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This collection of eighteen papers presented to a conference in 1990 illustrates the wide range of areas encompassed by 'runic studies' or 'runology', as the subject has come to be known. There are contributions relating to medieval Scandinavia, Viking-Age Scandinavia, pre-Viking-Age Scandinavia (and contemporary Germany), Anglo-Saxon England and early Frisia. Approaches to the material within this range of geographical areas and periods include deciphering individual inscriptions, commenting on their linguistic features, explaining developments in orthography and representation, seeking the historical potential in patterns of inscriptions, and examining the place of runes within a wider literate culture. Yet all of the articles that adopt such approaches to specific texts and groups of inscriptions are overshadowed, at least for the reviewer who reads the collection from beginning to end, by the first essay, 'On Types of Argumentation in Runic Studies', by Michael Barnes.

Professor Barnes has written a powerful appeal to runologists to put their house in order. He describes a discipline in which, all too often, 'intellectual rigour is kept firmly at bay', and he identifies four main headings under which common errors can be grouped:

1. Claims are made based on little more that the author's conviction.

2. There is too scanty a knowledge of other disciplines, often coupled with a lack of the intellectual rigour demanded by those disciplines.

3. Conjecture is silently transformed into certainty.

4. General principles are referred to or implied in support of arguments, but the principles are not enunciated, are of questionable validity, or are contradicted by the data.

He goes on to exemplify each category from the work of past and present scholars, and concludes by offering a list of nine steps 'that practitioners of runology could take to tighten up the discipline'.

As a compelling opening to a conference or conference-volume this could hardly be bettered. Barnes is extraordinarily frank, and bold too, for a number of those specifically criticised were present at Grindaheim. And what he has to say may be uncomfortable, but to a large extent it is clearly true. The many detailed instances of error, confusion and assumption that he presents would be difficult to defend. I find myself out of sympathy only with his insistence on rigorous linguistic notation (of which more below); the examples of false assertion, circular argument and blatant misunderstanding speak for themselves. There is more to discuss and question, as Barnes himself accepts, in some of the steps proposed for the future, and these will be taken up at the end of this review. First, however, the temptation to look at a few of the collection's other essays in the light of the principles advocated by the first is irresistible.

Two authors take on the interpretation of the Westeremden B inscription, a short stick of yew-wood found in a Frisian *terp* in 1917, with three sequences of runic characters that contain several problematic shapes as well as forms distinctive of developments in England and Frisia. Arend Quak examines a recently proposed 'solution' to the inscription, and finds it wanting because there are problems in identifying at least five of the rune-forms used in the text. He suggests alternative values for these characters, but it lies beyond his ingenuity to extract a full interpretation of the inscription from the challenging transliteration that he has produced. Ottar Grønvik, however, takes up the challenge and produces a dramatic connected text from Quak's transliteration. (The strange circumstance by which one paper builds on the conclusions of another presented to the same conference is explained by the fact that some were circulated in advance; Grønvik's contribution was in response to Quak's pre-print.) Grønvik's interpretaion runs: 'Auf der Heimstätte gedachte *Æmluþ* der Missetaten des Volkes; auf den Knien (kniend) schwiegen sie alle', a text that is explained as referring to the arrival on Frisian soil of a Christian missionary. Moreover, the author feels able to date the text on linguistic grounds to the years before c. 770, to identify *Æmluþ* as an otherwise unrecorded byname of the Anglo-Saxon missionary Boniface, and thus to place the in-

scription, 'mit recht großer Wahrscheinlichkeit', in the context of Boniface's mission to Frisia in 753-4.

Grønvik may have read Quak's pre-print, but he clearly did not receive Barnes's. It would be hard to imagine an argument better designed to illustrate the worst of the practices Barnes identifies. We need go little further than the transliteration. The inscription contains forty-one characters: by my calculation thirteen of them, representing nine distinct symbols, are unknown or problematic in an Anglo-Frisian, or early Germanic, context. Quak properly drew attention to several of these in casting doubt on earlier readings; it was perhaps proper also to investigate alternative identifications for the difficult characters. Yet the grounds upon which the new values are proposed can only be speculative. A plausible suggestion of a value for a difficult character should, one might think, be supported by context. The resultant text should make clear sense and/or be closely paralleled elsewhere; alternatively there should be unambiguous parallels for the values in comparable and easily intelligible inscriptions. The suggestions here lack these kinds of context; as a basis for further interpretation, therefore, they are very weak.

