runica manuscripta*. In a postscript we are informed that lack of interest for this topic in Scandinavia and the heterogenity of the material played a role in his decision. He hopes that some other enthusiast will meet this challenge. It is likewise to be hoped that René Derolez will consider bringing out a new, updated version of his *Runica Manuscripta*, in spite of the obstacles. No one else is capable of doing it.

The famous Ruthwell Cross at Dumfries in southern Scotland most fittingly carries portions of a splendid Old English poem: *The dream of the Rood* (=cross). Christopher Ball contributes to the understanding of its fragmentary text and the use of its runes in his article "Inconsistencies in the Main Runic Inscriptions on the Ruthwell Cross". He starts out by proposing a new, slightly different edition of the poem and further suggests that it is stanzaic, a verse form otherwise unknown in Old English poetry. The rest of the paper deals with four types of putative inconsistency in the use of the runes. Geminate runes occur irregularly and Ball can no more than other scholars explain why, but he does point out parallels to other runic inscriptions and Northumbrian manuscripts. There are a number of "extra" runes added to the originally 28 of the Old English *fuporc*. These, designed to represent palatal/ velar consonants, are believed to be derived from the manuscript tradition. Christopher Ball sees the **k**- and the **k̄**-runes as alternative rather than contrastive forms (not convincingly since they are used with a complimentary distribution), thus subtracting one rune from the "developed" *fuporc*. The argument that **[b]ug[a]** is an unlikely conjecture since **g** would be expected inspires more confidence. Ball's own alternative conjecture (**[n]ug[i]**... *nu giwendæ* "now turned") benefits from this sound reasoning. The inconsistent representation of final, unstressed /n/ is given the attractive solution that the preceding vowel had become nasalized, and the writing of the **n** a matter of choice. Ball's comparison to Latin scribal practices is not, however, necessary. The same problem existed in other runic traditions (see my article in *Proceedings from the third symposium on runes and runic inscriptions*, forthcoming). The representation of final, unstressed vowels is another matter, though. Here Ball makes more of a case for the manuscript tradition to explain why "traditional" **-i** and **-æ** spellings occur instead of expected **-e**. There are probably other alternatives (schva), but in any case the tie with other early Old English texts is established. Christopher Ball concludes his study by the well-argued claim that "there are no serious inconsistencies in the main runic inscriptions on the Ruthwell Cross."

In "Three Old English Runic Inscriptions" Alfred Bammesberger takes a look at the texts from the Loveden Hill urn, the Overchurch stone, and the Derbyshire bone piece. For all three he adds some observations on points of the inscription that deserve consideration, although a more thorough study would have been desired. The

Loveden Hill inscription **s3pæbæd picp hlæf** has so far been most convincingly interpreted by Bengt Odenstedt, although by no means unproblematically, as *Sipæbad picp hlâf* 'Sipæbad gets bread'. Bammesberger elaborates on an idea by Fred Robinson, seeing the < (**c**) in the second word as a obliquely cut ∧ (**u**), and the ƿ (**p**) as a badly executed ƿ (**w**), thereby getting **piuw** *piuw = peow(u)* 'female servant'. Neither his runological procedure nor the result of the interpretation invites credence. As for the Overchurch inscription, Bammesberger more convincingly proposes that the initial *folcæ arærdon bec[un]* 'the people erected the monument' (as interpreted by Elliott) should be interpreted as 'a monument was erected for the people', leaving unexplained what is really meant by "the people". Finally, the Derbyshire inscription is translated 'God, help this Hadde (fem.), who wrote this!', thereby interpreting **godgecaparæhadda** as *God geca paræ hadda*, which favorably contrasts with 'God saves by his mercy Hadda (masc.) ...' (proposed by Janet Bately). Bammesberger's own apprehension about the otherwise unattested feminine *Hadde* seems too cautious, granting the hypochoristic nature of the name, as he himself demonstrates.

