Futhark
International Journal of Runic Studies

Containing proceedings of the
Seventh International Symposium on Runes and Runic
Inscriptions, Oslo, 9–14 August 2010

Part 1: Plenary lectures

Main editors
James E. Knirk and Henrik Williams

Assistant editor
Marco Bianchi

Vol. 4 · 2013
What Is Runology, and Where Does It Stand Today?

Michael P. Barnes

Abstract
The purpose of this contribution is to offer a critical appraisal of runology as currently practised. The article begins by asking what runology is, and possible ways of defining the subject are discussed. Theory and methodology are then considered. While there is much to be learnt from analysis of the methods runologists employ, the search for runological theories turns out to be an unrewarding exercise. Theories from other disciplines have on occasion informed and guided runological procedures, however, and this is exemplified through an examination of the role graphemics has played in recent discussion of rune forms and how they may best be transliterated into the roman alphabet. The article concludes with brief consideration of problems that have arisen in the reading and interpretation of runic inscriptions, and a plea is entered for a critical and dispassionate approach to runological endeavour.

Keywords: runology, theory, methodology, graphemes, transliteration, interpretation

Introduction

In recent years several scholars have presented critical examinations of the state of runology. They have offered varying views of the subject and suggested different ways forward. Most recently James Knirk has obtained funding for a project (Lesning og tolkning av runeinnskrifter: runologiens teori og metode, ‘The reading and interpretation of runic inscriptions: the theory and method of runology’) whose principal objectives are: (i) to define runology as a subject or field of research; (ii) to establish the theoretical (philological) basis for runological research; (iii) to evaluate and develop the subject’s methodological tools. The most
important outcome of the project is to be a *Handbook of Runology*. Such a work will differ from previous introductions to the subject. Rather than offer a general survey of runes and runic inscriptions, it will lay down a methodological basis for the study of runic script and the examination, reading and interpretation of inscriptions. For the first time a text-book will be available for the student of runes, showing how things should, or at least might, be done. This is a project of some significance. Runic writing is of considerable linguistic, historical and cultural importance, and the study of runic writing in all its aspects is badly in need of critical reappraisal. Those working with runes require at the very least: (i) a definition of the subject; (ii) a statement of accepted, or at least acceptable, methodological procedures; (iii) a series of constraints within which they can work. Without these minimum requirements “runology” is anything and everything: it is unclear where it begins and ends, and there is no obvious dividing line between the serious practitioner and the charlatan.

An individual scholar like myself clearly cannot anticipate the outcome of an in-depth investigation into the theory and method of runology. One elderly academic of modest talents is no substitute for the eminent team who are to compile the proposed *Handbook*. I shall be unable, I fear, to define runology, to establish a theoretical basis for runological research or to develop the subject’s methodological tools—not least within the confines of a symposium paper. My aims are rather to raise and discuss a number of questions of the kind I imagine the compilers of the *Handbook* will have to tackle.

A definition of “runology”

Between them the authors referred to suggest or imply that runology comprises elements of linguistics, philology, palaeography, archaeology, cultural, religious, legal, literary and art history, mythology, cryptology, and occultism; others might want to add further fields of research, onomastics for example. Many of the contributors stress approvingly the interdisciplinary nature of runology (in part, perhaps, a genuflection to the gods of our time).

Two important questions arise from a claim made by Spurkland in his contribution: “Skal man operere med et vitenskapelig fagområde ‘runologi’, må det ha elementer av arkeologi, kulturhistorie og språkvitenskap i seg” (‘If there is to be a discipline called “runology”, it must contain elements of archaeology, cultural history and linguistics’; 1987, 56). But how obvious is it, I wonder, that runology should constitute a discipline in its own right? And how can one define a discipline that comprises so many disparate elements? Certainly, if a discipline cannot be defined, it is hardly meaningful to treat it as such. Peterson (1996, 41) argues for a narrow and a broad definition of runology. According to her narrow definition, it is a branch of linguistics; more broadly defined it includes analyses and evaluations of objects of study that bear witness to all kinds of circumstances of the past other than the purely linguistic. For my own part (Barnes 2011a, 147–49) I have questioned whether the study of runes and runic inscriptions requires the establishment of an independent discipline, noting that ogam, for example, does not seem to have called forth generations of ogamologists. I further enquire how much sense it makes to treat runic writing in comparative isolation from writing in rival scripts. On the other hand, as Lerche Nielsen emphasises (1997, 37), runic research is one of the oldest branches of scholarly endeavour in Scandinavia, with roots stretching back to the Renaissance; so it has the weight of tradition behind it. Also to be considered is the need for a theoretical and methodological basis on which the study of runes and runic inscriptions can be built, and that must surely start with some attempt to define, or at least delimit, the field as an area of academic activity?

Although I cannot offer a ready definition, I have a reasonably clear view of what runological research essentially entails. Runes are an alphabetical system of writing, and for the most part they are used to record language. An independent runological discipline, if it is to be established, must therefore deal with the runic symbols themselves, individually and
as systems, with their development, and their use to record language. Runic inscriptions are sequences of runes placed on an object, and these the runologist will attempt first to read and then to interpret. Reading will involve examination of the inscription itself, since photographs are subject to tricks of the light and drawings will always contain an element of subjectivity. Interpretation will often require help from and some knowledge of other disciplines, notably archaeology. But archaeology is not runology, any more than are art history, mythology, or occultism.

