Work Songs and Whetstones

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In his review of MacLeod and Mees (2006), the late Elmer Antonsen (2008: 116) dismissively comments on our epigraphic approach "whatever that may be". Antonsen was often criticised for the idiosyncratic nature of his contributions to runic studies, but he was one of the few runologists ever to attempt to explain his interpretational method. The Illinois linguist does not seem to have been alone, however, in his unwillingness to accept that there might be ways of doing runology other than that which he found prudent.

Of course Antonsen was not alone in mistaking bludgeon for persuasion. But it seems to me that just a little awareness of concepts such as Max Weber's (1947: 112-18) fourfold theory of social action or the neo-idealist notion of historical imagination might lead to the emergence of a more theoretically nuanced runology. Surely the intrusions of the stranger Germanophile scholarship of the 1960s and 70s did not require methodological overreaction. Indeed Ray Page's (1999: 12) distinction of imaginative from sceptical runology seems quite untoward when taken in light of the work of the leading English historical philosopher (and epigrapher) R. G. Collingwood (1946). To understand the culture of a pre-modern, traditional society, one might well have some need of historical imagination. Contemporary runology is dominated by what might be called neo-empiricists – i.e. many of the proponents of modern runic studies seem not have moved on in a methodological sense from the nineteenth-century critical tradition usually held to be epitomised by Leopold von Ranke (and encapsulated in Ranke's (1824: vii) dictum of "Geschichte wie es eigentlich gewesen ist"). Yet those wedded only to a rationalism of "common sense" are unlikely to be able to respond meaningfully to intellectual work informed by a broader theoretical canvas (cf. Morley 2004: 1-31).

Scholarly critics of calls upon "common sense" have long maintained that the notion is constructed and ideological (cf. Ichheiser 1949: 39). I am sure that Karl Hauck was convinced that he was a particularly commonsensical scholar, for instance, but it is not too difficult to see where he often fell down. Hauck had little sense of historicity – it did not seem to trouble him that his interpretations of the C-type bracteates involved a projection of a tenth-century source onto expressions from late antiquity (nor that similar medallion-based decoration – replete with typical Roman texts – appears on a range of Merovingian brooches); see Hauck (1970), Klein-Pfeuffer (1993: 173-79) and Düwel and Heizmann (2009) for a recent apology. This is not quite the same as assuming that Shakespeare ate at McDonalds, but seems a remarkable form of historical anachronism nonetheless. Indeed many runologists have been guilty in the past of similar anachronistic projections, a pro! blem of which Antonsen was aware, even if he never appeared to recognise the full ramifications that such a critique might entail.

Antonsen was particularly critical of philological projections from Old Norse onto the early runic texts (Antonsen 2002: 3-13). But in seeking to establish a method of controlling such practices, Antonsen (2002: 207-36) merely spoke broadly of a typological approach to runology, often without applying it properly himself. He was also evidently quite unapprised of the notion that runology could be seen not just as an epigraphic part of Old Germanic studies, but also as an Old Germanic expression of broader epigraphy. Yet his ignorance of other epigraphic traditions is shared by many who view runology mostly from the perspective of medieval studies today. Indeed the tendency to "medievalise" much earlier inscriptions has long also been a scourge in Old Celtic studies too.

One of the main arguments of my most recent book (Mees 2009) is that a comparative epigraphic perspective is required to control the etymological imagination. This means checking etymological possibility whenever possible against broader context – in the case of Old Celtic curse tablets, with similar texts of Greek and Latin origin. Old Celtic studies has also suffered substantially from what Page (1999: 10) has characterised as the "first law of runic studies": i.e. that every scholar who publishes an interpretation of an inscription will tend to offer a different understanding. In the case of the Old Celtic inscriptions, this interpretational over-population was often concomitant with ignorance of comparable Greek and Latin finds. Experiencing what was going on in Celtic studies made me realise that a comparable problem

afflicts runology.

Methodologically, Antonsen (2002: 1-3) claimed to follow Carl Marstrander (1929: 164), who had declared that runology was a mixture of palaeography, linguistics, archaeology and mythology. Runologists rarely study early handwriting (palaeography proper), however; Marstrander's palaeographic understanding of runology (instead) betrays how disconnected the field has long proven to have been from other forms of epigraphy. The notion that runic studies is a branch of palaeography has long tainted academic runology, and in effect betrays its status as a specialised form of Old Germanic philological endeavour rather than an independent, developed and properly theorised field of inscriptional study.