Bammesberger's brief article is followed by a 50-page study by Mark Blackburn entitled "A Survey of Anglo-Saxon and Frisian Coins with Runic Inscriptions", a most valuable contribution. Blackburn discusses the four major groups of early English and Frisian coinage: solidi (*c.* 575-675), small pennies ("sceattas", *c.* 675-750), broad pennies (*c.* 760-869), and stycas (*c.* 820-67). His approach is primarily numismatic, but he also makes comments on the linguistic aspects of the inscriptions, sometimes based on information provided by Ray Page. The cooperation is successful, e.g. when Blackburn questions Page's earlier assumption that the second-to-last rune of the **bEOnna rE᛬s** broad penny is an erroneous ᚺ-rune. Blackburn convincingly shows the rune to be a proper **ᛉ**-rune, where **ᛉs** represents /ks/ in *rex*. Page is then, according to a footnote, able to make the correct analysis of the **ᛉ**-rune as having the sound value [x]. Mark Blackburn's up-to-date survey is extremely useful. Hundreds of coins carry runic or mixed runic and Roman inscriptions. This large number only represents a much smaller portion of dies actually used for a total of some 70 individual inscriptions, and of these many constitute variants of the same contents (for example six different renderings of the moneyer's name *Æpa*) limiting the number of "words" to approximately half of that number, including several names with a single (sometimes uncertain) rune mixed with Roman letters, such as BEAgHEARD, as well as uninterpreted sequences. Even so, the numismatic runes are important since they make up a considerable part of the corpus and, as Blackburn points out, are well dated and localized, and furthermore record scarce runes, rune forms, and bind runes. I want to emphasize his discussion of the otherwise so rare ᛇ-rune which occurs a number of times in the coin inscriptions, there denoting /ʒ/, and his observation that erroneous inscriptions surprisingly often contain correct runic forms (e.g. **spi** for **epa**) and not just garbled symbols. Unfortunately, Blackburn, too, suffers from the confusion about what constitutes a transliteration (see above). This also affects the designation of the items themselves. No definitive practice has evolved. The *Skanomodu* solidus, for example, is referred to by Blackburn as "'**skanomodu**' solidus" with a footnote explaining why this "transliteration" was chosen. Runic inscriptions on coins are not best referred to by finding-place, since many exemplars make such a designation unclear. The inscriptions and the coins are therefore better referred to in terms of the contents. If the type of the coin is discussed I suggest that the italicized form be used, for example "the *Æpa* coins", "the *Welandu* solidus". In case the individual exemplar, group of exemplars, or specific inscription is mentioned I would

recommend the use of "the **epa (apæ, ep, spi)** coin" and "the **weladu** solidus", respectively. Numismatically Blackburn's main contribution is a more pronounced emphasis on the Kentish origin of some items.

In "Recent Finds from London" Kevin Gosling reports the find from the London City waterfront of a runic ring carrying the inscription **fupniine**, preceded by a "mirror" or bind rune ᛇᛠ or ᛇᛏ. Unfortunately the ring is as hard to date as the inscription is difficult to interpret. Gosling also reports on a newly found bone trial piece which carries Roman letters whose forms seem to be influenced by runes. Kevin Gosling proposes an interpretation: a woman's name *Ælfburh* which looks good, although it is not recorded elsewhere.

The magic of the runes is perhaps what is best known to people in general. The question is whether this magic quality belongs to the runes themselves when in use or only attributed to them by later tradition. Runic inscriptions provide scant evidence for the proponents of magical runes. Christine Fell makes a full investigation of the Old English evidence in her article "Runes and semantics". Her treatment is (as always) elegant, persuasive, and comprehensive. None of the five meanings of the word 'rune' provided by the Bosworth & Toller dictionaries: 'whisper', 'mystery', 'secret', 'written with a mysterious or magical idea', or 'rune, letter', can be said to cover the semantic range of the word as used in Old English texts. Fell demonstrates that the Anglo-Saxons themselves primarily used the word for 'thought, idea, knowledge'. As for the meaning 'mystery' this is only to be understood as a translation of Latin *mysterium* in the strictly Christian sense of the word, cf. Gothic *runa*. There are a few examples of the word *rune* or related lexical items that imply a magical connection or mean 'whisper', most explicitely in the writings of Ælfric. Chris Fell explains the former meaning by influence of Old Norse *rún* and the latter of Old High German cognates with the sense 'whisper'. The Middle English developments corrolate this interpretation. One could only wish for a similar treatment of 'rune' in other Germanic languages since the Old English evidence is now reevaluated. The basic question does remain: did the word originally imply a magical or esoterically secret connection?