I am struck by how many of the authors listed above approach “runology” as though it were first and foremost concerned with the interpretation of inscriptions. My view is rather different. Core aspects of the discipline are to me: the origin of the runic alphabet; the change from the older fuþark to the Anglo-Saxon fuþorc and the younger Scandinavian fuþark; the development of the additional runic characters of the Scandinavian Middle Ages and their status; runes as graphic systems; the distinction between graphs, graph-types, graphemes and units of the fuþark or runerow (see below); the principles and practice of transliteration. I would also suggest that the reading of runic inscriptions is more central to runology than their interpretation. The reading must be done first and must be undertaken by someone with experience in the field. Thereafter come attempts at interpretation, which may in some circumstances be made by historians, archaeologists and others — provided they possess the requisite linguistic knowledge, understand how the reading was arrived at, and have a proper grasp of all the caveats the reader has expressed.

I thus disagree with Spurkland that runology must contain elements of archaeology and cultural history. The runologist will almost certainly need to consult archaeological and historical expertise, but archaeology and history are not runology. Runology is to do with runes and runic writing. There will of course be grey areas. Peterson asks (1996, 40) whether an archaeologist who makes detailed analyses of the grooves of runes and of the ornament of rune-stones is a runologist. On the whole, I think not. Such a person is rather an archaeologist mapping the grooves cut on stones. The results arrived at may assist the runologist, but the endeavour is not strictly runological. This can be true, wholly or partly, of a wide range of studies that take runic inscriptions as their subject matter. Jesch (1994), for example, is a thoughtful and salutary contribution, which discusses difficulties in undertaking quantitative analyses of the information in runic inscriptions. However, many of the questions raised concern the collection and sifting of data and as such apply to quantitative analyses in general.
I have approached the defining of runology chiefly as a theoretical problem but I want to emphasise the practical aspect as well. As indicated earlier, the more elements of other disciplines “runology” is to include, the harder it becomes to define, and if we cannot define it, we can hardly map out its methodology and practice. Which would take us back to square one.

Runological theory

In his project description Knirk questions the use of the term “runology” on the grounds that the lack of a fundamental theoretical and methodological apparatus is palpable enough to render the “-ology” suffix inappropriate, suggesting as it does the existence of a well-established discipline. I agree that “runology” can give false associations (though on a practical level attempts to avoid “runology” and “runological” can lead to some fairly tortuous English). What I am far less certain about is where the theoretical basis of runology is to be found.

First, I suspect, we would need to agree what “theory” means. Much in runic research that goes under this heading would be better called “idea”, “suggestion” or “claim”. Consider, for example, “Theorien über die Bildung des älteren Runenalphabets” (‘theories about the creation of the older runic alphabet’; Grønvik 2001, 10–28); the “theory” that the total number of runes in a rune-row had to correspond to a multiple of eight (Spurkland 2005, 80); and the “mange teoriar” (‘many theories’) that have been advanced to explain the relationship between the long-branch and short-twig runes (Liestøl 1969, 474). If, as the Oxford English Dictionary suggests, and linguistic science endorses, a theory should be a statement of general laws or principles and have explanatory power, it is not easy to see where the theoretical basis of runology might lie.

Workers in the runic field have on occasion appealed to general principles, but it has not always been clear what the principles were or how they impinged on the argument. Erik Moltke, for example, would refer to the laws of alphabet history (e.g. 1985, 68, 173 f.), but I cannot see that he ever expounded these in detail so that the validity of his assertions could be checked. Braunmüller (1998, 16–20), attacking the idea that the runes were invented and used in “splendid isolation” from other cultures, appears to argue that language contact is the force behind almost all linguistic innovation and change, and that explanations in runology must, where relevant, take account of that circumstance. He instances in particular the change from the older to the younger fuþark. This has
perhaps the makings of a theory, but I am not sure it is borne out by the facts. There was undoubtedly contact (and thus presumably language contact) between the Roman Empire and Scandinavia at the time most people assume the runes to have been invented, and several runes look very like roman capitals and have more or less the same sound values. But how much contact was there with the Mediterranean world and its languages in the period A.D. 550–700? Braunmüller envisages a group of rune-carvers at this time with “fundiertes linguistisches Wissen” (‘soundly based linguistic understanding’), their insight gained from schooling of a type inherited from the ancient world and maintained through contact with the South. Only this, in his view, can explain the systematic nature of the change from the older to the younger fuþark. Michael Schulte, on the other hand, argues that the younger fuþark was the result not of a reform but of a gradual process of evolution (see, e.g., 2006a; 2006b; 2009). He contrasts developments in Scandinavia with those in Anglo-Saxon England: the relative cultural isolation of Scandinavia meant that the impetus to bring the rune-row up-to-date was lacking there, while in England the presence of the roman alphabet turned rune-carvers’ minds to thoughts of reform. The loss of g and d from the Scandinavian rune-row, which most have regarded as the result of a conscious decision, can only be put down to natural wastage, according to Schulte, because general “Verschriftungsprinzipien” (‘principles of rendering speech into writing’) dictate that change in usage will happen first, to be confirmed later by change in the system (2006a, 20). So here we have a possible theory that language contact lies at the root of almost all language change challenged (in this particular instance) by a possible theory that change in usage will precede any change in the system. For my part I would like to see both theories expounded in full with copious examples of how they work in practice. Even with such explication, I suspect lack of data from the period of transition between the older and younger fuþark would make it difficult to judge which theory, if either, had the greater explanatory power. And we would then be little further on.

Of course theory has played its part in runic research. I have myself appealed to phonemics and graphemics, and criticised those who write as though “speech sound” and “phoneme” were interchangeable concepts. But these are well-established linguistic theories used by certain runologists. They are not runological theories.

