# pictish arts society
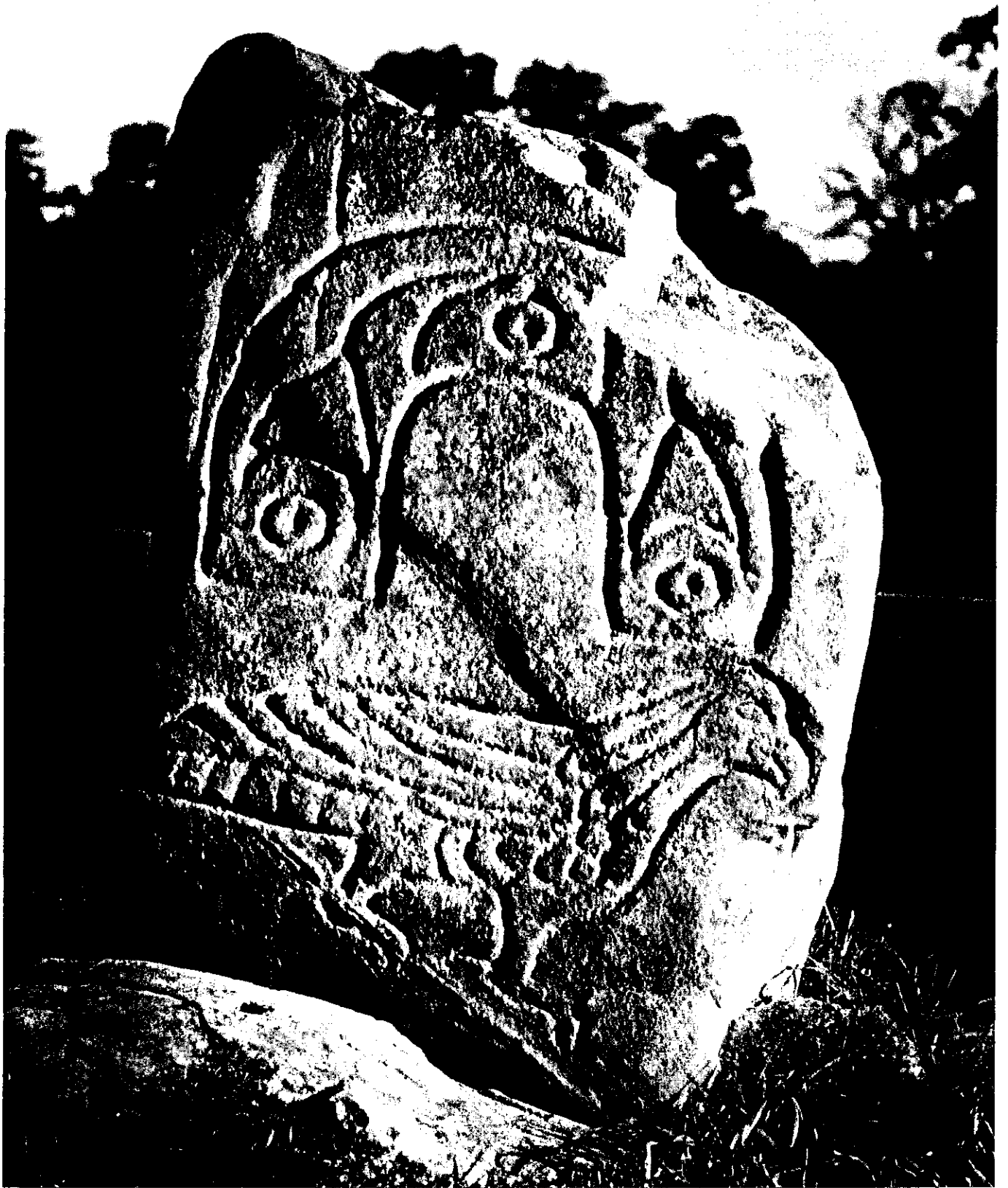## journal 14

Pictish Arts Society Journal
Volume 14 (1999)

Edited by: Niall M Robertson, Sheila Hainey, Eileen Brownlie, and Nick Simpson

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# Foreword

An apology is due to all the members of the Society for the failure to find a replacement for Niall Robertson over the past year. As a result, there has been an unacceptably long time taken in producing Journal 14.

Finally, here it is. Niall contributed some of his scarce free time to do some editing, Eileen and Sheila did the same, and Nick Simpson once again spent many hours on the production. We now have a new editor, in the person of Lloyd Laing, and look forward to producing the next issue in a rather more timely fashion.

## The Pictish Arts Society

Contributions for the next issue
of the *Pictish Arts Society Journal* should be sent to:

**Hon. Sec. PAS - Sheila Hainey**
**Torr of Kedlock Farmhouse**
**Kedlock Feus**
**Cupar KY15 4PY**

Contributions on disc (or via email to publications@pictarts.demon.co.uk) would make production much simpler - please remember to send a hard-copy for reference, but typescript or hand-written documents are also welcome.

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# The Forres Stone (Forres 2)

## Alastair Mack

"...early this year," T.E. Gray wrote in the Pictish Arts Society Journal (Autumn 1992, 25-26), "I heard ... that an unrecorded Pictish symbol stone had been found in Forres... At that time, the stone was built into a garden wall... about three feet up from the bottom of the twelve foot rubble wall... Mr and Mrs MacArthur had lived in their house (Rosebank, St Leonard's Road) since 1960, and were well aware of their stone.... However, it was only in the last couple of years.... that they decided to report it to the (Falconer) Museum"

The essentials of 1992 DES, 39 entry are:

'Rosebank', St Leonard's Road, Forres (Forres Parish) G King
Symbol Stone
NJ 039 588 A fragment of a Class I Pictish Symbol Stone ... was found built into a garden wall.

The essentials of the 1994 handlist report, 13 are:

'Forres',
NJ 05 NW 86 NJ 0393 5880
A fragment of a sculptured stone.... was found built into the garden wall of Rosebank,
St. Leonard's Road....'

The said wall is the garden wall on the east side of Rosebank, which is the wall dividing the house area and garden from an adjacent walled orchard (the near part of which was then owned by the MacArthurs), and the stone was found built into the wall's east side, the orchard side, near its base (about three feet up from the bottom'). The stone was in the wall's lower courses, which are patently different from the upper courses. The lower courses were part of the west wall of a garden or orchard which was there before Rosebank was built; they were part of a wall rather older than Rosebank.

