The first person to draw my attention seriously to the statistical difficulties that face the runologist was Prof. René Derolez, at the opening runic conference of this series, held in Ann Arbor, Michigan. In a paper on ‘The runic system and its cultural context’ he remarked (Derolez 1981:19–20):

If we consider the surviving inscriptions in the older futhark as a whole, especially those of the first three centuries of the runes’ visible existence, we cannot help being impressed by the following facts:

1. The total number of inscriptions down to the year 450 or so amounts to no more than between 10 and 20 in a century, or one in every five to ten years;
2. Those inscriptions are spread over a fairly wide area comprising large parts of Denmark, Norway, and Sweden, with a much thinner sprinkling on the Continent;
3. Yet they show a remarkable uniformity and stability, especially in view of the absence of a cultural center comparable with Rome .... I am afraid that I have no idea how many runemasters were at work in this vast area at any given time during the period under consideration; but if there were a dozen, carving an average of one inscription a month, they must have produced more than 40,000 inscriptions in three centuries. To be on the safe side, let us agree that the 40 or 50 surviving inscriptions represent no more than one percent of all inscriptions carved, and hence may very well not be representative or typical of the total output of the period.

Of course numbers of new inscriptions have been found since that was written, but not so many as seriously to invalidate Derolez’s contention. His thesis has

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1 The article has not been revised since 2000. The statistics are to that extent outdated, but the argument remains as sound as it was.
been repeated, in one form or another, in subsequent years and applied to different runic regions. I think it still remains valid within its own terms.

Taking the Anglo-Saxon period and lands from dates between 500 and 1000, England, southern Scotland and the Isle of Man, the figures when I revised my *Introduction to English Runes* in 1998–9 stood at, shall we say?, 84 inscriptions. The number cannot be precise because of uncertainty of identification and dating in some cases. It includes the dubious Braunschweig/Gandersheim casket, and also a couple of unclear fragments from Lindisfarne though they were not individually treated in the volume. It excludes runic coins because they present a different type of problem (which I discuss below), and also the small number of travellers’ graffiti which have been spotted on the Continent in recent years (Derolez and Schwab 1983; Derolez 1987; Arcamone 1992; Derolez 1994). Since I rewrote that book six new inscriptions have turned up, three in 2000. Mean figures can of course be misleading; but over five centuries, for inscriptions known at present, this works out at eighteen a century, a rate that would hardly keep a single rune-carver at any one time busy. We must accept Derolez’s assessment that we do not have a significant sample to work from. We should be aware that any general statement about English runes (and any specific conclusion drawn from it) ought to be made in the subjunctive. Unfortunately modern English has virtually no subjunctive forms of its verbs, and moreover the subjunctive mood is not in keeping with the modern ethos. I note that even in those languages that have a respectable subjunctive runologists do not use it often enough (as Barnes 1994:22–3 noted).

The inevitable clear and general conclusion to be drawn is that from the surviving material it is impossible to draw any clear and general conclusions. And the more cheering general conclusion, that from the surviving material it is impossible to disprove any general conclusions that others have drawn.

To give an example of both positions. I recently published a short note on a new inscription, one of two casually cut on a bone from a London site tentatively dated to the eighth/ninth century (Page 1999:9–10). There I suggested that the legend, which began with the rune Š and then went on to the sequence ri½, might be taken as a personal name with the first graph representing its name *dæg*. I identified the sequence *Dægric*, a name recorded in Old English and with Continental cognates (Searle 1897, s.n., Förstemann 1900:co1.395). In a further
note as yet unpublished I cited as parallels Anglo-Saxon manuscripts which evi-
dence this practice, of using a rune in the sense of its rune-name as a personal-
name element. I was privately rebuked for this by a scholar who pointed out that
the manuscripts I had adduced, Oxford, Bodleian Auct.D.2.19 the Rushworth
Gospels with the signature ‘far\mathcal{M}', and Corpus Christi College, Cambridge, 41
item 5 with the name ‘\textsc{Salom}’ (both manuscripts described in detail in Ker
1957), were some distance in time and place from the bone inscription. I should
seek out, I was told, manuscript examples nearer to it in both these particulars.
To reply that there are no manuscripts near in time and place to this bone
inscription would be true but would not answer the gravitas of the objection. If
there were no such manuscripts should I not keep quiet about my theory, which
clearly cannot be rigorously substantiated? If we stuck to that principle we
would write very little about runes (which might indeed be a good thing). My
critic was both right and wrong in bringing his objection; but then I was both
wrong and right in putting forward my explanation.

Can a runologist go further than that? Can he be more constructive, or more
creatively destructive? It is worth while attempting; hence this paper.