Many problems in runology are simply not susceptible to explanation by theory. The bulk of the Manx runic crosses are confidently assigned to the period c. 930–1020 by art historians. This conclusion is based on
observation and comparison. Some runologists (e.g. Holman 1998) have pointed out that certain rune-forms on the crosses as well as aspects of their language hint at a later date. That conclusion likewise derives from experience. In a paper from 1998 Hagland and Page suggest that the practice of dotting in runic writing may have arisen in the British Isles. Knirk has recently argued against that view (2010). Neither party to the debate appeals to theory, and with good reason. What we suffer from both here and in the case of the Manx crosses is lack of data, not lack of theory.

Finally: theory can often be so far removed from actual objects of study that it becomes hard to discern its purpose. A recent book by Piotr Chruszczewski (2006) summons up all manner of theories in an attempt to analyse twenty-nine Viking-Age runic inscriptions on stone from “North-Central Jutland” — the aim of the analysis being to show how various formulaic expressions employed in the inscriptions can shed light on discursive practices in the society from which the texts spring. The journey to the twenty-nine inscriptions is long. It takes in *inter alia*: anthropological linguistics, contact linguistics, language death, the development of Germanic from Indo-European, pidgins, creoles and mixed languages, Scandinavian invasions of the British Isles and their linguistic aftermath, the creation of writing, literacy, and the origin and development of the runic alphabet and runic writing — many of the topics accompanied by a fearsome theoretical apparatus. But the brief analysis of the texts of the twenty-nine inscriptions reveals very little. The conclusion is — as one might expect — that they do not differ greatly from the generality of Jutlandic or other Scandinavian commemorative inscriptions of the Viking Age. The relevance of the extensive and diverse theorising Chruszczewski brings to his task is to me wholly opaque.

**Runological methodology**

Methodology I find considerably easier to get to grips with than theory. Where theory can be abstract and vague, often seemingly far removed from immediate problems, methodology is at the heart of most serious runological endeavour. There has nevertheless been a general lack of thought about method and procedure — what might be deemed acceptable and what unacceptable, for example — as Peterson recognises in her critical 1996 analysis. In an admonitory tone she notes (p. 39) that as well as a definition of runology, a description of the aims of runological research, and a critical self-examination of the discipline by its practitioners, we
need an account of its methodology — the clear implication being that up to now there has been something of a methodological free-for-all.

Methods will of course vary, depending on the object of study. Field runologists examine inscriptions with a view to reading and interpreting them. Here, as Peterson avers (1996, 44), it is an essential part of runological method to distinguish between reading and interpretation. The runologist first attempts to read what s/he thinks to see. It is of course hard to do this without bringing to bear preconceived notions of what particular inscriptions might be expected to say, and that problem has to be freely acknowledged. Nevertheless, the reading should be primarily the result of observation. When it has been established to the runologist’s satisfaction, and presented in the form of a drawing, an idealised runic representation and/or a transliteration, an interpretation can be essayed. That will usually involve an attempt to identify words and to mould them together into an edited text, which can then be translated into a modern language to help non-specialist readers. It is an essential part of this process that all uncertainties in the reading and interpretation are made clear.

Methodological guidelines for the interpretation of inscriptions have so far been conspicuous by their absence, and it is certain much detailed work needs to be done in this area. Yet only if runologists indicate what is and what is not permissible can purveyors of make-believe such as Ole Landsverk and Alf Mongé (see, e.g., 1967) or Kjell Aartun (1994) be kept at bay. The difficulties are considerable, as Braunmüller’s article “Mutmaßungen zum Maltstein” (1991) unwittingly makes clear. Commendably, the author sets out to provide a set of interpretational procedures that can be checked and replicated. He does not know, or mind, he says, whether the interpretation of the Malt inscription he arrives at is right or wrong — his aim is solely to initiate a discussion about methodology and to indicate how things might be better done. The result, alas, is not reassuring. Not only does Braunmüller commit the cardinal error of confusing reading and interpretation, as Peterson stresses (1996, 44); his interpretation involves considerable textual emendation and the assumption that the Malt carver made widespread use of abbreviation. And it does not end there: while Braunmüller regards it as axiomatic that the carver adhered to the most common syntactic patterns of Viking-Age Scandinavian, he allows him where necessary for the interpretation to deviate from them — for example by using the reflexive possessive sinni where non-reflexive hans is to be expected.

Braunmüller can be forgiven for getting himself in a tangle (though perhaps not for the self-confident language in which his lesson in
methodology is couched). The way forward is much more problematic than he allows. By way of illustration I introduce the Reistad inscription from Vest-Agder, Norway (KJ 74, to be dated probably some time before A.D. 500). The inscription is in three lines and is normally read:

\[
i\text{uþingaz} \\
e\text{kakraz:unnam} \\
w\text{raita}
\]

The sequences \textit{iuþingaz} and \textit{wakraz} are interpreted as personal names (of disputed meaning); \textit{ek} (or perhaps \textit{ik}) is almost certainly the pronoun ‘I’. So far there is a measure of agreement. Antonsen (2002, 5) reads \textit{idringaz} rather than \textit{iuþingaz}, but still recognises a personal name here (which he “translates” ‘of memorable lineage’). The segment \textit{unnam wraita}, on the other hand, has been taken in two completely different ways: by many as \textit{undnam wraita}, meaning something like ‘undertook the writing’; by Thórhallur Eythórsson (1999) as ‘took Wraitaz’, where \textit{wraita} (acc. sg.) represents an earlier form of Old Norse \textit{reit} ‘marked-out space’, and is the name given to a farm (cf. the \textit{Rei-} in modern \textit{Reistad}). The first of the two words has also been read \textit{unnamz} (Antonsen 2002, 6 f.), whereupon it metamorphoses into a by-name qualifying \textit{wakraz}, and \textit{wraita} becomes a 1st person sg. past tense (strong) verb form: ‘I Wakraz, the untakeable, wrote [this]’. The only point the three interpretations of this part of the inscription agree on is that the sequence \textit{nam} is somehow to be connected with a verb meaning ‘take’.