Robert Douglas wrote in Annals of the Royal Burgh of Forres (1934), 495, On the left hand (north) side of the road, at the entrance, there stood, in 1823, opposite Mr. McCulloch's (where later stood the Independent Church), a number of small houses' Following this there was a large open space used as a garden and a little further on three houses facing the road and known as Trafalgar Place.' on his p. 496, he continued: 'In place of the old houses on the left we have the modern block built about 1880...Asher's Buildings. Next in order comes: Laurel Bank...Adjacent to Laurel Bank is.... Rosebank. Before these houses were built, about 1863, there was a square here, (between and behind the third and fourth of the 'small houses') A high wall runs alongside the road till we come to Towerside.... Towerside was formerly Trafalgar Place.'

From its earlier name, Trafalgar Place/Towerside was presumably built after 1805, perhaps not long after that year. The line of the later reported 'high wall (that) runs alongside the road' is shown on the 6"O.S. map Elginshire Sheet X (surveyed 1868-70) as the roadside wall of a rectangular walled area that appears to be an orchard (the latter apparently where the 'large open space used as a garden' was). The lower courses at least of the 'High wall' are therefore pre-1868. These may be contemporary or nearly so with what was Trafalgar Place and therefore may have been built not long after 1805. But because Trafalgar Place/Towerside is a terrace-block, a block of what were three or four adjacent dwellings, it is unlikely that the ? orchard/garden was to do with it. The present wall's lower courses may have predated Trafalgar Place, and it is possible that the stone was found and built into them some time before c. 1805. But even if the lower courses were contemporary with Trafalgar Place the symbolled fragment

or fragments could have been found about that time and built into them without report; such finds before c. 1820 are seldom reported.

Although Douglas seems to state that Laurel Bank and Rosebank 'were built, about 1863,' the afore-mentioned map shows on the north side of St. Leonard's Road four separate small houses or buildings next to the ?walled orchard, and a block named Towerside after the ?orchard. The four were not back from the road, as both Laurel Bank and Rosebank are, but between the third and fourth houses was a space, and there was a court or square behind them with three ? outhouses at its rear, which court or square may be Douglas 'square'. It seems that Laurel Bank and Rosebank were, like Asher's Buildings, built after 1868 rather than about 1863. They are not shown on the O.S. Map Sheet X; in front of where they now stand were the third and fourth of the four small houses on the north side of St Leonard's Road.

The 1:10,560 OS Map Sheet NJ 05 NW (revised 1930-45) gives no names for the relevant buildings but shows on ?Asher's Buildings and a walled ?orchard, back from the road, from their shapes and positions the latter pair are Laurel Bank and Rosebank; a building recognisable as the earlier Trafalgar Place/Towerside block is on the other (east) side of the walled ?orchard. The overall picture is that at least the lower courses of the wall in which the Forres 2 fragments were found pre-date 'Rosebank', which was built certainly after 1868 (from the house's appearance c. 1870). These courses were laid before 1868, possibly before 1823 and perhaps not long after 1805-but they may be earlier still, earlier than the adjacent Towerside/Trafalgar Place which was presumably built when the 1805 sea-battle was a recent memory). The stone may have been placed in the wall either near the beginning of the 19th century or before it. Even the later date will explain why the stone was not at the time reported, but it is interesting to note that when the stone was taken out of the wall it was found to be in two pieces that had been reassembled. This may be evidence of some familiarity with Pictish symbol stones on the part of the finder or builder-but not enough, it seems, for the find to be reported.

Where did the stone come from? Trafalgar Place's being renamed Towerside was not because of a nearby early tower or castle, in the remains of which the stone or its fragments may have been discovered. There was a mediaeval castle in Forres, but its site, Castle Hill, is at the burgh's west end (some 700 metres west of Towerside). The new name was assumed after 1823, presumably because of the building of 'Nelson's Tower' on the summit of the Cluny Hills — about 500 metres north-east of Towerside, which backs on to the Hills. The Tower was opened in 1821, and Trafalgar Place's new name may have been adopted because of the Tower — and also perhaps because there was another 'Trafalgar Place' in Forres.

The stone or its fragments may have arrived in one of the loads of stone carted in to build the original ?orchard wall, but such loads would have been quarried rather than simply gathered. The 'rubble' of even the wall's lower courses is quarry–cut stone. As the Forres 2 fragments are, when joined together, somewhat larger than most of the wall's other component stones, it is possible that the fragments were 'local' pieces used in addition to the wall's quarried materials. Although there is a cemetery, the Cluny Hill Cemetery, only 300 metres east of Rosebank, the earliest part of this was laid out only in 1849-and Douglas also stated that 'Previous to 1848 there was no Cemetery on Cluny Hills'. There was an oval hill-fort (shown on Sheet X as 'British Camp (Site of)') on the summit where the Tower stands, but no burials in its vicinity have been reported-save for an isolated one said to be c. 1810. It is possible that if the hill-fort was still in use in Pictish times or even later the surviving pieces of Forres 2 may have come from the material of the fort's upper ramparts, the stone or its fragments even then having been 're-used'. But this can be no more than guess-work-especially as a 1798 survey report was that the fort's wall was an earth-work (with a ditch outside it). It is most unlikely that Forres 2's earlier findspot, far less its original findspot, will ever be known.

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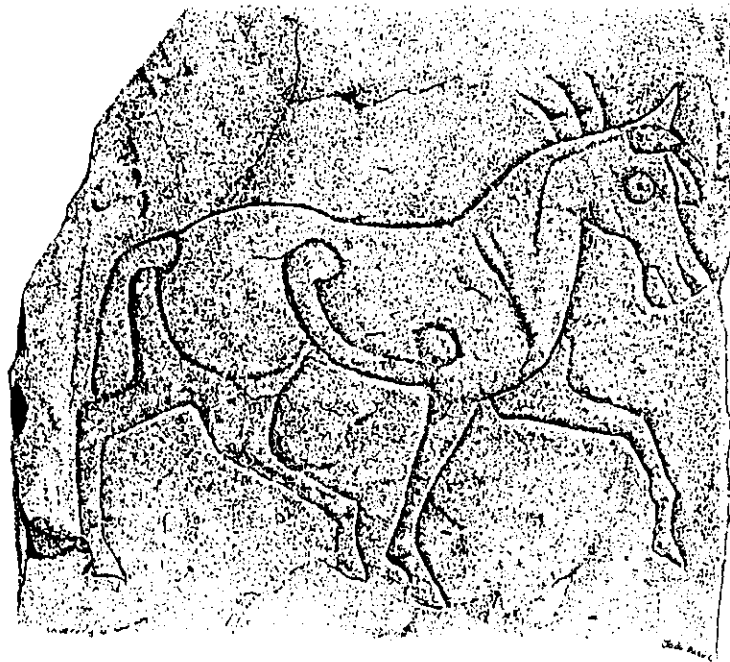
# The Camuston Cross and its Iconography

## Lloyd Laing

Although weathered, the Camuston Cross, near Monikie, Angus, is of singular interest on account of its Crucifixion and *Majestas Domini* iconography.