R. W. V. Elliott's "Coming Back to Cynewulf" is another revisiting of manuscript runes. From an almost purely literary approach Elliott discusses four poems by the poet Cynewulf who incorporated his name in runes into his work. Cynewulf's riddling signatures are part of an old acrostic tradition, and were meant to achieve intercessionary prayer (not uncommon in mediaeval runic inscriptions). These passages were intended to be deciphered and thence they had to be intelligible. Ralph Elliott plausibly argues that all the runes of the signatures make sense, in spite of claims to the opposite. Moreover, every rune must be "decoded" by its name which still must have been known at this time (a reasonable attitude). He strengthens the interpretation of most of the runic passages and reiterates his opinion that the **u**-rune must stand for 'aurochs', even though Ray Page has bluntly dismissed this. Elliott now thinks that 'aurochs' not only figuratively and emblematically may refer to the physical strength of the animal but also to the "manly prowess" of the men who hunted it.

The remarkable box from Auzon, with its fascinating pictures and enigmatic runic inscriptions, has been the subject of many learned studies and is of course subject to scrutiny in the book reviewed. Three articles appear, but for two reasons cannot be properly assessed here. First of all I am not sufficiently informed to be a judge of these complicated matters, especially not where iconography and Old English philology is concerned. The second reason is that none of the contributions is in a form suitable for reviewing. In "The Lid as Conclusion of the Syncretic Theme of the Franks Casket" Marijane Osborn sees "exile" as being the over-all theme of the

casket. Its text is supposedly summarized in the legend **ægili** on the lid, whether this means *Ægil* 's 'belonging or referring to a person Ægil' (with different implications), *Achilles* (the Greek hero), or *egle* 'trouble, terror'. Osborn accounts for all alternatives in her interesting paper. She grapples with an enormous topic but in my opinion is not successful in her interpretation. In spite of her wide reading and a highly imaginative line of reasoning the impression of her work is impaired by a lack of structure and clarity. The ideas should have been sorted out and each argued separately. Marijane Osborn's subject is more a topic for a monograph than a paper. As it is, a proper summary would have made her points more readily understandable. - A full treatment of the runes on the Auzun casket is the topic of Heiner Eichner, who presented a lecture at the conference, but who did not have the time to finish his article for the book. His presentation "Zu Franks Casket/RuneAuzon (Vortragskurzfassung)" is published as an appendix to the volume. Since Eichner's "Gesamtinterpretation" is presented in such a preliminary manner, it is exeedingly difficult to present his results or judge their validity. Suffice it to say that the text he establishes is not intended to be "epigraphisch akribischer" but rather a useable basis for the interpretation of the language and subject-matter. Heiner Eichner posits that the casket was made at the beginning of the eighth century and that the author of the inscriptions was well versed in the conventions of religious poetry (manifested also by Cædmon) as well as the epic tradition of Beowulf. - Alfred Bammesberger's "Franks Casket: Editor's Notes" follows Eichner's draft. Bammesberger briefly provides some useful linguistic comments on linguistic details in the Auzon text with Eichners paper as a starting point.

The second part of the book is labeled "Runes on the Continent and in Scandinavia". It contains a number of interesting articles, but gives a slight impression of having been grafted unto the first part. With a few exceptions this is not really a sketch of the Continental background of the English runes. For reasons of space, I have therefore tried to make my review of this part more condense.