One area of disagreement in this case concerns the reading. Before embarking on an interpretation the runologist must be confident s/he can recognise the runic graphs. I have not examined the Reistad stone closely myself and so can offer no firm opinion on its runes. But as a general rule, uncertainty about what is actually carved on an object should give pause for thought. The radically differing interpretations, too, ought to act as a warning signal. None is unproblematic. The difficulty with \textit{unnam} as a verb form (stressed by Antonsen, 2002, 6 f.) is that \textit{un} is a highly unlikely runic spelling of \textit{und-} at the relevant period (an assimilation /und-/ > /un:/, as envisaged by Eythórsson 1999, 191, is not to be expected so early). The understanding of \textit{wraita} as ‘writing’ suffers from an absence of corroborative evidence from later Scandinavian or other Germanic languages, while as a past tense verb form it has been criticised as too archaic. If it is a noun meaning something like ‘marked-out piece of land’, it renders the Reistad stone typologically odd by turning it into
a proclamation of ownership, for which there seem to be no parallels among the inscriptions in the older *fuþark*.

The problem here, as commonly where runic inscriptions are subject to rival interpretations, is to identify grounds for preferring one interpretation to another. Is it possible to establish some “hierarchy of doubt”? Can the lack of parallels to putative *wraita* ‘writing’ be said to weigh more heavily than the dearth of older-*fuþark* rune-stones documenting claims to land? Is it as certain as some assert that a North-West Germanic or Scandinavian strong past tense verb form would have lost its end vowel by the fifth or sixth century? And is the conviction that ‘(I) wrote’ could have taken the form /wraita/ at this period more, or less, surely grounded than the claim that /nd/ in *und-* could have assimilated to /n:/ (or that /nd/ could have been written *nj*)? On the evidence so far adduced I do not see how we are to rank these rival interpretations and assertions in terms of plausibility, and my conclusion would therefore be that the sense of the Reistad stone still eludes us. Given our tenuous grasp of language in Scandinavia in the pre-syncope period and our relative ignorance of early Germanic society, not least in the first six or seven centuries of the Christian era—with the resulting uncertainty about how rune-writers might have expressed themselves and what they might have thought it important to record—I have no high hopes that future interpretations of the Reistad inscription will prove any more persuasive.

We may wonder where this leaves runological methodology. In fact the only purely runological question in the various attempts to get to grips with Reistad concerns the reading. Here one might suggest that a group of experienced readers of runes examine the stone and try to agree on what they think to see. The interpretations, on the other hand (aside, perhaps, from the disagreement about whether *un* can be a way of writing *und*-), owe little to runology. They follow traditional philological practice, where educated guesswork—supported by parallels (or not, as the case may be)—is the order of the day.

My overall conclusion is thus, I fear, that the interpretation procedures we are looking for may prove hard or well-nigh impossible to establish. Scarcity of data will often be an insurmountable obstacle—the lack of enough information on which to base an interpretation that will satisfy the majority. One consequence of this, as Ray Page and I have both argued, should be a cultivation of the subjunctive. It will not do for runologists to plug their own interpretations to the exclusion of others of equal validity—or non-validity—in the manner of politicians seeking to convince a sceptical public that only their policies can save the nation.
I now turn to an area in which linguistic theory and runological methodology meet. My aim is to show how a well-established theory from a related discipline can inform and guide runological procedures. I first present a highly simplified sketch of the theory (for a detailed exposition, cf. Spurkland 1991).

Graphemics is a branch of linguistics that seeks to identify the contrastive units in the writing system of a language—just as phonemics aims to establish the contrastive units of a sound system. Both are identified by function. The contrastive units of a writing system are called graphemes. Like phonemes, these are an abstraction. You cannot see a grapheme any more than you can hear a phoneme. What you see are graphs, which are the individual realisations of graphemes. Take Viking-Age _estimator mark__positions_and_features_1, _estimator mark_positions_and_features_1, _estimator mark_positions_and_features_. Each of these runic characters may vary in a multitude of ways: the vertical may be of different lengths, it may be absolutely upright, slightly or notably slanting, or it may curve, for example. But if you place any variant of _estimator_mark_1, _estimator_mark_1, _estimator_mark_1 between _estimator_mark_1 and _estimator_mark_1 you have the word _estimator_mark_1 ‘this [nom./acc. n.]’. If instead you substitute _estimator_mark_1 for _estimator_mark_1, _estimator_mark_1, or _estimator_mark_1, you have _estimator_mark_1 ‘this [acc. m.]’. The characters _estimator_mark_1, _estimator_mark_1, _estimator_mark_1 with all their varieties of form can thus be classified as realisations of a single grapheme, whereas a rune with a descending right branch extending from roughly midway on the vertical is a different grapheme. Substitution tests of this kind would show that _estimator_mark_1, a rune with a descending crossing branch about midway on the vertical, is a variant of _estimator_mark_1. Variants of a grapheme are called allophones, on the model of allophones, which are variants of phonemes. The characters _estimator_mark_1, _estimator_mark_1, _estimator_mark_1 are thus allophones of a runic grapheme we can portray as _estimator_mark_1, while _estimator_mark_1, _estimator_mark_1 are allophones of _estimator_mark_1. The choice of roman transliterations to represent runic graphemes means we are able to bring a degree of abstraction to the process, which accords with the abstract status of the grapheme: we do not have to make an arbitrary selection from among the myriad of realisations of each rune. It is possible, of course, to use runes to denote runic graphemes, but they must then be marked in some way to indicate their status: we could, for example, place the chosen symbol between angular brackets, as _estimator_mark_1, _estimator_mark_1.