The primary focus of scholars like Marstrander was on decipherment – on analysing runic orthography, explaining the (basic) linguistic facts revealed and contextualising them in terms of archaeology (and, for Marstrander, also mythology). Most of the runic handbooks which grace library shelves are not written by linguists, however, but instead by experts in early European literature and culture. Indeed few such scholars consider themselves to be epigraphers – instead most have proven themselves uninterested in broader epigraphic matters. As a result, their works not only fail to accord with widely accepted epigraphic practices such as use of the Leiden editorial conventions (van Groningen 1932), but are also often strangely silent on key theoretical and (often) even methodological concerns.

One aspect of epigraphy that runologists usually get right (Antonsen being a noted exception), however, is a pronounced sense of the value of personal inspection ("field runology"); cf. Moltke (1981). Seeing a text at first hand should not be necessary if one can rely on another set of expert eyes, but studying inscriptions solely from photographs has long been regarded as a no-no in most fields of epigraphy (cf. Merrit 1940: 21). Autopsy should ideally be matched by physically visiting the site of discovery as well as the museum collections which hold items found in similar surrounds. Much as Eduard Norden (1920: iv) described walking the Roman *limes* as an experience essential to his development as a philologist, I have always found autopsy and availing oneself of broader museological context epigraphically revealing, whether Germanic, Celtic, Greek, Roman or Archaic Italic. Visiting the archaeological museums of Northern Italy, Switzerland and Austria proved invaluable in developing my understanding of North Etruscan epigraphy, for example. What should they know of England who only England know.

Linked to autopsy is transcription – i.e. transferring what textual information is thought to be preserved on an epigraph into what palaeographers refer to as a diplomatic form. Whether the transcription is in runic text or is a transliteration into a Romanised form, judgements are often made at this point concerning letterforms, direction of writing, textual continuity and punctuation that ideally should be noted and applied in as transparent and minimalist a form as possible. Runologists are often stronger in their (neo-empirical) sense of faithfulness to extant text at this stage of epigraphic interpretation than are scholars who work in other inscriptional fields – and it is here that the practice of runology seems closest to traditional palaeography.

After autopsy and transcription, though, comes the assessment of archaeological (and if available historical) context: particularly in terms of utility and representivity. If other items of a similar type are sometimes found inscribed, then the competent epigrapher should want to know this. Similarly, if the inscribed item is an object of a type well known to archaeologists (or even historians), then expert interpretations of its use and function can confidently be referred to. Unfortunately, archaeology is not an exact science, and the rarer the type of find, the more likely that the judgement the epigrapher experiences in archaeological (and historical) literature will not be sound. The practice and scope of archaeological investigation has improved greatly over the course of the last 50 years, but runologists still need to recognise the many limitations characteristic of the field.

Antonsen was an expert in the next aspect of epigraphic analysis: studying the phonology, morphology and etymological semantics of a particular inscription – what might be called the "lexicalisation" of a text. Antonsen also added "palaeography" to his method, arguing that the study of letterforms was part-and-parcel of the linguist's job. Often it is not, however, and it was one of Antonsen's more criticised failings that he frequently showed disregard for widely accepted readings by scholars who had actually seen the relevant inscriptions. I myself have succumbed at least once to trying to reread a text (following Antonsen) in light of what I

imagined orthographically and linguistically "should" have been there (Mees 1997, *pace* the obduracy of Antonsen 2008: 118). Yet texts are primary evidence in epigraphy. Tweaking attested forms is a tempting practice, but not one to be broadly condoned (cf. Fjellhammer Seim 2003).

It is quite often the case, however, particularly with early runic texts, that it is not entirely clear whether a reading or spelling can necessarily be trusted. Scholars such as Latinists have long since realised that the unexpected spellings characteristic of Roman epigraphy often have little linguistic value and it can often be a case of overstating the worth of runic evidence when complex arguments are based upon data that is so fragile and (often) so partial. It is quite often the case that words take on novel (even ephemeral) meanings, too, when they appear in particular collocations, discourses or text types. Sometimes (as in the case of the Tune stone's **arbija**) it seems much wiser simply to accept that we simply do not know whether an etymologically inherited meaning (i.e. 'inheritance' at Tune) is more fitting to apply to an early text than a newly developed Old Norse one (i.e. 'funeral feast'), no matter the apparent firmness of the judgements one might find represented in the relevant literature (cf. Grønvik 1981: 176-84 and 1982 to Antonsen 2002: 129-30).