To take examples. Two of the characters are explained by Quak as rare 'mirror-runes', related to a type identified at Illerup (Denmark) and Spong Hill (England). In third-century inscriptions from Illerup the runes **þ** and **w** appear with the pocket that usually appears on one side of the stave cut on both. The identifications are secure because the words in which the runes appear can be easily recognised; **w**, for instance, comes in **tawide**, 'made', a verbal form that is found elsewhere amongst early Scandinavian runic inscriptions. In fifth- or sixth-century inscriptions from Spong Hill the runes **a**, **l** and **u** appear similarly mirrored on either side of a vertical stave. This reading convinces because the resultant **alu** is the single most common sequence evidenced in Scandinavian inscriptions of the Migration Period, because the archaeological context at Spong Hill suggests a community of settlers from southern Scandinavia, and because there are other possible and probable echoes of contemporary Scandinavian 'charm-words' in early Anglo-Saxon inscriptions from eastern England. There is no comparable support for the identifications on Westeremden B where, moreover, the proposed mirroring is of a different type (from top to bottom, rather than from left to right). A mirrored **m** is identified in two sequences, which may read **gimnda** and **mæhþukn**, though it is difficult to say, partly because there is no

indication of word-division, so that it is impossible to define a 'sequence', and partly because the instances of **g**, **n**, **æ** and **k** are also uncertain. A mirrored **e** of similar type is identified in **suedun** where, again, the **n** is problematic.

If, from these transliterations, a text similar to one known elsewhere in Frisia or Anglo-Saxon England smoothly emerged, if at least there were sequences of familiar-looking words, there might be some merit in the identifications. Instead an interpretation has to be forced from the unpromising material by assertion, assumption and spurious parallel. Grønvik tells us that **gimnda** 'steht sicher für' *gimunda*, equivalent to Old English *gemunde* 'remembered, bore in mind' (though hardly, so far as I can see, the 'recorded, made mention of' that the interpretation requires). The missing **u** is excused on the grounds that the syllable *-mun-* seems to be written that way also in one or two Viking-Age Scandinavian inscriptions. The sequence **suedun**, on the other hand, is explained as 'eine ungenaue Schreibung für /suwedun/'. When we remember the extreme uncertainty of the transliteration in the first place, this kind of proposal is difficult to take. When the whole interpretation gives a text so unlike anything else known from runic inscriptions anywhere, when a date is given to it on linguistic grounds that would be thoroughly questionable even if the interpretation were certain, and when a precise historical context is then proposed on the strength of a case thus built on layer after layer of quicksand, then one certainly sees what Barnes means about 'the futility of pronouncing on matters where the insufficiency of data precludes the drawing of plausible conclusions'.

Such examples are not, fortunately, the whole story. Lena Peterson tackles a similar problem involving inscriptions that use unusual runes – she advances a hypothesis, examines it cautiously, and justifies it conclusively. Peterson is very clearly aided by the fact that there is a sample of several inscriptions to work with, and that 'Thanks to our knowledge of many hundreds of other rune-stones and due to the stereotyped nature of the messages on them, it is not difficult to arrive at ... reading[s] and interpretation[s]'. Given this context, one might consider her task much easier than attempting to interpret Westeremden B. Alternatively, one might simply regard it as *possible*.

Peterson is concerned with the variety of Scandinavian runes known as 'staveless' and attested principally in the Swedish province of

Hälsingland. She reviews earlier analyses of this variety of the script, which is clearly based on a normal Viking-Age *fuþark* with the vertical staves omitted. Two runes, the fourth, named *ás*, and the seventh, **h**, have proved elusive, however, and have not seemed to be attested on the major rune-stones of this type. Peterson's argument is that **h** is indeed found on several of the rune-stones, and that the feature that distinguishes it from **f** (simply that **h** is significantly shorter) has been overlooked. She proceeds by showing that in several inscriptions **h** gives at least as good a reading as the previously accepted **f** (e.g. **hrumuntr**, *Hrō(ð)mundr*, against **frumuntr**, *Frøymundr*). On no Swedish stone is **h** the only possible reading, but in none is it excluded. All other available evidence, including three portable objects from Sigtuna, is methodically reviewed, and its indications carefully weighed. Several points that could support the theory are warily rejected, before the clinching detail is introduced: a rune-stick from Bergen with a staveless *fuþark* in which **f** and **h** are distinguished exactly as Peterson proposed. Even then she is cautious about the possible disparity in date between the Bergen stick and Hälsing stones (though evidence for the date of the latter is extremely hard to find). But few could argue with the conclusion that she has demonstrated, within the limits of currently available evidence, that her hypothesis is correct.