The difference between _estimator_mark_1, _estimator_mark_1, and _estimator_mark_1 is often considered to be of a different order from that between, say, _estimator_mark_1 and variants thereof with slanting, curved or wiggly verticals. In recognition of this, the concept “graph-type” is used. A graph-type is a variant of a grapheme clearly distinguishable from other variants by one or more features. Unlike a grapheme, however, it
is not an abstract concept: it is classified on the basis of observation, not function. This leaves a great deal of discretion to the observer, and the classification of graphs into graph-types can therefore vary according to the judgements of the investigator and the purposes for which the classification is made.

Some writing carries no linguistic message. Runic inscriptions recording partial or complete fuþarks are examples of such. These cannot be used to establish graphemic oppositions since there is no linguistic function that would make a substitution test possible. What we have in the rune-row is not a graphemic inventory, but rather the raw material from which a graphemic inventory can be built. A clear distinction has thus to be made between a runic grapheme and a fuþark unit (cf. Dyvik 1996, 13).

The upshot of these considerations is that a rune can be analysed on four different levels: as a graph (an individual realisation), a graph-type, a grapheme and a unit of the fuþark.

So much for the theory. What of its implications for runological methodology? As a general point, I would argue for a heightened awareness of precisely what we have in mind when we write about runic characters. As a concrete example, I take the matter of transliteration.

The conversion of the characters of one script into those of another is not a simple process, and there can be differing views on how it should be undertaken. It is to say the least surprising that Thompson’s paper on transliteration given to the First International Symposium on Runes and Runic Inscriptions does not consider the process at all, but is concerned solely with “the establishment of a unified system of notation for transcribing runic inscriptions” (1981, 89). When transliterating from runic to roman we ought at least to be able to agree that we cannot invoke sound value: if speech sound were to be the guiding principle the end result would be a phonetic transcription rather than a transliteration. The alternative is a system based on shape—the form of the written symbols to be transliterated. But are the roman equivalents of the runic characters to represent graphs, graph-types, graphemes or fuþark units—or some combination? Graphs, as will readily be acknowledged, are wholly unsuited to the purpose. The accuracy of observation required would be beyond the reach of mere mortals and the complexity of the roman equivalents mind-boggling—with no practical gain at the end of the process. Much the same can be said of transliteration by graph-type. Even if one could find a satisfactory basis for determining which features were typologically significant, it would be difficult to build a system of transliteration on the result that was easier to read than the runes.
themselves (cf. Barnes 2000, 148 f.). Transliteration based on graphemes presupposes a graphemic analysis, which requires a clearly identifiable system of writing. In spite of the considerable complexities involved, this does seem to offer a feasible way forward. Transliteration according to position in the fuþark also presents possibilities, but is hampered by the fact that additional medieval characters (such as dotted runes) one might want to distinguish—on the grounds that they have, or can have, contrastive function—appear not to have been regarded by rune-writers as part of the row.

Transliteration practice has up to now seldom reflected the theoretical and methodological considerations I have outlined here. Shape has been the chief determinant, though there has also been phonological input. Because transliteration serves a practical purpose, roman equivalents have been chosen that will suggest to the reader the approximate sounds different runes (however conceived) are thought to have denoted. There is no harm in this, but the tendency can be taken too far, as when ñ is transliterated now ð, now ó, or ñ now ð, now ð, depending on the phonetic value attributed to the character in different contexts (cf. respectively NyR, 3: 155; DR, Text, cols. 952, 968–71). Such a procedure involves subjective judgement; it also introduces uncertainty, because the transliteration suggests the existence of two distinct runic characters, whereas only one is in fact involved. Phonetic considerations aside, what most transliterators of runes have done is to reproduce in roman what they considered to be the distinctive units of a given system of runic writing—however they came to that conception.

Some might argue that the transliteration of runes to roman hitherto has not thrown up major problems, even if it has mostly lacked an explicit theoretical basis. There are, though, many oddities to be found. The fourth rune appears in several different guises (ฝ, ḫ, 俸, for example); it is also transliterated in different ways (a, å, o). However, although shape is for most transliterators the guiding principle, there is no correlation in this case between shape and roman equivalent; rather it is presumed phonetic value that determines whether a, å or o is used. To add to the uncertainty, 俸 may in some contexts also be transliterated b. A way around these problems lies in the positing of discrete systems of runic writing, for which different systems of transliteration are employed. Awareness of this as a possible expedient appears—as indicated above—to underlie some of the varying transliteration practices we find, though few have attempted to make explicit the processes they followed.