Once a text is parsed and likely phonological shapes of words have been determined, however, is where the fourth and most difficult part of epigraphic investigation begins: what I call "epigraphic pragmatics". This is where the sociological notion of "tradition" is most important – i.e. harbouring a healthy understanding of how alike many inscriptions are to each other. This inscriptional sameness or formulism witnessed in most epigraphic traditions can be explained by the notion of intertextuality. Any runic text must primarily be seen as an expression of broader discourse – and as Julia Kristeva (1980: 36-38) argued for literary texts, inscription entails a transformation of spoken or imagined discourse into material form (cf. Ricœur 1976: 26-28 and 42). The intertextual choices made by the inscriber are the characteristic features of epigraphic pragmatics – of understanding the methods and choices which are intrinsic to the act of inscription. And in a t! raditional society one expects inscriptions to be produced for traditional purposes, not ad hoc or (individually predicated) "rational" ones. So if there are no clear examples of "symbols of office" among contemporary finds (Antonsen 2002: 232-36) – i.e. there is no evidence for a traditional text type or genre of this kind – then such an interpretation for a difficult inscription is probably wrong. Similarly, certain stylistic forms (from alliteration to merism, topicalisation to rhythm) are often found in epigraphy – and isolating such behaviours can be of particular help when one is seeking to garner some sense of control over the etymological imagination. Matters of syntax, semantics and pragmatics are also rarely assessed in particularly adroit manners by runologists – and this failure to engage with the part of linguistics which is often characterised as "natural language" today hardly does much credit to the standing of runic studies as a field.

When assessing the Strøm whetstone in our book (KJ 50, MacLeod and Mees 2006: 77), the publisher's word limit did not allow us the space to formally dispute the assumptions that Antonsen (1975a: no. 45, 1975b, 2002: 155-61) brought to his study of the inscription. Evidently it is important to understand that whetstones of the Strøm type were often carried in hollowed-out animal horns until a comparatively recent date and lubricated while so carried with water, oil or wine (cf. Tresemer 1981: 28). The Strøm whetstone is clearly of a different physical type (its inscription too) to the younger Timans and Sigtuna finds (Jansson 1987: 90, MacLeod and Mees 2006: 226-27), so we (or rather I) instead compared it to another form of Old Germanic sharpener: the early runic Vimose plane (KJ 25). The Vimose plane was probably used to sharpen spears (Christensen 2005: 75), the Strøm whetstone either agricultural tools or swords (whetstones often being found along with swor! ds in warrior graves). The Strøm inscription was not found in a clear archaeological context (other than a charcoal-filled cairn), and no broad studies of early whetstone typologies seem to be available as yet (cf. Steuer 2007). But even if the Vimose inscription is quite difficult to make out in parts, it bears some clear parallels to the text on the Strøm find.

The runes on the Vimose plane are transcribed by Moltke (1985: 89) with several words made quite clear. The first line's **talijo** can be read either as a (weak) verb (or verbal noun) *taljō* 'count(ing)' or a miswritten object description *tal(g)ijō* 'wood-plane', and the sequence **wiliz** seems most likely to represent a 2nd sg. subjunctive *wilīz* 'you may want'. Two sequences beginning with **hl**- can also be made out (presumably references to **hleu-/hlau-*

'protection'), seemingly evidence of a heroic (or military) *figura etymologica* – and a final verb **regu** 'I counsel' seems clear enough, too, given the evidence of Noleby **ragina**- 'gods' (literally 'counsellors') and sundry related later manuscript forms such as Goth. *garehsns* 'definite time, plan, determination', *rahnjan* 'reckon' and OE *regnian* 'decide, arrange' (Antonsen 1975a: no. 10, MacLeod and Mees 2006: 76-77, Markey forthcoming 2010). Hence our (MacLeod and Mees 2006) inclusion of the plane under the rubric "protecting and enabling".

Yet such a perspective was ignored by Antonsen, even though the Strøm inscription features both clear evidence of alliteration and 2nd sg. imperative forms. Instead Antonsen seemed especially (albeit tacitly) taken by Krause's comparison of the inscription with passages often taken as literary forms of work songs from *Gróttasöngr* and *Darraðaljód*. The relevant "songs" are written with plural voices (*mölum, vindum*), however, as is typical of old work songs (which were usually sung by groups of workers; see the classic study of Bücher 1919). This is clearly not the case at Strøm (which is clearly written in the singular) – Krause's suggestion was little more than a guess. Yet Antonsen allowed this medievalistic perspective to dominate his reading of the relevant Strøm forms.