The Cross has a Crucifixion on the head, with a Sagittarius (or centaur with a bow and arrow) beneath in a panel, and a panel of vinescroll at the bottom of the shaft. Further vinescroll is represented on the sides, and on the back is Christ in Glory flanked by angels, with four facing figures in two panels beneath (probably the Evangelists). It stands 6ft 6in high, and belongs in the same tradition of free-standing crosses in Pictland as that at Dupplin, which display eclectic influences in their iconography and design.

The Crucifixion is an extremely unusual subject for a monument set up in Pictland. Allen and Anderson (1903, 406) list five examples, of which Strathmartine 4 is clearly an error (it has no figural decoration), the others being Kingoldrum 3, Monifieth 4 and Abernethy 4. Perhaps distinctively Pictish is the absence of a cross in all the depictions.
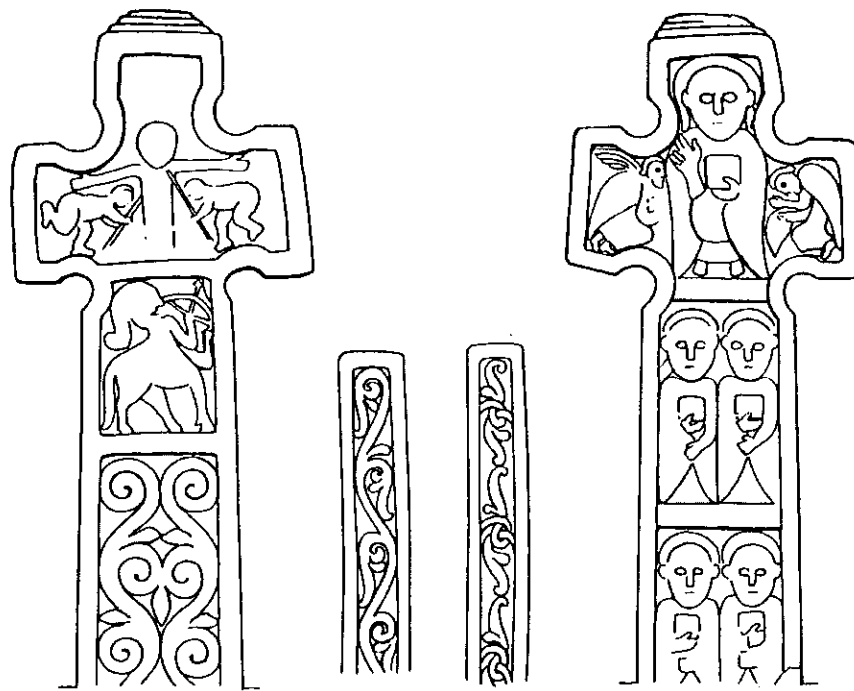


Fig.1. **The Camuston Cross**, after Allen & Anderson, 1903

The iconography of the Crucifixion on Camuston and Abernethy 4 appear to be similar. Christ is shown on both flanked by the soldiers Longinus and Stephaton; Stephaton (holding the sponge on a pole) is depicted on the Camuston Cross with the pole going across Christ's body, the sponge just below his chin. It is difficult to determine due to the weathering what Christ is wearing on the Camuston cross, though it may be a loincloth, as suggested by the drawing in Stuart, 1856, Pl.87, but the figure on Abernethy 4 appears to be in a loincloth. The top of the pole and Christ's head are missing on Abernethy 4, but the angle of the pole, if projected, would appear to be in the same position as that on Camuston. The Camuston Christ is beardless (as is the Christ on the Kingoldrum 3 fragment), and his head is erect, not tilted in death, as in some Carolingian representations.

The inconography of the Camuston Crucifixion is unusual, but is found in Ireland on the cross-slabs from the islands of Duvillaun More and Inishkea North, Co Mayo, incised monuments regarded by Francoise Henry as being of the seventh-early eighth century but re-assigned by Harbison to the ninth (Harbison 1987). On these an apparently unbearded Christ is shown wearing a loincloth. A notable feature of the two Co Mayo Monuments and the Pictish examples is the positioning of Stephaton on the right and Longinus on the left. This is the reverse of the usual arrangement on Irish high crosses, with the exception of representations at Moone, Castledermot South, Ullard and Arboe (Harbison 1987, 76), where however Christ wears a long garment rather than a loincloth.

On the High Crosses of Ireland the usual type of Christ wears a long robe (the colobium of Eastern tradition), though a loincloth is depicted at Glendalough, Tuam, Temple Brecan (on Arran) and in a few other dubious examples (Ibid, 285-6).

Christ on the Camuston Cross is depicted beardless (this is also the case with the Kingoldrum fragment), a feature of most Irish crosses and very rare before the ninth century. Harbison has pointed out that although a beardless Christ appears on a fifth-century Italian ivory, and on a lost mosaic in the Old Basilica of St Peter's in Rome, the next occurrences are on examples of art datable around 830 or later (Harbison 1992, 276-7).
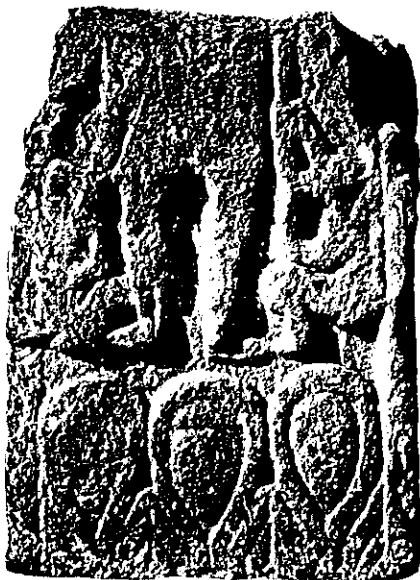


Fig.2. **Abernethy 4**, after Allen & Anderson, 1903

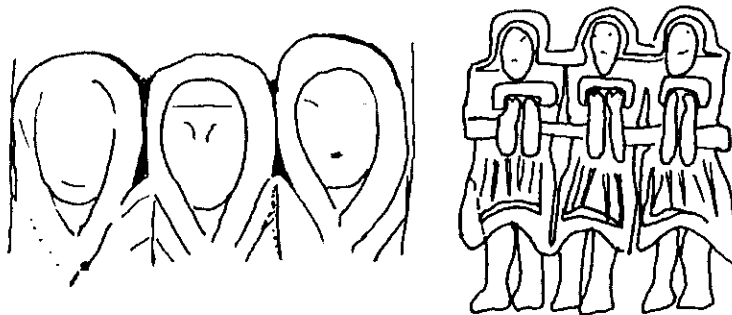Fig.3. **The Kingoldrum Crucifixion fragment**, after Allen & Anderson, 1903