More serious difficulties affect the transliteration of the additional

Futhark 4 (2013)
characters of medieval Scandinavian runic writing. Traditionally, for example, ð has been transliterated y, ð, e. As a purely practical device this is perhaps acceptable: when we come across e, we know that the runic character concerned is a dotted I, and so on. However, the impression can easily be gained that e is an “e-rune”, whereas in reality I (in the tenth and eleventh century, at least) is a marked form of I, which may denote [e:], but can also stand for other sounds, notably [æ(ː)], and sometimes even [i(ː)] (cf. Lagman 1990, 78). There are two problems here, a practical and a theoretical. The practical problem is: how does one transliterate dotted characters in a way that alerts the reader to their status? Before a solution can be proposed, however, that status must first be elucidated, which is a theoretical problem.

Dotted runic characters can hardly be classified as futhark units, because they are very seldom included in rune-row inscriptions, and when they are, appear to be randomly selected and ordered. They might be considered graph-types—clearly recognisable variants of particular graphemes. Yet the pair I/I, for example, differ from the trio I, I, I in that I often has a different function from I. This suggests graphemic status, but the fact that I and I are not always clearly distinguished by function renders that a slightly tricky analysis. In the Middle Ages, where I is very often used to denote [e:], and I does not normally denote other sounds, it is perhaps unproblematic to analyse it as a grapheme. But in the late Viking Age, where dotting seems to be used “för att markera ett ljudvärde som ristarna fann det angeläget att markera i förhållande till andra ljud som den ostungna runan kunde stå för” (‘to mark a sound value which the carvers considered it important to mark relative to other sounds which the undotted rune could stand for’; Lagman 1990, 153), the graphemic status of I and other dotted runes is less clear. At what point does a variant marked for an indeterminate function turn into a grapheme?

Whatever else, these theoretical deliberations make clear that I in its initial incarnation is not an “e-rune”, nor is ð a “y-rune” or ð a “g-rune”. A soundly based system of transliteration, it seems to me, should try to capture what it can of the essence of the dotted runes (and the other additional characters of medieval Scandinavian runic writing). In The Runic Inscriptions of Viking Age Dublin (1997) and The Scandinavian Runic Inscriptions of Britain (2006) my co-author(s) and I sought to put this desideratum into practice by making the transliterations of dotted runes approximate as closely as possible to the runic characters themselves. To render I, for example, we applied diacritic dots to roman i, giving I—a variant of i distinguished by dotting. Dots above consonant
letters present typographical obstacles, but these can be overcome. An alternative is to use small capitals, thus i for í, but that moves the transliteration equivalent further from actual runic practice. From a more purely theoretical perspective, we reasoned, í for Í takes account of the fact that the character is neither a free variant of i (if it were we would transliterate it i) nor—in its early life, at least—an independent grapheme (in which case some other letter than i would be appropriate), but rather a variant—dotted to distinguish it for one purpose or another from its undotted counterpart. Here, it seems to me, consideration of the theoretical basis of transliteration has pointed the way to a principled and practical solution.

In this brief exposition I have only been able to examine the basics of what is a fairly complex topic. The issues are given much fuller discussion in Barnes (2011b).

Further considerations

The critical examinations of the state of runology enumerated at the beginning of this paper deal with a number of different issues. Certain questions recur, while others are raised more sparingly. Peterson’s 1996 article is the most wide-ranging of the contributions, and I have found it helpful to structure my remaining comments on what I consider the more important of the points she makes.

Peterson begins by asking what runology is and offering various answers. That is a question I have discussed at some length already. She moves on to theory and methodology, matters to which I have also devoted considerable attention. Yet there are additional methodological questions raised by her and others that deserve scrutiny. I consider first approaches to interpretation as envisaged by Düwel (2004) and Lerche Nielsen (1997).

Düwel’s contribution appeared in a volume entitled Early Germanic Literature and Culture. As befits such a work, the perspective from which he views runes and runic writing is almost exclusively that of the older fuþark. Although something is said of the essential features of early runic writing, the emphasis is very much on problems encountered in the reading and interpretation of individual older-fuþark inscriptions. By way of illustration widely differing interpretations of the Tune and Eggja stones (KJ 72, KJ 101) are presented, and reasons for the differences of understanding analysed. Apart from disagreement on readings and the expansion of lacunae, there are the “varied assumptions and presuppositions” scholars bring to the task (2004, 130). Düwel shows in
considerable detail how these have affected the interpretation of Eggja, and sums up as follows (2004, 131): “Thus the enigma of the Eggja inscription, despite all the efforts of leading runologists ... is still unsolved and will probably remain so.” This conclusion surely rather misses the point. It is not because of the “varied assumptions and presuppositions” that no interpretation of Eggja has so far satisfied the scholarly community; rather it is because there is no agreed way of measuring the validity of competing interpretations. And this stems from the extreme paucity of linguistic and cultural data from Scandinavia in the period concerned. In such circumstances a Handbook of Runology might require a measure of self-criticism and restraint from would-be interpreters—beginning with a frank admission of any preconceived views with which they approach an inscription, backed up by a pledge to keep such views in check or at least to acknowledge freely the part they play in the exposition. That way non-runologists would have a better understanding of the degree of trust they could place in individual interpretations.

The approach suggested here is not far removed from that espoused by Lerche Nielsen (1997), who uses the Viking-Age Glavendrup stone (DR 209) to show what a dispassionate examination of an enigmatic inscription might look like. First he castigates a number of procedures that in his view have brought runology into disrepute: recourse to textual emendation, particularly the assumption of omissions and abbreviations that have no obvious parallels; reliance on reconstructed forms of otherwise unknown words, or on undocumented semantic developments of recorded words; interpretations based on cult or magical practices for which we otherwise have no evidence. Turning to the Glavendrup inscription Lerche Nielsen points out what he considers to be the advantages and disadvantages of earlier interpretations, but refrains from promoting any particular view himself because he finds the evidence insufficiently persuasive. He warns against basing conclusions on too sparse a knowledge of other disciplines and against allowing academic fashion to influence interpretations, citing the rush to embrace “magic” in the inter-war years, and the sudden discovery of “power structures” in more recent times. If future interpreters of runic inscriptions would follow Lerche Nielsen’s practice, scholars in other disciplines—and the general public too—would at the very least have a clearer idea of what was reasonably well established and what mere conjecture.