Klaus Düwel presents and up-dates the Continental inscriptions in "Kontinentale Runeninschriften". We now know of some 70-75 texts from this area; the uncertainty of the number is due partly to possible falsifications, partly to inscriptions whose "runicity" is hard to decide. The concept Continental, too, is somewhat complicated. Düwel takes it to mean inscriptions found on the Continent, indeed a both pragmatic and practic definition, even though some of the runic items obviously are imported. Düwel's mainly focuses on the Alamannic inscriptions. He looks at their bearing on the question of Pagan-Christian syncretism and concludes that no specifically Pagan inscriptions are known and that Christian grave goods as well as runic inscriptions are disproportionally common on Alamannic soil. Klaus Düwel also discusses the relationship between the runic inscriptions and those with Roman letters. The runes belong to the perod *c.* 550-700 and are primarily of a private character. Roman letters start appearing in earnest in the seventh century and have more of an official character. The Latin alphabet, furthermore, seems to be connected with the highest stratum of society, whereas runes belong to the "upper middle class" and predominantly to a female sphere. The end of the runic tradition on the Continent was not due to Christianization but to new burial customs, which more gradually were the result of the missionary efforts.

The Frisian runic inscriptions are the most important for comparison with the English. Arend Quak studies the forms of the Frisian runes in "Altfriesische und altenglische Runen". The 16 (or 18) Frisian texts record most of the runes but some forms are very problematical, either because their identification with a certain grapheme is uncertain (is for example Westeremden ᛗ a unique Frisian example of the p-rune?) or because their shape is difficult to accomodate to any grapheme. Quak

briefly discusses a number of inscriptions and rune forms and draws some interesting parallels, but leaves many questions unanswered. This problem no doubt deserves a full treatment together with continued attempts to establish credible interpretations of the Frisian material. It may also be noted that Arend Quak accepts the view that the runes were introduced on Frisian soil from England before the sixth century, a perfectly possible alternative, but one which I do not consider to have been ~~be~~ conclusively proven.

Problematic endings are the subject matter of two very short articles: "Unaccented Vowels in Runic Frisian and Ingveonic by Hans Frede Nielsen and "On the Morphology of Old Frisian *deda*" by Alfred Bammesberger. Nielsen compares *deda* to other Frisian words with endings reflecting Germanic **-ōm* and **-ǣp* but fails to explain how the endings represented by **-a** and **-æ** are distributed in the runic material. As for words such as **kabu** Old Frisian *kamb* < **kambaz* he chooses to uphold Wolfgang Krause's explanation that the **-u** represents a "Murmelvokal" (schva), i.e. a last reflex of unaccented *-a*. In spite of Nielsen's contention, it is probable that the original masculine nominative singular ending **-az* had completely disappeared by this time in view of the phonetic development in Runic Frisian. In "The Personal Name *Aniwulf /*Æniwulf", appendixed to Mark Blackburn's numismatic article reviewed above, John Insley (following Heinrich Beck) attractively explains Runic Frisian personal names ending in *-u*, for example *Welandu*, as having a case marker for the instrumental. Insley does not discuss the form **kabu**, but it is possible that one should look along the same lines for an explanation. In *deda* Bammesberger wants to see a (secondary) reduplicated perfect formation, Germanic preterite **dedō̄*. It makes good sense, provided one believes that the attested Old Frisian (regular) preterite *dede* (with its Germanic cognates) originates from an imperfect of a reduplicated present formation.

"The Scandinavian Runic Inscriptions of the Older Fupark and Old English Personal Names" is the somewhat misleading title of John Insley's comparison between personal names in the oldest runic inscriptions with West and East Germanic onomastic material. Insley sees *WarawiniR*, *FrawaradaR*, *KunimunduR*, *LandawarijaR* and a dozen other names as examples of influence from Continental Germanic dialects and Ingvæonic, following a theory proposed by Otto von Friesen. Insley's article makes interesting reading. His method is critically reviewed by Lena Peterson in *Namn och Bygd* 81, to which I refer. A comparison between name elements recorded in West Germanic sources 500 years older than their Scandinavian counterparts is somewhat lopsided.