The bone referred to was one found in a secondary deposit, in a group of
waste pits linked to \textit{Lundenwic}, the Anglo-Saxon settlement north of the Thames
upstream from the Roman city of London. It was the thoracic vertebra of a
sheep, which had been cooked and then presumably thrown out as kitchen
rubbish. Of course it was one of a large accumulation of bones found in this
confused environment. In many excavations such bone collections have been
neither examined in detail nor set out in number and this makes the present find
of particular interest, for the excavators (Pre-Construct Archaeology Ltd) of the
site, that of the extension to the National Portrait Gallery, meticulously provided
an inventory. The total number of mammalian bones found in this waste pit
infilling was 14,155; of sheep bones 3,760; of sheep vertebrae 634. Out of these
large numbers one solitary example has inscriptions upon it and has been cited
as part of the evidence for a literate society there. Taking the most favourable
of these figures, that of sheep vertebrae, this gives (if I interpret the figures
correctly) the statistic that 0.16\% of these bones hold runes. Even if we restrict
the sample to the thoracic vertebrae (202), the sample is only 0.49\%. Which is
not in itself a clear demonstration of general literacy in a community. Of course
there are uncertainties. How fragmentary were some of the bones listed? Can we be sure all inscriptions would be spotted when the bones were collected and, presumably, cleaned? Moreover, let us take a modern case. If all the bones we have found in our mutton stews throughout our lifetimes were gathered together, how many would have inscriptions upon them? Yet most of us would claim to live in literate communities. The tenuous evidence cannot be ignored; but it is equally important that it be not overvalued.

My second case involves coins. We have in this field a double advantage: i) we can rely upon refined expertise from professional numismatists, ii) in recent decades there has been vigorous discussion and debate over the statistics of coin finds, particularly individual examples as opposed to those from hoards. The coins I shall discuss here are the mid-eighth century East Anglian pennies of the king known variously as Beonna and Benna. In 1973 there were six examples known, one of them only from a drawing. By 1985 this had risen to 76; by 1995 it was 106 (Archibald 1985; Archibald and Fenwick 1995). This is a notable increase over twenty-five years and one that should make us hesitate to draw deep conclusions about runic inscriptions in case other finds should change the picture as drastically. Many of the Beonna pennies have runic legends, both on the obverse where the king’s name and title are recorded, and on the reverse which gives the moneyer’s name. The known find-spots are overwhelmingly and apparently significantly in East Anglia: as far south as Ipswich, as far west as Royston; extensively in Norfolk and Suffolk, but with perhaps two centres, one in northern and one in southern East Anglia: see the maps in Archibald (1985:29). There are two surprising outliers, at Whithorn in south-west Scotland and at Dorestad in the Netherlands. Both of these will be ignored as aberrant at the opening of this discussion though perhaps they should be given greater weight as I hope to show.

In considering these coin legends we must make a distinction. How significant are runes on the dies from which coins were struck; how significant are runes on the pennies themselves? The first, runes on dies, show craftsmen familiar with the script and presumably administrators requiring or accepting its use. The second, runes on coins, indicate a readiness in the moneyed public over quite a broad region to recognise if not to read the script, to observe its appear-
ance as part of the test of the validity of a coin design. Both aspects need examination and if possible statistical summary.

To take the second of these first, the public recognition of runes. Runes are used on the obverse for King Beonna’s name and title, though not exclusively. Roman is occasionally found there and the two scripts are sometimes mixed. Runes are employed on the reverse always for the names of two moneyers Wilred and Werferth, but not for a third, Efe, who uses Roman for his name. The mixture of these scripts on an official royal coinage shows some sort of formal acceptance of both in commercial use. Whether one can go further than that is doubtful.

In assessing the die evidence numismatic expertise comes into action. Twenty-six distinct obverse dies have been identified from the coins of the two most prolific moneyers, Efe and Wilred, whose names appear on 96 of the pennies. One of the dies, Efe no.9 represented by three coins, has Roman letters only on both faces. The others have either runic or a mixture of runic and Roman. How many of the total number of obverse dies that were once cut does this 26 represent? In his analysis of the Beonna coinage Cowell estimated – using a method advocated in Esty (1986) – that the total number of obverse dies cut for these moneyers was 46, though this figure was presented within wide ‘confidence limits’ as befits a careful statistician (Cowell in Archibald and Fenwick 1995:18). Of course this is a tentative figure, but if it is even approximately correct, evidence would appear to survive for about half of the material in this section.

The next question demands more speculation. How many coins could have been cut by any one die? This is a subject of debate among numismatists, one of some savagery which the non-numismatist may observe with amusement: Grierson (1963 and 1968); Metcalf (1963b); also Stewart (1963 and 1964), Buttrey (1993 and 1994), the latter article with an admonishing subtitle; but these discussions are drawn from coins of widely differing dates. Part of the problem is pragmatic, dependent on the materials and technologies available to the die-maker. How long would a die last in use, if indeed there was an assessable average life? Sellwood (1962:64) reports an experimental result:
I have been using only one obverse die and have made 9,000 coins without the
die showing any really big flaw... I should suppose that 10,000 coins per die
would be a not unreasonable minimum.