As an example of “en fin demonstration av metod” (‘an excellent demonstration of method’) Peterson cites Lagman’s 1989 article on regularities and irregularities in the orthography of Swedish Viking-Age memorial
inscriptions. His piece concludes with a “tolkningsschema”—recommendations for the step-by-step interpretation of inscriptions. This is a thoughtful and innovative contribution. Its impact is lessened, however, by a weakness in the basic premise on which it is founded. Lagman holds that the most natural spelling rule for rune-carvers was: “Skriv som det låter!” (‘Write as it sounds!’; 1989, 28). But as Dyvik points out (1996, 20 f.), experience in trying to establish alphabetic writing systems for hitherto unwritten languages has shown what a demanding and complicated task speech-sound analysis is, even of one’s native language. Dyvik thinks, in contrast to Lagman, that the regularities we find in runic orthography have to do with a written tradition passed on from teacher to pupil. He invites comparison with the first attempts at writing by a five-year-old who has just mastered the alphabet, or the orthography of a dyslexic: in comparison runic writing appears far too “velordnet” (‘well organised’) to be the product of spontaneous sound analysis. I am in full agreement with Dyvik, and have myself argued that those learning to write with runes tried to memorise a particular spelling of the more common words—often without complete success, as attested by the considerable orthographic variety encountered. However, while I do not have complete faith in Lagman’s methodology, for the reasons here given, I applaud his attempt to get to grips with a fundamental question in runology. It is by advancing hypotheses and testing them that we take the subject forward.

Antonsen, it could be argued, has performed a similar service. He has urged a “rigorous linguistic approach” both to the history of runic writing and the interpretation of inscriptions. This has led him to the view that the runes are much older than most have thought and that the language of the inscriptions in the older futhark is closer to Common Germanic than to later Scandinavian (cf., e.g., 2002, 3–13, 93–117). He gets short shrift from Peterson, however (1996, 44). She praises his stubborn insistence that the runes are linguistic, not magic, symbols, but otherwise dismisses him as a “desk runologist” on the grounds that many of his linguistic arguments are based on idiosyncratic readings: “Han ser—eller låtsas se?—runor som ingen annan kan se” (‘He sees—or affects to see—runes that no one else can see’). Düwel, too, finds Antonsen’s approach rather far removed from reality. “The impression sometimes arises”, he writes, “that linguistic analyses strain the linguistic record and become an artistic game” (2004, 137). It is clear the scholar he has chiefly in his sights is Antonsen.

Linguistic argumentation leads to improbability when the phonemic system that the oldest futhark inscriptions are based upon is traced back to the middle
of the first millennium B.C., thereby establishing the origin of runic writing in a pre-classical Greek alphabet.

For my part I would suggest that both Peterson and Düwel dismiss Antonsen too summarily. To establish the plausibility or otherwise of his new readings a group of experienced field runologists should examine the inscriptions concerned and report on their findings—in the way I have already suggested. The case that leads Antonsen (and his pupil Morris—cf. Morris 1988) to move the origin of runic writing back to near the middle of the first millennium B.C. is cogently argued and by no means without interest. It founders not because of its artificiality but because of the dearth of runic inscriptions between the supposed period of origin and A.D. c. 160, and the lack of a trail of such inscriptions leading from the Mediterranean northwards towards Scandinavia. My complaint against Antonsen and Morris is rather that they have been unwilling to engage with the reasonable criticisms their proposal about the high age of the fuþark has attracted. Antonsen regularly substituted confident assertion for reasoned argument in an area where there is little we can be confident about. Indeed, over-confidence characterised much of his scholarship. Not only did he urge his own views to the exclusion of all others, he seemed at times to elevate subjectivity to a virtue. In his examination of the “science” of runology, he goes so far as to claim that the minds of those who do not see what he sees are “out of focus” (1995, 127; a revised version of this paper is Antonsen 2002, 1–15). He comments on:

the problem that has faced so many a runologist trying to read and interpret the oldest runic inscriptions … WE CANNOT ALWAYS DEPEND ON THE EYES OF THESE INVESTIGATORS, BECAUSE THEIR MINDS WERE NOT IN PROPER FOCUS. They often could not recognize what was actually before their eyes, or correctly identify and emend errors in the text.

This assertion prefaces a restatement of the view that the language of the oldest runic inscriptions is much closer to Proto-Germanic than it is to Old Norse and a claim that the minds of scholars who failed to recognise that “basic fact” were not focused on “the proper language”. Over fifty years previously Gustav Indrebø had opined of the oldest runic inscriptions as linguistic remnants: “Dei kann ikkje stå so sers langt frå samgermansk” (“They cannot be so very far removed from Common Germanic”, 1951, 41), and there is much to suggest he and Antonsen are right about their linguistic status. But the way to show that is through the deploying of evidence. The bald claim: “I can see things others cannot see” is unlikely to
convince anyone with a modicum of critical sense. As a general conclusion and recommendation in this instance I can do no better than reiterate one of the suggestions I made in my paper “On Types of Argumentation in Runic Studies” (1994, 26).