The Næsbjerg clasp carries an inscription which is most difficult to read. Carl Marstrander suggested **sinwaraw** 'ever wary' plus **w** as "Begriffsrune", Wolfgang Krause **waraflusa** 'one wary of chattering', Erik Moltke **warafnisa** 'one wary of tittering', and Elmer Antonsen **warawnis** (Gothic) *Warawins*. John Insley now follows Antonsen's (and Marstrander's) reading but asserts that the element **war-ōlja-* can be ruled out. Insley proposes the emendation **waraw[i]n[a]s,** genitive of *WārawiniR* < *wǣr-a-win-i-*, *per se* an attractive solution onomastically but one that operates with two errors, hardly a permissable method. It is also unclear to me whether Insley has inspected the clasp personally. I have done so, and would hesitate to propose an interpretation. There is an unidentified rune at the end of the inscription (**i, l, t,** or **a**), which Insley has not taken into account. Insley suggests the element *hapu-*, found in the seventh century Blekinge inscriptions, as an example of influence from West Germanic. He considers this element to be found in **hadulaikaR** on the Kjølevik stone since there is no reason to prefer the proposed emendations **h<âi>du-, ha<r>du-, or ha<n>du-**. Insley here misses the basic

problem with *hapu-*: in the period of the Kjølevik stone it would have been written **hapu-** not **hadu-**. The voicing of [þ] to [ð] between vowels occurs later.

In "Yew Wood and Runic Inscriptions in the Frisian Terp-Area" Tineke Looijenga presents her investigation of the authenticity of Westeremden B. Using high-power microscopic photography she could study the carved lines. Nothing indicates a forgery, including the circumstances of the find. The age of the stick cannot be decided until a C14-dating is carried out.

"The Bones with Runic Inscriptions from the Lower Weser River. New Results of Scientific Investigations Concerning the Problem: Original(s) or Fake(s)" by Peter Pieper is an English summary of the same author's thesis in German, in which he proves the runic inscriptions from Weser to be genuine.

Bengt Odenstedt participates with a review: "A New Theory of the Origin of the Runic Script: Richard L. Morris's Book *Runic and Mediterranean Epigraphy*". To review a review may seem strange, and indeed this is not a full-fledged attempt to do so, but Odenstedt's review is very interesting and does contain some new material compared to his own monograph on the subject: *On the Origin and Early History of the Runic script. Typology and Graphic variation in the Older* Futhark (1990). Odenstedt designates the Latin alphabet around the year 1 as the sole source for the runes, whereas Morris advocates the fifth century B.C. Greek letters. Given the chasm of difference in approach, material and results between the two scholars, Bengt Odenstedt's treatment of his "opponent" is very fair and appreciative; he is able to be both pleasant and very critical at the same time. Since much of Odenstedt's criticism of Morris is a restatement of his earlier work, I will not enter into that discussion here (but I may return in another context, cf. my review of Odenstedt in *Arkiv för nordisk filologi* 107 and his reply in Umeå Papers in English 16). Odenstedt's constructive review of Morris will very likely be the best judgement of this book. Odenstedt is able to see both the merits and the flaws. He rightly commends Morris for his striving for objectivity, his excellent introduction to and thorough knowledge of previous theories on epigraphy, and his exhaustive discussion of all aspects of epigraphic influence. He also justly points out Morris's inferior basis for the investigation, his uncritical dependency on the work of Elmer Antonsen, and his sometimes very far-fetched derivations of individual runes, obviously aimed to make the facts fit his theory. Because of the valuable information contained in the book it will become a standard reference work, in spite of its deficiencies. Morris's main weakness, as pointed out by Odenstedt, is his untenable dating of the origin of the runic script. Odenstedt proves the incorrectness of Morris's statement that "with each new runic find, our dating of the birth of the runes must be pushed further back in time". Odenstedt devotes some space to reiterating his arguments against the "runicity" of the Meldorf inscription (*c.* 25 AD). Even if it would be runic (which I think is possible), Odenstedt's criticism is still valid: Morris's theory presupposes that no inscriptions from the first 500-700 years of runic writing have been found. This is clearly extremely improbable. At the same time Morris does present some strong parallels between Greek and runic epigraphy, especially the multi-directional writing, non-representation of nasals, interpunctuation, and use of letter names. Odenstedt's attempt to disarm these arguments is not entirely successful, yet his conclusion must be correct: these parallels cannot be seen as pre-classical Greek influence on runic writing.