Fig.4. **Three Maries** at Abernethy 4 and Gainford

The depiction of Stephaton holding the upper part of the pole across Christ's chest with the sponge roughly below his chin occurs as early as the sixth century, but only on depictions with Stephaton on the left and Christ in the *colobium*. The first occurrences of Stephaton on the right and with Christ in a loincloth occur in the northern part of the Carolingian Empire, in the ninth century, notably in fol.51v of the Utrecht Psalter, produced around 830 (Harbison 1987, 83). But, as Harbison has noted, the occurrences of a Crucifixion in this tradition with the figures of Stephaton and Longinus smaller than Christ (as on the Co Mayo monuments and on the Pictish representations) are on Carolingian ivories, one in the Treasury of the Cathedral of St Just in Narbonne, the other on the cover of the Pericopes of Henry II (Ibid, 83). Harbison has also drawn attention to the fact that most Carolingian depictions of the Crucifixion show Longinus' spear penetrating Christ's armpit, not his ribs. The spear entering the ribs which is apparent on the Co Mayo monuments and on the Camuston cross can however be seen in a tenth century Breton miniature in the Bodleian library, Oxford (fol. 9b, Laud lat.26) (Harbison 1987, 83- 84).

The small crouching figures of the soldiers apparent at Camuston and Abernethy 4, are in keeping with the depictions on Irish High Crosses.

The heads of three figures beneath the Crucifixion on Abernethy 4 have their counterparts in the North of England, for example on the Aycliffe shaft and at Gainford (Bailey 1980, figs 55-56). The Aycliffe cross also has a Crucifixion scene with Stephaton and Longinus (positioned below the three figures rather than above) (Coatsworth 1978). The linking of the haloes is a distinctively regional feature, which Bailey has seen to have been derived from the earlier cross at St Andrew Auckland. If this is correct, then the inspiration for Abernethy 4 may have come directly from Northumbria (Bailey 1980, 193-4). The subject matter of the three heads was seen by Allen and Anderson as representing the Three Maries, but Dr Ross Trench-Jellicoe has suggested to me that they were probably representations of priests - those designated to impart the salvation message.

The Crucifixion with Stephaton and Longinus is a feature of Viking Age sculpture in the north of England, where Bailey has noted that it harks back to archaic models, such as Bakewell (1980, 152).
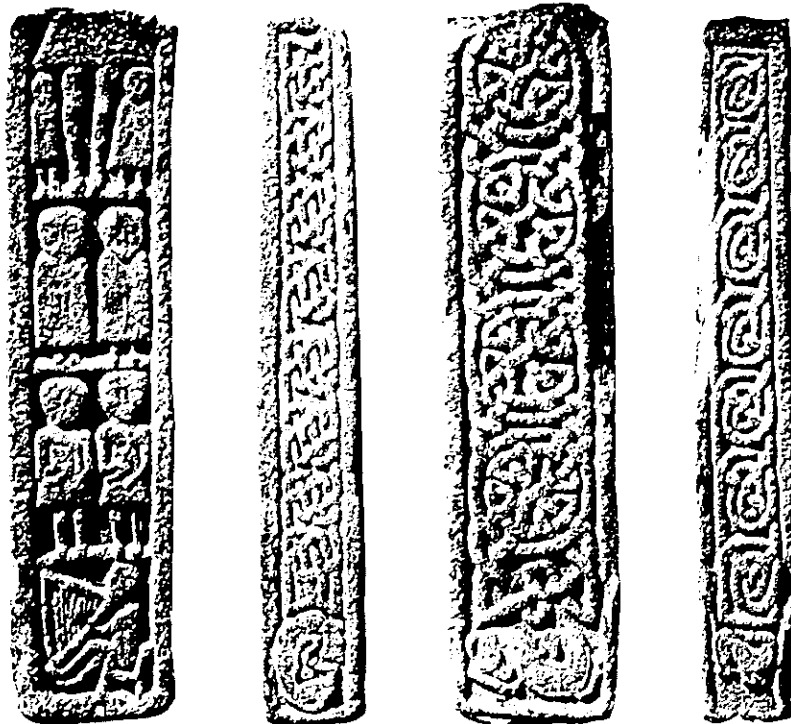


Fig.4. **Monifeith 4**, after Allen & Anderson, 1903

Monifieth 4 has a different type of Crucifixion iconography from that at Camuston and Abernethy 4. On this fragment Christ appears to be wearing a loincloth (though this is far from clear), and is flanked by two figures, possibly St. John and St. Mary. This iconography is firmly rooted in Carolingian tradition, and was that generally favoured in southern England in the tenth and eleventh centuries (Bailey 1980, 152). The figures appear however to be carrying books, and it is possible that if this is the case they represent another version of the *Majestas Domini* depicted on Camuston. Monifeith 4 displays Viking Age animals with double outlines, of Jellinge derivation, and interlace which likewise would be at home in a Viking-Age millieu in the tenth century.

Little can be said about the iconography of the Crucifixion on the Kingoldrum fragment, since only the very top survives and is extremely crude (reminiscent in this respect of the Co Mayo slabs). There is some doubt at to whether this fragment is in fact of Early Christian period date, and may well be later medieval.

Turning from the Crucifixion to the *Majestas* on the back of Camuston, there is no other occurrence of this subject in Pictland, though Isabel Henderson has seen some similarities of composition in the Albar Stone in Brechin cathedral (1978, 58), and Close-Brooks and Stevenson have suggested it may be represented at Invergowrie (1982, 39). It is comparatively rare in Ireland, being represented at Carrowmore (on the South Cross), the Cross of the Scriptures at Clonmacnois, the Cross of Patrick and Columba at Kells, and on Muiredach's Cross at Monasterboice (Harbison 1993, 301). There are fairly close connections between Pictish sculpture and the Clonmacnois and Kells sculptures, but the closest representation to that at Camuston is found at Carrowmore (Harbison, *loc cit*).

The Sagittarius does not occur elsewhere in Pictish art (though centaurs without bows and arrows do) and is very rare in Irish iconography, but appears on the east side of the base of the Market Cross at Kells and on the north side of the base of the Cross of Muiredach at Monasterboice. In the latter case it appears to be part of a series of zodiac figures.

The vinescroll has been studied by Isabel Henderson, who has compared that on the front with that on the shaft at St Peter's, York (Henderson, 266 - for St Peter's, York, Cramp and Lang 1977, no 9, where dated to the ninth century), while seeing the vinescroll on the side as being of fleshy-acanthus type, similar to St Andrews 19. Lang saw a close connection between the vinescroll on the Camuston Cross and the Brechin hogback, comparing the pattern with that on the arch of an Irish bell shrine from Ahoghill (Henry 1967, pl.56). He also saw Irish parallels for the plants tendrils on the second side, with connected bunches of leaves (Lang 1972-4, 216). Lang has also pointed to the presence on Camuston of facing clerics which he saw as being of Irish style, as are the similar figures on other monuments such as on slabs from Kirriemuir and Aldbar (1972-4, 216). These features Lang saw as dating the monument to around 1000, and to be due to Irish influence at nearby Brechin at this date.