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?”

In other words, we must not become missionaries trying to convert others to our point of view. We must learn to sift and weigh evidence dispassionately.

Runes and magic is a subject Peterson touches on only briefly, and I will follow her example. Serious runologists have, I think, abandoned the epigraphical maxim “that whatever cannot be readily understood must be sorcery” (Page 1999, 100), although in a highly speculative section of his paper to the Fourth International Symposium on Runes and Runic Inscriptions in Göttingen Braunmüller appears to be making valiant efforts to reinstate the general principle (1998, 7–13). Crucial in the context of runes and magic is the distinction between runes as magical symbols in themselves and their use to write incantations. Clearly, any script can be used in an attempt to invoke the supernatural. To determine whether a given runic inscription has that aim, one needs a definition of magic as well as close study of the object bearing the inscription and its likely context. It is quite a different proposition to claim that people believed the mere employment of runes was enough to harness magic powers. The evidence for the existence of such belief among rune-carvers is in my view slight.

Page (1999, 106–16) surveys the field. Having discussed the etymology of Old English run and cognates in other Germanic languages, and possible “magical” contexts of certain inscriptions, he concludes: “it is the literary evidence that bears the burden of the theory of rune magic.” This evidence comes chiefly from medieval Iceland, a society clearly familiar with traditions that associated runes and supernatural powers—not, however, a society much given to writing in runes, to judge from the paucity of inscriptions it produced and their largely post-1200 date. Indeed, some have considered runic writing in Iceland to have been inspired by a renaissance of the art in medieval Norway (Bæksted 1942, 20). It is not hard to imagine that people with little or no everyday experience of runic script could come to view it as esoteric and shrouded in mystery. On the other hand, it seems improbable in the extreme that
the inhabitants of medieval Scandinavian towns such as Bergen could have shared the Icelanders’ conceptions. I would thus find it problematic to join Braunmüller (1998, 8) in citing the procedure described in Egils saga ch. 44 of inscribing runes on a drinking horn containing a poisonous concoction and smearing them with blood (whereupon the horn burst asunder) as evidence of actual practice among rune-carvers.

With the association of runes and magic apparently still widespread in many circles it is important that a Handbook of Runology enable students to approach the topic without preconceptions. The evidential basis for runes as magical symbols, and for their use in writing spells, should be laid out as clearly and dispassionately as possible.

The impingement of national or political considerations on runic studies is arguably no longer a serious problem. Peterson, I note, does not address the matter in her examination of runology. Debate about whether the fifteenth rune of the older fuþark, Ý, should be transliterated R or Z now concerns the relative weight to be attached to (assumed) phonetic as opposed to phonemic status, where previously it centred on R as evidence for the existence of a specifically Scandinavian linguistic entity before the transitional period (Antonsen 2002, 73–91). The perversion of runic studies in the Third Reich has been thoroughly documented in all its absurdity (Hunger 1984), and the use of runes by neo-Nazi organisations can likewise easily be recognised for the nonsense it is. Erik Moltke’s belief that all important innovations in runic writing took place in Denmark may now safely be ascribed to an excess of patriotic zeal. Yet political ghosts still seem on occasion to haunt runic research. I have at least the suspicion that Braunmüller’s dismissal of significant Germanic input into runic writing stems in part from a desire to exorcise the demons of the past (1998, 17 f.).

**Conclusion**

Peterson (1996) draws attention to several encouraging developments in runic research. I concur in her positive assessment, but will refrain from singling out specific trends, works or scholars. My aim here has rather been to raise questions about runology, specifically about how it might be defined and how most efficiently and rewardingly pursued in the future. I have drawn a number of conclusions along the way, and I do not want to invite the charge of repetitiousness by rehearsing them here. I will, however, offer a few final observations by way of summary.
1. A meaningful definition of runology must, I think, be narrow. If it is to include archaeology, mythology and all kinds of history there might as well be no definition at all.

2. Runological theory can only come from the application to specific runological problems of theories from other disciplines. It is hard to identify the basis on which a purely runological theory could be built.

3. In certain areas there is scope for the elaboration of runological methodology, but probably not in all. The interpretation of inscriptions is a case in point. Lack of knowledge hampers our understanding of many inscriptions and it seems unlikely a general interpretation procedure could be devised that all or most runologists would accept.

4. Whatever methods and procedures we adopt, we must strive for transparency. Everything must be laid bare so the reader can follow our deductive processes and thus gain a fuller understanding of how our conclusions have been reached.

5. Transparency requires the use of plain language: we must avoid as far as possible expressing ourselves “in the rather forbidding format that specialists adopt to convince their colleagues of their scholarship” (Page 1993, 155).

6. We must be willing to give up attempts to interpret inscriptions where the evidence is insufficient (cf. Seim 1991, 84 f.). A charge of defeatism may well be levelled or the cry go up: “the reader has a right to know what you believe!” But if you believe you do not know what an inscription means because the evidence is too slight, the honest thing is to say so.

Bibliography


DR + number = inscription published in *Danmarks runeindskrifter*, i.e. DR.

*DR = Danmarks runeindskrifter*. By Lis Jacobsen and Erik Moltke. 3 vols.: *Text; Atlas; Registre*. København 1941–42.


*Futhark* 4 (2013)


NlyR = Norges innskrifter med de yngre runer. By Magnus Olsen et al. 6 vols. to date. Oslo 1941–.


—. 1999 An Introduction to English Runes. 2nd ed. Woodbridge.

Futhark 4 (2013)