Alfred Bammesberger's main contribution "Ingvaeonic Sound Changes and the Anglo-Frisian Runes" is a study of the important problem of why, when, and how the new runes were added to the *fuporc*. When the use of the runes was changed from a West Germanic to an English context, later sound changes affected the names of

the runes. Old ways of writing a word - "spelling is always traditional" according to Bammesberger - gave the wrong impression of its pronunciation. But only certain sound changes led by necessity to new runes or forms of runes, e.g. /a/ > /æ/ and possible /ai/ > /a:/. Bammesberger posits stages where certain runes were mere allographs, e.g. ᚠ and ᛟ at one time both represented /o/. Bammesberger also discusses the Undley, Chessel Down, and Caistor-by-Norwich inscriptions on the basis of his previous findings, but he cannot present any definitely better interpretations. Alfred Bammesberger addresses an important and classical problem within Old English runology. It has not yet received a complete explanation, and I feel that Bammesberger's article would have contributed more if it had been a more structured effort. "Traditional spelling" muddles the issue. The same goes for the hypothesis of runes momentarily turning into allographs. The main problem as I see it is why rune number four, ᚠ, received the extended branches and not the æ-rune. Ray Page explained this addition as a kind of ligature with the **n**-rune, seemingly the only possible solution, albeit problematic. This indicates that the old ᚠ for a time was used for /a/ or/and /o/, before it was needed for /o/ after ᛟ > /oe/. It should be possible to work out the chronology of this development.

That Óðinn was considered the inventor of the runes is well-known from the Old Norse *Hávamál*. Many scholars believe that this poem is heavily influenced by Christianity and the scene with Óðinn hanging on the sacrifical tree reeks of parallels to Christ on the cross, but there are also important differences. In the fascinating article "Hermes-Mercury and Woden-Oðin as Inventors of Alphabets: A Neglected Parallel" Rolf Bremmer makes probable that the invention of the runes was attributed to Oðinn only through secondary influence from his equation with the god Hermes-Mercury. By the intermediary of pagan Romans or by early Germanic Christians the idea that it was Mercury who invented the runes was transferred to Woden. This tempting theory also explains how the Woden deity, with his roots in the non-literate Indo-European period, was credited with the art of writing. Bremmer sums up his argument: "[W]hile it is already an accepted fact that the Germanic alphabet has its roots in the Mediterranean world, the conclusion lies at hand that the origin of the myth of Woden-Odin having invented the runes must be sought there, too." His proposal rhymes well with the fact that the Germanic culture often associated itself with the classical world, as can be seen in, e.g., Snorri Sturluson's Trojan origin myth (cf. Michael Srigley in *Tor* 22).

Another classical question is the subject of Edgar Polomé's "The Names of the Runes". Polomé believes that a reexamination of the problem will provide new information on the rune names. His study is, by and large, an eclectic repetition of older ideas, coupled with some observations of his own. He does indeed provide interesting parallels and isolated observations but fails, as he himself admits, to get a definite result of his extensive survey. His modest conclusion is that there is not a "complex proto-Germanic cosmological system" reflected in the names of the runes, but that such a system is imbedded in these concepts. The original nature of the names was ideographic and their primary purpose was divinational. Perhaps it is asking too much, but from this competent scholar I had expected more than the general statement, already made by Ralph Elliott, that the names of the runes can be divided in groups that relate to the gods, to the things in nature, and to objects and emotions in the world of men. As for the discussion of individual rune names I shall only question Polomé on one point, viz. his acceptence of Wolfgang Krause's theory that the original name of the **l**-rune was **laukaz*, not **laguz*. The case is made on the form given in *Codex Leidensis* which contrasts with all the other manuscripts. Krause rests his argument on the fact that the word *laukaR* appears on

several runic items. However, so do *lina, lapu*, and above all *alu*, without anybody ever having tried to ascribe to one of these the status of an original rune name. Still, Krause (and Polomé) may be right on this point, in which case an important characteristic of the rune names is exemplified: their names may alter but not in a way to change the acrophonic value.