The Camuston Cross seems to owe a debt to Carolingian artistic traditions, probably reaching Eastern Scotland from Ireland. It represents a separate iconographic tradition from that of Monifieth 4, but all the depictions of the Crucifixion in Pictland probably date from the late tenth century.


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# The Pictish Symbols at Trusty's Hill, Anwoth, Kirkcudbright

## Lloyd Laing

The supposed Pictish symbols inscribed on a rock face at Anwoth, Kirkcudbright, (Allen & Anderson 1903, 477-9 and fig 508; Stuart 1856, pl 97), have prompted vigorous debate in the past, and have been used to advance the otherwise insubstantial claim that there were Picts in Galloway. They were illustrated by Radford (1953), who pointed out that although the horned face 'symbol' appeared to have been enhanced in the recent past, the recutting followed the lines of an original. All the symbols otherwise appear to have been cut at the same time, and if they were a forgery, as postulated by Oram (1993, 15), and by implication by Jackson (1984, 37), then they must pre-date Stuart's drawing in the mid-nineteenth century, probably by some duration for him to consider them genuine, projecting any 'forgery' to a period when interest in Pictish symbols in SW Scotland was likely to have been virtually non-existent. The carvings should probably be seen as ancient, but whether they should be regarded as the work of Picts or not is another matter.

In a recent perceptive study of the problem of interpreting Pictish symbols, Katherine Forsyth has argued cogently that many of the so-called 'symbols' are not in fact part of the symbol repertoire, and that true symbols must be found in pairs with other symbols. 'The key criteria for inclusion in the inventory of 'core' symbols is that the design must appear at least once in paired combination with another symbol' (Forsyth 1997, 88). On the Anwoth rock face there are four 'symbols' in combination. Forsyth has argued that only two symbols compose a 'statement', though in the case of mirror or mirror and comb juxtaposed this can be seen as a qualifying statement, an interpretation which she also sees as an explanation for rods and notches combined with other symbols (ibid., 89). She has drawn attention to the fact that while there are often several symbols on Class II stones, they are in pair combinations, and each pair may relate to a separate individual being commemorated on the stone.

For the sake of argument, the left-hand pair of symbols at Anworth might loosely fit Forsyth's definition, since the upper device is a double-disc and Z-rod, and thus arguably a recognized symbol, even though the bottom device, a horned human head, appears nowhere else in the repertoire of symbol art. The right-hand pair are totally without parallel in the symbol repertoire: a hippocamp-like figure and a 'sword', (Radford saw it as a 'whetstone'), positioned not in the usual arrangement of one symbol above the other, but with the 'sword' at an angle to the 'hippocamp'. Allen and Anderson drew attention to the general similarity of the hippocamp to those on a Class II stone at Brodie, Elgin (1903, 132). The Brodie stone is a late monument, probably of the ninth century, and bears in addition to the pair of hippocamps actual symbols (elephant and double disc and Z-rod) and ogham inscriptions. It must not be forgotten however that Allen and Anderson regarded hippocamps as symbols, but later commentators have rejected them from the repertoire (see Forsyth 1997, 88), and therefore this right-hand pair must be rejected as a genuine Pictish symbol pair altogether.

The double-disc and Z-rod looks at first sight like an orthodox symbol - but is it? The style of the Z-rod, with identical spear-heads at each end, is more in keeping with Class II monuments than the incised work of Class I, and the Z-rod is woven through the double-disc symbol instead of crossing it in the universal way found on Class I stones. The weaving of the rod is however discernible on some Class II stones, such as Kirriemuir 2, Elgin and Meigle 7. While not discarding the idea that the symbol is of Class I (presumably since it is incised, not in relief), Henderson drew attention to its late date (on the basis of the similar ends to the extremities of the Z-rod - 1958, 50n), a view endorsed by Mac Lean, whose own typology of the symbol put it late, and who suggested it may have been carved during a Pictish raid following the Battle of Dunnichen (1992, 66-67), an explanation endorsing an earlier con- clusion of Radford (1953). Murray, in his typology of the 'declining symbol' put Anwoth into his Class 4, that is, at a stage quite far removed from the 'parent' form (1986, ill 8, 733). It may be noted that the Elgin slab is one of the latest in the Pictish series, being datable to the Viking Age.

Excavations by Charles Thomas at Trusty's Hill, although finding evidence for secondary activity, produced no positive evidence that the site had been occupied in the early medieval period beyond the occurrence of the symbols and the possibility that the name is derived from the Pictish Drust (cognate with Tristan) (1960). This interpretation of the place-name however has been questioned, as 'Trusty' could be derived from other Celtic personal names (Cessford 1994, 84).

The likeliest explanation for two of the 'symbols' is that they were inspired not by Class I engravings at all, but by relief carvings found on Class II monuments. The interweaving of the Z-rod and the matching curlicues on its terminals are indeed in keeping with symbols done in relief on Class II stones, at a time when standardization had been abandoned. They were executed, then, by someone who had seen Class II Pictish stones, but was not necessarily remembering correctly what he had seen. They are intended to look like Pictish symbol combinations, but in fact are not. It is very unlikely that any of the Class II stones pre-date the mid eighth century, and the majority are probably of the ninth. This fact would preclude the 'Battle of Dunnichen' theory favoured by Radford and Mac Lean and render the carving of the symbols by Picts on a raiding party exceedingly unlikely.

The two totally aberrant symbols are worth further consideration. The horned face, which looks disconcertingly like that of a pixie, is not something found in the repertoire of early medieval art. A somewhat similar phenomenon occurs however some 7 miles north of Glasgow on Craigmaddie Moor, where the 'Auld Wives Lifts', a stone setting, has relief carvings which include a horned head which Alcock has suggested may in fact be an Iron Age carving of a Celtic 'horned god' such as Cernunnos (Alcock, 1977). Rather than being a Pictish carving, could the horned head be of Iron Age date, recut at the time the other symbols were added? The same might be said, though with less confidence, about the 'sword' (if that is what it is), the round pommel of which is not in keeping with swords of the early medieval period but rather with Roman daggers and swords of *gladius* type, current in the first/second centuries AD (Cf. ill. in Bishop & Coulston 1989, ch 4). In a recent study of the Anwoth carvings, Cessford has suggested that the horned head might be of Germanic derivation, and be seen to represent a 'devil', possibly a depiction of Woden (Cessford, 1994, 86). He has suggested that the pair of 'non-symbols' were added to convey the message 'we killed' or 'death to' the person commemorated in the first pair (1994, 86-7).