My only quarrel with Peterson's article is a minor one, entirely peripheral to its main concern. Early in the argument, she becomes embroiled in a complex and ultimately fruitless discussion of the definition of a grapheme. At one point she announces, ominously: 'There is only one way out of this dilemma, and that is to employ a strict, non-relational graphemic analysis'. Yet, as far as I can see, the page of analysis which follows leads nowhere, except to the conclusion that 'one cannot entirely escape the semantic factor in a graphemic analysis'. Paraphrased, this means that the identification of a distinct character stands or falls on whether or not it makes sense in context ... which is what the remainder of Peterson's paper goes on clearly to test and to prove.

A scholar who employs the terminology and notation of graphemic analysis to a much greater extent is Terje Spurkland. His article examines the evidence for two post-Viking-Age rune-forms in Norway, one with the shape of a b-rune containing two diacritical dots, the other a form similar to roman K. He aims to show that both variants were used to render /p/, that dotted **b** was probably an earlier

innovation, that both forms nonetheless existed side by side for a long period (though never appearing together in the same inscription), and that K generally remained in use later. Finally he offers a plausible explanation for the emergence of the K-variant: since a dotted t-rune gave voiced /d/, and a dotted k-rune gave voiced /g/, then a dotted b-rune giving *unvoiced* /p/ may have been perceived as an anomaly. The K-shape was therefore perhaps invented as an alternative.

Spurkland's article is supported by details of inscriptions, and by the archaeological dates of finds. There is no apparent lack of intellectual rigour and the conclusions are worthwhile. Yet the whole paper is couched in the technical language of linguistics, which makes it hard to read, and in my view confers no particular benefits of precision or clarity. Compare parts of my summary with Spurkland's statements: 'Some time before 1175 we get the graphic simplification of (b); the innovation (K) and the traditional dotted (b) are then facultative allographs of the grapheme <p> (or <K>, since it is arbitrary how one designates a grapheme)' or 'The system called for a graphophonological shift, and the new allographs of <p> were created, the (K) variants, which did not clash with the general dotting disambiguation rule'. Such terminology can be followed, but I wonder if it is necessary.

Neither Peterson nor Spurkland appears to fall into any of the intellectual traps Barnes describes. Yet by using terms borrowed from linguistics they bring us back to an area with which he is much concerned. Under the heading of 'ignorance of other disciplines', Barnes presents a selection of scholars' attempts to describe the value of the twenty-second rune (which I would warily characterise as the sound(s) or cluster represented by the consonants in the rune-name *ing*). The definitions are confused and contradictory, and lead to the advice from Barnes that 'all serious practitioners of runology should acquaint themselves with basic phonemic theory and with the conventions of phonemic and phonetic notation. This would enable them to make clearer statements and would help readers to understand more quickly and easily what they mean'.

There are three comments to make about this suggestion. The first is that Barnes may here be guilty of taking some quotations out of context. For instance, he says:

> Derolez ... reckons Anglo-Saxon *Ing* 'stood for the final sound of its name' (presumably [g] ...) and that therefore '*n* was about the best rendering' in *runica manuscripta.*

In its original context, however, it is clear (i) that Derolez himself envisages the 'final sound' of the name to be one expressed by the pair of roman letters *ng*, and (ii) that the phrase '*n* was about the best rendering' is presented entirely from the point of view of a medieval scribe looking for the best single-letter equivalent to place beside the rune-shape. Derolez is not trying to present a detailed linguistic assessment here; and the use of phonemic notation in this context would be a nonsense. The second comment is perhaps merely a personal one. I am acquainted with basic phonemic theory, but tend to choose not to use its notation when working with early Anglo-Saxon runic inscriptions, simply because I could not claim to understand in detail the sound-system of the language(s) they record. One has to employ some sort of notation, of course, and under these circumstances it is bound to be a compromise, but phonemic notation is not my first choice – it seems to me to imply a 'scientific' precision which the evidence will often not support. Thirdly, I would cite Spurkland's graphemics as a cautionary warning against the view that linguistic notation will necessarily help readers to understand more quickly and easily.