The final contribution is a minor monograph by Elmar Seebold entitled "Die Stellung der englischen Runen im Rahmen der Überlieferung des älteren Fupark". Seebold thinks that Ludvig Wimmer made a mistake to exclusively use the Nordic inscriptions as a basis for his theory of the rune's origin. By including all of the inscriptions from the entire Germanic area, Seebold maintains that the runes were derived straightforwardly from the archaic Roman alphabet, not as Wimmer thought the 3rd century AD script, hence the **e**-rune was borrowed from ancient II (>Π, with a "Markierung der Zusammengehörigkeit" to explain the *horizontal* branch). According to Seebold the runic creator followed certain principles such as using at least one vertical staff and avoiding curved or horisontal lines and branches attached only to the bottom of the staff. Four runic variants that show "fuller" forms (ᚠ **z,** ᛈ **p,** ᚺ **h,** and ᛝ **ŋ**) are declared original, without mentioning the chronological problems arising from such a hypothesis. The Meldorf inscription is declared runic, on the grounds that its letters deviate from Roman forms, and Seebold reads **idin** or **ipin** without any interpretation and without any references to earlier scholars. Nevertheless, his opinion that the Meldorf inscription records early runic forms of **d** and **n** deserves consideration. Seebold then tries to come to grips with the North and East Germanic rune forms. The inscriptions are here divided into geographical and chronological groups. The bracteates are separated. Seebold investigates the rune forms and draws a number of interesting conclusions. The notoriously corrupt-looking bracteate runes are the basis for much of the results from the study of the "late Danish" inscriptions. Seebold's basic position is that seemingly degenerated forms could be viewed as the inception of later development. Thus he asseverates to have found "new vowels" represented by runes, e.g. [y] and [æ] and furthermore that late rune forms for **k, z**, and **A**, just as the rune stones themselves, were introduced into "Scandinavia" from Denmark. Unfortunately, Seebold's results are based on some very questionable interpretations, and the complicated dating problem is ignored. At least one example of his *modus operandi* must be mentioned. The Broholm bracteate with its two strings of irregular-looking runes is "interpreted" by taking every second graph (but skipping the last letter of both groups), thereby procuring the inscription "**uihu - wiho -wiuu**" which is supposed to be "das wort **wihu** 'ich weihe'" written thrice "aber in verschiedenen Runen-Traditionen". The reduction of graphemes in the younger *fupark* is partly given a phonological explanation, but Seebold introduces a new reason for the loss of the **g-, d-**, and **p**-rune. These graphemes, according to his own theory of the rune's origin, are derived from the "doubled" **k-, p-**, and **b**-rune respectively. The creator(s) of the younger *fupark* knew of this, and therefore (possibly for magical reasons) felt that the "Verdoppelung" could be "zurückgenommen". The difficulties with the time span between the invention of the older and younger *fuparks*, and the fact that the **d**-rune rather should contrast with the **t**-rune than the **p**-rune, are brushed aside. The Continental, Frisian, and especially the English manuscript runes are also dealt with in detail. The origins of the **æ-, oe-**, and "new" **a**-runes are discussed at length and the great value of the manuscript tradition is stressed, but in my opinion given too much independent weight. The manuscript sources are listed and a number of individual runes are discussed: **j**, *ior* , *ear* (which is here derived from a misunderstood name for the **z**-rune), and the **c-k-q-x** complex. Seebold also studies some of the

manuscript sources: the *de inventione* tract, the *Pseudo-Nemnivus* alphabet, and the *Isruna* tract *fuporcs*. Finally, the rare *stan*-rune is derived from a form of the old **p**-rune. As a conclusion one may note that Elmar Seebold's study is much of the same scope as Bengt Odenstedt's above mentioned monograph on the subject, but must be considered inferior to it and much less well structured. Large portions of his text, especially the bracteate part, are opaque to me. Much of what is said has been stated before, and often better, by somebody else. Seebold's own contributions are too speculative to convince; one gets the impression that he allows every one of his thoughts to find its way into print.

Old English Runes and their Continental Background is full of fruitful ideas. It sometimes presents more of an outline than a full picture, but also includes many exciting details. It is the most valuable addition to the study of Old English runes since Ray Page's "little red book" from 1973.

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