In the final analysis, in what context the Anwoth 'symbols' may have been carved remains a mystery; certainly they do nothing to further the myth of the Galloway Picts. Nor is the 'Pictish raiding party' a very convincing explanation in the context of the period to which the symbol pair belong. Perhaps they represent a local commemoration of a marriage alliance which paired a high-status Pict with a Galloway aristocrat, possibly (given the date), an Angle.

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# Matriliny at the Millennium: the Question of Pictish Matrilineal Succession Revisited

## Kyle Anne Gray*

### INTRODUCTION

Just over one thousand years ago, the Picts of Dark Age Scotland disappeared from the historic record (Broun 1997). Documentary accounts of the Picts from that time on deal with them as something of a mystery, particularly from the later medieval period forward. A large part of the mystery arises from the fact that many historic descriptions of the Picts state that they followed a custom of matrilineal descent. Irish genealogies and literature, Saxon and Welsh chronicles, even Bede's "Ecclesiastical History," all give some version of the same story, that inheritance among the Picts was decided by virtue of the mother's bloodline. When systematic studies of the Picts began in the Victorian age, these accounts of Pictish matriliny were seen to be confirmed by the Pictish King Lists, Dark Age documents which show a pattern of succession different from that followed in contemporary Irish, British and English kingdoms. (Skene 1867). By the middle of the 20th century, Pictish matriliny was accepted as an established fact among Pictish scholars (Wainwright 1955). At our end of this century, however, scholarly opinion is now split on the issue of matriliny among the Picts (Foster 1996). With the help of modern anthropology and ancient Irish literature, this paper will explain how the split occurred and why, at the end of this millennium, it is now safe to finally accept that the Picts, in fact, were a matrilineal people.

### DISCUSSION

#### From Skene to Smyth

I first stepped into the maelstrom of matriliny in 1996. Attracted by the intriguing Pictish King List question, I suggested a solution in an article published in this Journal that I was honestly astounded had not been propounded before: that the reason the names on the parental side of those lists are so strikingly different from the names on the king or son side, is because the listed parents – for the most part – are mothers, not fathers (Gray 1996). Given the Pictish matrilineal succession pattern, I argued, a listing of mothers would not be unexpected.

In researching to make sure this solution had not been suggested before and rejected for solid reasons of which I was unaware (it had not), I discovered that while up through the 1980s Pictish scholars were in near total agreement that the Picts followed a practice of matrilineal descent, in the 1990s there was a swing away from this general agreement (e.g., Foster 1996). In that same issue of this journal in which my article appeared, another author expressed the opinion that the idea of Pictish matriliny is not "generally accepted anymore" (Cessford 1996, 29). Intrigued, I decided to find out what caused this split, and to assess the evidence that drove it.

The first period of modern Pictish studies began with William Skene's publication in 1867 of *Chronicles of the Picts, Chronicles of the Scots*, a compilation of Dark Age and medieval documents from early Scottish history. Skene discussed Bede's famous statement that the Picts chose their kings from the female royal line. Bede explained this succession practice by reference to an Irish legend that the reason the Picts followed such a "curious" form of succession was because when they first

immigrated to Scotland, the Picts brought no women with them, and thus asked for wives from the Irish, who consented on the condition that thereafter the Picts would choose their kings from the female line. While skeptical of the legendary explanation, Skene believed Bede's explained "fact" of matrilineal succession was true. In particular, Skene pointed out that sons did not succeed fathers in the Pictish King Lists, and that the names of the sons and their "fathers" were strikingly different. Skene also commented on the prevalence of brothers succeeding brothers, and the fact that some Pictish kings apparently had foreign fathers. He attributed this evidence to matrilineal succession. (Skene 1867, ci-ciii).

Relying on Skene's textual evidence, the theory of Pictish matriliny was accepted among early 20th-century Pictish scholars as the descent pattern for the Picts (e.g., A. Anderson 1922; Diack 1944; H. Chadwick 1949).[1] When the second period of modern Pictish scholarship began with the publication in 1955 of Wainwright's *The Problem of the Picts*, Pictish matriliny became an established fact (Wainwright 1955; Thomas 1963; Henderson 1967). Indeed, by the 1970s, Pictish scholars turned increasingly to questions of how matrilineal succession worked within the structure of Pictish society (Jackson 1971; Anderson 1973; Kirby 1976; Boyle 1977; Miller 1978, 1979a, 1979b, 1982).

In 1984, Alfred Smyth published *Warlords and Holymen*, a reassessment of the history of Dark Age Scotland. Within his book, Smyth "took on" the issue of matrilineal succession, arguing that it was a remnant from the days when the Picts were wrongly thought to be other than a Celtic people. Based in part on the existence of brothers in king lists from Dark Age Leinster, and the presence of the words *meqq* and *maqq* in Ogam inscriptions on a few Pictish stones, Smyth contended that the theory of Pictish matriliny was simply the result of a misreading of Bede – who Smyth claimed had said matrilineal succession occurred only in "exceptional circumstances" – and a regrettable misunderstanding of how kingship worked in the Dark Ages. Seemingly out of the blue, Smyth also referred several times to matriliny as "matriarchy," a phrase not seen in Pictish studies for 50 years or more.[2] In that context, he accused other Pictish scholars of pursuing "an introspective approach to Pictish studies in their relentless quest for the elusive matriarchy" (Smyth 1984, 57-83).

At first, Smyth's attack on matriliny went nowhere. Anthony Jackson, an anthropologist, pointed out that Smyth's references to matriarchy missed the last half-century of anthropological study and were "utter nonsense" (Jackson 1984, 242).[3] In 1985, David Sellar wrote a devastating critique of Smyth's patrilineal theory, showing that the Leinster king lists relied on by Smyth actually had few brothers and many more sons eventually succeeding fathers; that Smyth had misread Bede; that to make his theory work, Smyth had to invent a British princess for whom there is no evidence; and that Smyth had ignored the teachings of anthropology (Sellar 1985). The esteemed Marjorie Anderson chimed in two years later, showing that Smyth had to rely on a questionable date of accession for Pictish King Tallorcan to give his theory any plausibility; that Northumbrian sources would not be expected to be silent regarding the "puppet king" theory Smyth invented to avoid the "foreign father" pro-matriliny evidence; and that Smyth's arguments were a re-hashing of a hypothesis rejected some fifty years ago (Anderson 1987).[4]

By the 1990s, however, Smyth had gained some converts. Quoting him with approval in The Picts and the Scots, Lloyd and Jennifer Laing repeated Smyth's non-Bedean "exceptional circumstances" phrase, and stated their belief that Pictish matriliny is an unproven theory for which there is "no support in the surviving evidence other than the curious succession pattern" (Laing & Laing 1993, 58-59). A year later, Sutherland concluded, based solely on Smyth's thesis, that the case for matriliny is "not proven" (Sutherland 1994, 62-63). In 1996, Foster hopped on the "exceptional circumstances" adopted approach and, relying on Smyth's thesis, stated that while learned opinion is split, patrilineal succession "seems far more likely" (Foster 1996, 36-37).[5] Other scholars, however, still believe Skene is correct (e.g., Cummins 1995; Henderson 1994; Jackson 1989; Anderson 1987; Thomas 1986). At the end of this millennium, then, the battle-lines stand drawn between Smyth and Skene.[6]

## The Anthropology of Matriliny at the Millennium

The first step in assessing Smyth's theory is a review of the anthropological knowledge that Jackson, Sellar and Anderson accused him of wrongly ignoring. As will be demonstrated below, even beyond what those scholars suggested, that knowledge is indeed crucial to the question of Pictish matriliny.