Two detailed technical examinations of the structure of surviving dies to support
such estimates are Archibald, Lang and Milne (1995) and Blackburn and Mann

But there are also questions of a different nature. Would a die be worked to
destruction or would it be subject to systematic change as in the later Anglo-
Saxon period? Would all dies have equal use? Some of the questions are econom-
ic. What control would the authorities place over the issue of coinage? How
much silver would be available and how would it be administered and checked
for quality? Was there a regular replacement of the currency, again as in later
Anglo-Saxon times? For eighth-century East Anglia there is no external evi-
dence for issue sizes, and many guesses – perhaps I should charitably say esti-
mates – have been made as to the numbers of coins that would be struck from
an individual early medieval die. One of the more modest of these has been
10,000 pennies, a figure less than that proposed for the roughly contemporary
pennies of Offa of Mercia in Metcalf (1963a: 44), but see his general assess-
ment, Metcalf (1998:25). On that basis the 46 obverse dies postulated would be
good for 460,000 coins which looks to an outsider an enormous figure. Suppos-
ing for modesty’s sake we take 10% of this number. This still gives us 46,000
coins for these moneyers. There survive 96, or 0.21%, a figure that allows only
the most tentative conclusions to be drawn about the spread of runic awareness
through currency. In the Beonna case the problem is acerbated by the fact that
37 of the Efe coins and 12 of those of Wilred, that is just over half the surviving
examples, derive from a single hoard, from Middle Harling, Norfolk. In these
circumstances the two errant examples from Whithorn and Dorestad may
assume a greater significance than has been given them hitherto. Do they imply
that the coins scattered quite widely beyond East Anglia?

Before leaving the coin evidence I would make one further point. The
importance of coins in the runic record is probably being weighted by new
techniques available for finding them, particularly the common use in parts of
England of metal-detecting equipment. This could lead to a skewing of runic
evidence in favour of metal objects, of which coins are prominent examples. Here, however, we go beyond coins. It is suggestive that the most recent (to 1999) ten issues of Nytt om Runer list twelve new finds of Anglo-Saxon epigraphical runes from the British Isles (and a possible thirteenth in the uncertain Boarley). Of this number nine are on metal, and of them five were reported as metal-detecting discoveries. Two of the three new finds of 2000 are also the fruit of metal detecting. Of this group of sixteen only one (from Billockby: Page and Blackburn 1998) is a coin find, but that was reported because it is a type hitherto unknown. Duplicates of known coin types discovered by this method may not be generally advertised.

This leads to another primary question: how was the Anglo-Saxon epigraphical runic corpus established? How did the number of known inscriptions reach the 1999 figure of 84 over the years; to which number must be added the six more found since that catalogue was compiled? In 1884 George Stephens produced his Handbook of the Old-Northern Runic Monuments of Scandinavia and England, summing up his immense, then three-volume, corpus (Stephens 1866–1901: vols. 1–3). Of course his identification of objects as runic was often faulty. Indeed he deliberately included in that collection a number which had early inscriptions but which, as he himself realised perfectly well, were not runic at all. If we remove these aberrations, his 1884 Handbook included 34 English runic monuments. Nineteen were on stones, eleven on metal and the rest on artefacts of bone, wood or semi-precious stone. The posthumous vol. 4 of his great work added four more: three of them on stones and one on bone. In 1961 Hertha Marquardt published her British Isles volume of the Bibliographie der Runeninschriften nach Fundorten. Again it contained disputed examples or those that Stephens had included knowing them to be non-runic. If we remove these her list has 52 Anglo-Saxon items: 33 on stone, thirteen on metal, six others. In 1973 the index to my first edition of An Introduction to English Runes listed 64 runic objects: 35 stones, 20 metal pieces, nine others. The 84 of the 1999 list is composed of 36 on stones, 32 on metal. Sixteen are on wood, bone, semi-precious stone and pottery. Of the six found subsequently, five are on metal, one on bone.

The increase in the number of stones known is not negligible, from 19 (1884), 22 (1901), 33 (1961), 35 (1973) to 36 (1999). That of metal inscriptions
is, however, much more dramatic: 11 – 11 – 13 – 20 – 32, and now with the new finds 37. The big jump is from 20 in 1973 to 37 in 2000 (contrast the expansion of known rune-stones by one in this period). If one adds to this the immense numerical increase in some of our runic coin samples it becomes evident that runes on metal are a growth industry.