What is usually taken as the opening line of the Strøm inscription was largely already interpretable to scholars such as Olsen (1908, *NIæR* 52) well before Antonsen first wrote on the find. Antonsen's main contribution here was to attempt to explain the verb in **watehalihinohorna** (following Von Grienberger 1910: 390) as a 2nd sg. class-III weak imperative (rather than 3rd sg. subjunctive): 'wet this stone, horn!' (cf. Goth *habai*, Lat. *habēre*). He might have been better off by accepting that **wate**, like ON *væta*, is a denominative class-I verb (cf. ON *vátr*, OE *wæt* 'wet', OE *wæter*, OS *watar* 'water'), however, and that as a long-stem form (< **wētije*), its (unstressed) desinence could be spelled differently than that of a comparable short-stemmed verb (i.e. with *-i* < **-je*); cf. Goth. *sōkei*, *sōkeiþ* to *nasei* (for expected **nasi*), *nasiþ* and the attested spellings -**de**, -**dai** and -**da** (presumably all for *-dǽ*) for the 3rd sg. dental preterite desinence in early inscriptions. The 'wetting' (**wate**) and the 'horn' (**horna**) clearly relate to the context of carrying the whetstone in a horn filled with some lubricating water. But proceeding from the alliterating expression which describes the function of the whetstone's lost water-filled carrying horn to positing that the text records a work song is quite a leap, and not at all an obvious one.

Antonsen compared the Strøm form **horna** with the similar term on the Gallehus horn, **hali** with the *a*-stem variant of **hall*- 'rock, stone' (cf. Goth. *hallus* 'rock', ON *hella* 'flat stone', Lat. *collis* 'hill') found at Stenstad (KJ 43 and 81, Antonsen 1975a: nos 23 and 37). It is a pity that he stopped there, however, as such a typological approach would necessarily have meant a quite different interpretation of the line on the other side of the Strøm find. Rather than comparing **haha** to the form **hahai** on the Möjbro stone (which he read, with Krause, as a dative '(on a) steed') and **haþu** to the like onomastic element attested at Blekinge (KJ 95-98, Antonsen 1975a: nos 11 and 116-20), Antonsen instead invented nominal interpretations (again, much as had many of his predecessors) which are not directly supported otherwise in Germanic for his assessment of what is usually taken as the second line of the early runic text.

Evidently convinced on *a priori* grounds that the inscription was a work song, Antonsen proposed suitably agricultural meanings for **haha** and **haþu**, the objects of **skaþi** 'damage, scythe!' and **ligi** 'lie!' (the former form now paralleled onomastically as 'damage(r)' on the Wremen stool; see Schön et al. 2006). Reading **haha** and **haþu** otherwise – i.e. in line with the best-paralleled Germanic (and early runic) forms – would not have been commensurate with a work-song reading; hence Antonsen relinquished his typological method when assessing the second line of the Strøm inscription, conjuring up new terms and meanings from his etymological imagination instead. Now the inscription was a work song referring to a sickle and to fallen crops, not to a (damaging) steed and to battle. Yet reading either nominal term as it is most obviously to be from a typological or intertextual perspective brings the Strøm inscription not just into line semantically with the text on the Vimose plane (and indeed culturally into accord with most of the other early runic finds), it makes it quite clear that the Strøm inscription does not record a work song.

Krause's work-song interpretation seems to have become an *idée fixe* for Antonsen, one that led him to ignore the very method that he proposed runologists should properly follow: i.e. to reject the notion that the early runic inscriptions should be read primarily from a