In 1961, David Schneider and Kathleen Gough published their opus on matrilineal societies, *Matrilineal Kinship*. In his introduction, Schneider set out the "rules" of matriliny which had been developed in the preceding decades, positing that men always retained control over women and children, that women were no better off in matrilineal societies than in patrilineal ones, and that matrilineal societies always functioned under a tension between a man's interest in his sister's children and in his own biological offspring. In short, like his predecessors, Schneider saw a "matrilineal puzzle," and was confused as to how such a system could work in the face of a husband's "natural" authority over his wife and children, and the need for a brother to "control" the children his sisters had by such husbands (Schneider 1961, 1-29).

By the 1970s, the androcentric and ethnocentric assumptions which lay hidden behind the Schneider and Gough compilation were under fierce attack. (Peters 1997, citing sources). In particular, the idea that patriarchy (or even patrilineal descent) was a norm against which other systems should be compared, and that women were always under the strict control of men, was shown to be false by field work conducted among various matrilineal peoples (e.g., Stivens 1996; Lepowsky 1993; Peletz 1988; Flinn 1986). By 1984, Schneider himself had abandoned his kinship theories, admitting that by applying assumptions he did not even know he used, he had misidentified the Yapese society he studied in the 1940s as patrilineal when, in fact, to the extent any labels could be applied to them, the Yapese practised matrilineal descent (Schneider 1984, 81). Field studies showed that matrilineal societies, in fact, functioned quite well, and that it was women and men working together, particularly uterine siblings, who made them work (e.g., Lepowsky 1993; Peletz 1988; Flinn 1986). By the 1990s, it was well-established in anthropological circles that there is no matrilineal "puzzle" at all, unless patriliny and assumptions of universal male authority are used as a lodestone against which everything else is measured, an androcentric premise that has now been utterly discredited (Id.; and Peters 1997; Stivens 1996; Peletz 1995; Scheffler 1991, Schönenberger 1991).[7]

Where has anthropology taken us, then, at the end of this millennium? We now know that matriliny is as stable and functional a system of kinship as patriliny, and that matrilineal societies – both today and historically – are not uncommon.[8] In fact, in some areas of the world, matrilineal succession predominates. We also now have a wealth of reassessed material on matrilineal societies – both historic and current – from which some conclusions can be drawn that are useful in assessing a historical society. Stripping away prior andocentric assumptions, it turns out that "matrilineal forms of organization and ideology give greater social and political space to women" (Peters 1997, 133). Matrilineal societies are now understood not to fit "cookie cutter" molds, instead certain "clusters of characteristics" are seen to exist (Id. 139). Those characteristics include such matters as: 1) elected leadership roles for men based on maternal bloodlines; 2) matrilocal residence patterns with males who marry into their wives' kin and who are often only temporary mates, and female ownership of property; 3) children raised by their matrikin; 4) strong sibling ties; 5) no dowries and little or no bridewealth exchanged upon marriage, with mothers and their daughters often negotiating marriage arrangements, and husbands engaging in brideservice; 5) female autonomy, including choice in sexual partners, active roles in political and family decisions, and active roles in ritual (*Id.*; and Lovett 1997; Stivens 1996; Lepowsky 1993; Schönenberger 1991).

Of particular interest for Pictish studies is the new role played by documentary evidence of historic societies. When reviewing such evidence it is necessary to do so with an eye toward the now

established fact that travelers and patrilineal neighbors often misperceived and/or misrepresented matrilineal societies. In short, because of the biases of who wrote them, a review of documentary sources,

[i]s as much a study in uncovering what has not been said, or has been downplayed by past writers, as it is an endeavor to reinterpret what has been privileged through consignment to the printed page.

(Lovett 1997, 171). In particular, it is crucial to return to source documents, as much as is possible, to avoid the unsupported (often unstated) assumptions contained within earlier analyses of those documents (Id. and Silverblatt 1991, 161). With this new understanding, the mist surrounding the issue of Pictish matrilineal succession begins to clear.

**Why Skene was Right and Smyth is Wrong**

Back in 1867, Skene established what scholars consider the three main pillars of the matrilineal theory:

1) The Irish legend of Pictish matriliny, exemplified by Bede's version of that legend written circa 730, in which he stated the then "well-known" fact that Pictish kings were elected from the female line under a custom that continued to that day;

2) The Pictish King Lists' brother-favoring, non-patrilineal succession pattern; and

3) The historical fact that some Pictish kings had foreign fathers.

This is the evidence that Smyth attacked in 1984, setting forth his critique as follows:

1) The *maqq/meqq* Ogam inscriptions on a few Pictish stones represent a borrowing of the Gaelic term "son of," and list fathers who would be unknown or unimportant in a matrilineal society;

2) There is not one "certain instance" of a mother or sister of a Pictish king being named in the sources;

3) Sons of foreign fathers were likely puppet kings forced on the Picts by stronger neighbours, not sons with rights to the kingship through their Pictish mothers;

4) The multiple successions of brothers and lack of direct father-son succession present in the Pictish King Lists can be as readily explained by kingship rights oscillating between several patrilineal families, as can be seen in the Dark Age Leinster king lists; and

5) Bede's retold legend is merely a legend, and more importantly, Bede only said that the female line was preferred in "exceptional circumstances."

(Smyth 1984: 58-76).

As an initial matter, a flaw immediately surfaces from Smyth's theory. He puts the burden on those proposing matriliny to prove it, without explaining why patriliny should be the default presumption. It should not. Matrilineal and patrilineal societies often exist side-by-side, and it is simply a fundamental error to assume that either form of succession is some kind of norm, even in an area where many patrilineal societies are found (e.g., Peters 1997; Schneider 1984). Rather, it is imperative to assess competing theories on an even playing field. In that light, Smyth's theory fails at the outset because he offers no evidence, contemporary or otherwise, that the Picts were a patrilineal people.