The seventeen new examples (which excludes Boarley) comprise two from Brandon (Suffolk), one each from Chessell Down and Froglands (both Isle of Wight), Gayton Thorpe (Norfolk), Harford Farm (Norfolk), Heacham (Norfolk), Heslerton (North Yorkshire), Keswick (Norfolk), London (Thames Exchange), Malton (North Yorkshire), Morton (Lincolnshire), Undley (Suffolk), Wardley (Rutland), Watchfield (Oxfordshire), Wheatley Hill (Durham); one as yet unprovenanced. Those of Brandon, Harford Farm, Heslerton, London and Watchfield, were finds from controlled excavations. Wheatley Hill, Heacham and Undley are reported as chance discoveries. The new Chessell Down example was spotted in a museum collection. The finds from Froglands, Gayton Thorpe, Malton, Morton, Wardley and probably Keswick were by metal detectors. In recent years, then, metal-detecting has revealed a significant number of rune-inscribed pieces, and the distribution pattern they reveal may reflect that of metal detectorists rather than of inscribed metal artefacts. There is the teasing parallel of Anglo-Saxon grave-good finds in the 1920s and 1930s, which were discovered to be significantly within cycling distance of Oxford and Cambridge, reflecting less the distribution of Anglo-Saxon burials and more that of Anglo-Saxon archaeologists.

The small number of runic monuments involved here prompts my third case, my final question. What is the significance of no runic monuments at all in a region? Is zero an important statistic? Should we keep in mind the Great Detective’s positive assessment of negative evidence when he drew attention to “the curious incident of the dog in the night-time”?

“The dog did nothing in the night-time.”
“That was the curious incident.”

I adduce two examples from towns that have undergone detailed and careful excavation in recent decades, Winchester in Wessex and York in the north. Both
were important administrative centres in Anglo-Saxon times, were major trading
towns, were the seats of bishops, and both had mints. a) For Winchester the
hand-list of Anglo-Saxon non-runic inscriptions (Okasha 1971) adduces five
examples: two stones (one fragmentary), two metal artefacts, and one group of
clay bell-mould fragments. Okasha (1983), supplementing (1971), added three
more: one on stone, two on metal. In so far as these can be dated they appear to
be tenth- and eleventh-century. As far as I know, coins from Winchester in these
centuries do not evidence runes, at any rate as significant forms. Numbers of
Anglo-Saxon manuscripts are attributed to Winchester Old Minster. A check of
the list in Ker (1987) against Derolez (1954) shows that only two of them have
even the most slender runic links – \( \Upsilon \) used for the syllable \( \text{wyn} \)- and runic forms
in an obscure passage of an Anglo-Saxon charm. No runes were found in a
similar check of New Minster material. There is thus virtually no evidence of
(English) runic usage in Winchester in the later Anglo-Saxon period, but can
that be accounted evidence for no runic usage? b) In the case of York Okasha
(1971) has eight entries, all on stone, most from comparatively late dates though
a couple may be as early as the eighth century. Okasha (1983) added a late ivory
seal-die, and Okasha (1992) the eighth-century Coppergate helmet. Again,
coinage does not clearly evidence runic usage from York. No Anglo-Saxon
manuscripts survive from the town. Here, as far as it goes, there would seem to
be a zero result were it not for two embarrassing details: i) there survives a
photograph of a small stone fragment from York Minster (the original piece
seems to be missing at present) which has two fragments of graphs which could
be runes, more likely Anglo-Saxon than Norse but not certainly either. ii) a find
from 1884 is a wooden spoon decorated with a series of incised step patterns,
alternatively hatched and plain (Waterman 1959:85-6). Two runes are cut on
one of the plain sections. Unfortunately they are ambiguous. They give no
obvious sense but their forms could equally well be Norse as Anglo-Saxon –
there is no means of determining. The evidence for the Anglo-Saxon use of
runes in York is confusing but not convincing, but neither is it negative.

This short discussion has, I hope, demonstrated once again the fragility of
the database from which we draw our conclusions. It also shows how that data-
base can change with time as fresh research adds more material; quite drama-
tically in as small a sample as that of the Anglo-Saxon runes. It changes in
extent, but also in distribution through time and space. Not surprisingly we may expect new theories about the use, development and distribution of the script in Anglo-Saxon England (as, for instance, Parsons 1999), which is salutary if such theories are regarded with caution. But let us remember that even the newest theories are likely to look jaded after a few more years of runic discovery. Hence we must be wary about conclusions drawn even a short time ago. Inevitably much of our thinking builds upon observations made by earlier scholars, scholars writing when runic evidence was even sparser. So we need continuously to rethink our principles.

Should we then accept our vulnerability and refuse to come to any conclusions at all? Here my early training as an engineer comes into play. A primary rule of engineering is that one should not demand perfection. “Do the best you can with the materials at hand.” So I do. And to this I add a second rule of engineering. “Don’t be surprised if it doesn’t work.”
Bibliography