Before pulling together some conclusions, I want – albeit very briefly and uncritically – to summarise two further articles in the collection to give some idea of the range of areas covered. Birgit Sawyer writes about the memorial rune-stones of Viking-Age Scandinavia, arguing that patterns in the overall distribution of the stones and in the phrasing of the inscriptions are to be explained in relation to social and political history. In particular, she argues that the rune-stones of the town of Sigtuna, founded at the end of the tenth century, reflect its status as a royal and Christian centre. The distributions of stones in the area around the town may indicate, further, the differing rate and nature of the extension of Christianity and royal power into the surrounding Mälar region. Christine Fell examines aspects of the relationship between runic and roman scripts in Christian Anglo-Saxon England. She traces the choices made by the literate from Latin language and roman script, to English language and either roman or runic script, to English language and a third script, the blend of roman, occasional runes and

other characters, with which readers of Anglo-Saxon manuscripts and printed texts are familiar. She indicates several regional and chronological areas that can be distinguished in these respects, and discusses the differences in attitude that they imply. It does not take detailed analysis to indicate that articles like these draw on types of evidence and methodology rather different from those that have been so far discussed, a point which will be of importance as I turn to some concluding comments.

It was noted at the beginning of this review that Michael Barnes drew from his criticisms a series of positive steps that might be taken to rectify the situation. One can have no quarrel at all with several of these, such as:

> 2. We must ask ourselves not just: 'What evidence is there in support of my theory?' but also (and preferably more often than the former): 'What evidence is there against it?'
>
> 7. Theories and ideas should be recognised for what they are and never referred to as though they were facts.

Yet there is much to disagree with in the implications of the critical first step:

> We must try to decide what the discipline seeks to achieve, what it should reasonably encompass and what the constraints are under which it must operate. Is runology just an extension of epigraphy, alphabet history and historical linguistics, or something separate? If separate, should there not be some basic ground-rules and definitions?

My objections to this revolve around the idea of a discipline of runology. I had always regarded 'runology' as a pseudo-scientific, rather light-hearted term for an amalgam of perfectly serious, but only loosely connected, subjects. Of course the connection is very obvious: all 'runologists' study runes, an alphabet apparently invented by Germanic-speaking people and used almost (but not quite) exclusively to record Germanic languages. Yet the alphabet, like the languages, underwent many different developments over a long period of time, and spread to widely separated lands with quite distinct cultures. It is difficult to see in what sense, apart from the

most vaguely general, a historical study of Sigtuna rune-stones might share a common aim with the linguistic interpretation of a Frisian *terp*-find. And such an example only scratches the surface of the range of specialisms encompassed by the runic scholar. The bibliographical 'key-words' at the front of this volume, when they have finished with words involving 'rune', run as follows:

> epigraphy, graffiti, Old German, Old Scandinavian, Old Norse, Runic Danish, Runic Swedish, Old English, Old Frisian, language history, orthography, onomastics, historical phonetics, graphemology, history, social history, archaeology, writing systems, alphabets.

In these circumstances, just as I object to a centrally imposed system of linguistic notation, which may suit some circumstances but not all, so it seems to me problematic to talk of 'basic ground-rules' that runologists, as distinct from other scholars, should observe.

To take perhaps an exaggerated example. The inscriptions of Roman Britain and the manuscripts of Anglo-Saxon England employ varieties of the same script, and their relationship is thus of the same *kind* as that between any two runic areas. No-one, of course, would group them together for study under the heading of a single discipline. Yet the effect would not be so very different from juxtaposing, say, the runic finds from Illerup bog and the Ruthwell Cross. Indeed, one might find more points of contact in terms of place, social context, religion and language between the first pair than the second. To begin to set the Illerup runes in context one needs to be familiar with the archaeology of the Iron Age in Scandinavia, with the practice of ritual weapon-deposit, and with the earliest fragments of Germanic language in a theoretical linguistic framework. For the Ruthwell Cross one needs to know about Christian memorial stones in northern England that use runic and roman scripts, about Christian scripture in Latin, about an Old English crucifixion poem in a manuscript written in southern England and recovered in Italy, and about a Germanic language widely attested in a variety of written and carved sources. The study of both Illerup and Ruthwell presumably falls within the ambit of 'runology', though the runes in each case are also quite distinct. Yet, it is difficult to see that *methods* of study are so particularly comparable that they would lend themselves to being regulated together. Indeed, to group all runes

into a 'discipline' with common aims and procedures, seems to me indirectly to encourage the wayward scholar who argues that a rune can be supplied in an inscription of the ? eighth century from Frisia on the grounds that there may be a parallel with Scandinavian texts of much later date.

None of this is to say that runologists should no longer gather together, nor that there is not a great deal of wisdom in Michael Barnes's advice. But, in my opinion, this wisdom lies primarily in the suggestion that scholars should cultivate a critical attitude and a degree of self-criticism towards their work, advice which applies equally to all researchers in the humanities. I am not convinced that runologists need discipline that is any different.

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