medieval perspective. Reading the Strøm inscription principally in light of its early runic context instead suggests that the whetstone was a military item – a sharpener of swords – and that its interpretively most difficult line represents a reference to damage and to battle. Whether or not one accepts **haha** as 'steed' (surely more acceptable than Antonsen's phonologically irregular comparison with Goth. *hoha* 'plough', OE *hōc* 'hook' etc.), Olsen's reading of **haþu** as 'battle' is too obvious to be blithely discounted: the semantic 'lie' (as opposed to 'lay, cause to lie') seems to indicate a state or a situation; hence Aag (1980) followed Olsen in assuming a (prepositionless) dative (Aag of place, Olsen of means): 'lie (fallen/peacefully) in battle!'. Given the *u*-stem dative *-ku(n)diu* preserved on the Tjurkö bracteate (KJ 136), we might prefer to read an accusative (perhaps of reference or situation rather than direct object). But as Kiil (1953) has stressed, the inscription is connected with a burial cairn, and attested Old Norse usages of *liggja* include 'be buried' as well as 'lay anchor' and the like (and cf. *leg* 'burial place, position, lie (of a region)'). Indeed the owner of the whetstone might well have been considered a 'steed of battle'; cf. *Knútsdrápa* 4 which allusively associates *Hǫðr* (< **haþu*) with *hein-* 'whetstone' in a complex military kenning (Jesch 2000: 245-48). Whetstones are often connected with warfare in Old Norse tradition (cf. even the kenning *beðr ryðfjónar* for 'sword', Anon (*ÓT*) 6[1]) and it seems quite perverse (with Antonsen 1975a: no. 45) to appeal to Sanskrit *śātayati* 'cause to fall down, crush, break to pieces' to argue for an agricultural association for **haþu**, this most military of Old Germanic terms when Blekinge is so much chronologically and geographically (not to mention orthographically and culturally) nearer at hand. Antonsen rejected Von Grienberger's (1910: 389) association of **haþu** with a putative **hawiþu* 'haymaking' as phonologically anachronistic, but followed the Austrian's agricultural semantic assumption nonetheless; cf. Schulte (1998: 98), however, for a recent linguistic defence of Von Grienberger's reading and Grønvik's (1996: 136-54) invocation of the Old Norse adjective *háðuligr* 'scornful' (< **hawiþa-līkaz*) for a quite different (and typologically unparalleled), phonologically "late" (and hence unlikely) interpretation.

Given the early runic attestations of both **skaþi** and **haþu** in dithematic names (at Wremen and Blekinge), it might be tempting to assume a similar onomastic reading for **hahaskaþi** and **haþuligi** (after all, the medieval descendant of *haþu-* is only attested in Old Norse names). Yet the alliteration, trochaic rhythm and rhyme suggest that Olsen's reading of verbs in these forms has much more to commend it – and despite some assertions to the contrary, all the readings suggested here can be accommodated with the form of early Nordic that existed during the bracteate period. The shortenings in the desinences of the imperatives are just as regular as that seen in **hari**- < **harja*-, and reading **haþu** as it is most obviously to be interpreted obviates the need to posit a strikingly early medial shortening in putative *hāþu < *hawiþu*. Despite the lack of a (non-onomastic) descendant of *haþu-*, collocations of words for 'battle' and *liggja* are also fairly frequent in Old Norse (e.g. Hfr III, 6: *gunn- ... lá*), much as the animal + **skaþi** name attested at Wremen suggests that horses, too, could be associated with **skaþjan*. The best reading of the Strøm inscription, based on the principles described above, would consequently seem to be:

wātī hal(l)i hinō horna!	'Wet this (whet)stone, horn!
hāha skaþi! haþu ligi!	Scathe, steed! (In/Let) battle lie!'

And these forms presumably correspond to a putative Proto-Germanic:

wētije hallin hinōn hurnan!
hanhan skaþje! haþun legje!

Whetstone charms are a recognised genre in medieval experience (there are at least two mentioned in Icelandic manuscripts; see Flowers 1989: 85 and 101), but much like Antonsen's "symbols of office", an epigraphically attested work song is quite unknown to me – runic, Roman or otherwise. Marvellous whetstones feature in Old Norse tales and Bücher (1919: nos 216-17) records two work songs from Georgia that mention the sharpening of sickles. But it is not clear whether, e.g., Odin's (agricultural) whetstone that appears in *Skáld-*

skapamál is clearly to be linked with the Strøm find (Spurkland 2005: 31). The alliterating, trochaic nature of the Strøm inscription might be equally consistent with a work song or a charm, perhaps even with a (heroic) literary text. Yet it is above all the vocabulary and medium of inscription that links the Strøm lines to those on the Vimose plane, a more-or-less contemporary (i.e. dating from the early runic period) sharpening tool which also seems to feature a text that "talks" (i.e. in the 2nd sg. subjunctive/imperative) to the reader, much as if it represents some sort of military charm (cf. Fischer 2005 on the military and elite nature of most early runic inscriptions). The work-song interpretation of the Strøm inscription appears to represent much of what is methodologically unwarranted in runic studies, a field to which Antonsen otherwise made many important contributions. His bluster and bluntness aside, Antonsen seems only to have made half of the jo! urney towards establishing a runological methodology tightly focused on structural linguistic and intertextual concerns, and above all one free from the (often) pernicious influence of medievalistic anachronism.

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