## Meqq/Maqq

The fatal flaw in Smyth's methodology jumps out from his first main argument. Androcentric and ethnocentric assumptions – both stated and unstated – pervade his theory. For example, he assumes that the inscription *meqq* or *maqq* on the Pictish Ogam stones is a cognate for the Q-Celtic (Irish) patrilineal term *mac* meaning "son of" (Smyth 1984, 58). Smyth makes this assumption in the face of a countervailing theory he fails to acknowledge, that the Ogam terms instead are a cognate to the Irish*mocu* or *maccu* meaning "of the tribe of," which is often a*matrilineal* lineage designation (e.g., Ó Cróinín 1995, 36; 42-43; Battaglia 1993, 46; Duncan 1975, 51; MacNeil 1907). This is a crucial piece of unstated evidence because there does not seem to be a compelling reason to assume that *meqq/maqq* is a borrowed Q-Celtic word.

As Smyth himself concedes, the Picts would have had to have their own word for "son" because "even Pictish matriarchs (sic) clearly had male offspring." (Smyth 1984, 58). Because the Picts were likely P-Celtic speakers, the Pictish word for "son" more feasibly would be related to the (P-Celtic) Old Welsh *map* or *ap* (meaning "son of").[9] In fact, it may well be that the term*ipe*, which appears in a Latin inscription on a stone at St. Vigeans following the name Drosten, and between two other names (possibly the mother and father), is the Pictish term for son (e.g. Gray 1996: 11). In that light, it seems more likely that the *meqq/maqq* terms mean something other than the "son of " assumed by Smyth.[10]

Smyth next assumes (again unstated) that if *meqq/maqq* does mean "son of," then the parent of that son listed in the Ogam inscription must be a father, not a mother (Smyth 1984, 58). Even in Irish, a son can be a *mac* of a father *or* a mother. Moreover, despite Smyth's assumption (this one stated) to the contrary, matrilineal societies often keep track of, and give a great deal of importance to, fathers (e.g. Schneider 1984; Witherspoon 1975). Thus, even if the Ogam stones*did* list fathers – which is clearly not the safe assumption Smyth believes it to be – this "fact" would provide no proof against the matrilineal theory, despite Smyth's heavy reliance upon it.[11]

## No "certain" instance in "the sources" of the sister or mother of a Pictish king

Smyth's next argument is also loaded with untenable assumptions. What are "the sources" he is referring to? (Smyth 1984, 58). Because he discounts Bede – usually seen as one of the most reliable sources for 7th and 8th century Britain – Smyth seems only to count the Irish annals and Scottish chronicles as "sources." In his eyes, then, the only "sources" that matter are those written by a patriarchal monk in a structured annalistic account. Such a narrow definition erroneously limits the field (Lovett 1997, 171; Silverblatt 1991, 161-63). This is important, because – as explained below – other sources, like 7th century Irish literature – provide strong evidence of Pictish matriliny.[12]

And what does "certain" mean? Precious little in the area of Dark Age Scottish history is "certain" in the sense Smyth seems to be requiring for Pictish matriliny. If such "certainty" were truly required for this time period, Smyth could not have written his book. Smyth even fails to apply this rule to himself on his assumptions in favor of*patriliny*. For example, he assumes "centuries of intermarriage between Pict and Scot," even though there are no "certain" instances in the sources of Pictish wives of Scots, and no instances at all of out-marrying *virilocal*[13] Pictish wives in the sense Smyth seems to mean it (Smyth 1984, 71).

More troubling is Smyth's assumption that none of the names in a source he does accept – the Pictish King Lists – is the name of a mother or sister. Even before my article suggesting that most of the parental names are mothers' names, Miller had shown that*Diu* from the King Lists was likely the sister of King Dectotr'ic, and the Andersons had established that*Derelei* may well have been the mother of

Kings Bredei and Nechton, and *Domelch* was likely the mother of King Garnart (Miller 1982, 156; M Anderson 1973, 175; A Anderson 1922, 122).[14] As anyone who practices law knows, ignoring evidence does not make it go away. Instead, the sources – even as Smyth limits them – offer evidence of sisters and mothers more certain than much of Smyth's "counter" evidence.

## Puppet Kings

Smyth's next argument is not really evidence at all, but an attempt to explain away the apparent historical fact that at least two Pictish kings had foreign fathers – one a king of the Cumbrian kingdom of Strathclyde, and another a Northumbrian prince exiled in Pictland. In a matrilineal society, sons of foreign fathers would be expected to have no impediment to becoming rulers because their mothers' blood would define their rights. The opposite is true in patrilineal societies, where sons of foreign mothers often rule, but not sons of foreign fathers. In Dark Age Ireland, Cumbria and Northumbria, for example, kings – for the most part – were the sons of former native kings.[15] Indeed, Elizabeth Gray has shown that there is only one instance in Irish heroic literature of the son of a foreigner being named king (Bres in *Cath Maige Tuired*), and that is a cautionary tale to show what a "disaster" such a rule of succession would be (E Gray 1982, 200).

To avoid this obviously strong proof of matriliny among the Picts, Smyth argues that the sons of foreigners in the Pictish King Lists *may not* have had Pictish mothers, and *could* be puppet kings forced upon the Picts (Smyth 1984, 61-70). Anderson and Sellar amply showed the lack of any evidence to support this rank speculation, and the prevailing evidence against it (Sellar 1985; Anderson 1987). In any event, this supposition is clearly not evidence of a normal practice of patriliny among the Picts, under which one would expect the fathers to be Pictish, not Cumbrian or Northumbrian. At most it is a weak attempt to explain away facts which, on their face, support the matrilineal theory.

## Oscillating Kingship

One of Smyth's key pieces of "proof" against matriliny is another attempt to work around facts that otherwise seem to show a practice of matriliny. He argues that brothers could succeed brothers, as often occurs in the Pictish King Lists, in a system of oscillating kingship between competing patrilines – as happened in Dark Age Leinster. In such a system, Smyth opines, sons would not be expected to directly succeed fathers (Smyth, 67-75). As Sellar showed, however, if a man from the second patriline generally succeeds upon the death of a king of the first patriline, brothers would rarely be expected to succeed, as they rarely did in Leinster, but often did in Pictland (Sellar 1985). Smyth's speculation also ignores the anthropological evidence that in a matrilineal society, a series of brothers *would* be expected to succeed because the sibling tie is particularly strong (Peters 1997, 129; Schönenberger 1991, 60; Peletz 1988, 9, 332-39).

Smyth does not even address Skene's second observation about the King Lists, namely that the names on the "son" side of the lists, are radically different from those on the "parent" side of the list. Even assuming, *arguendo*, that the parents listed are fathers, an oscillating kingship would require that sons (or patrilineal grandsons) of former kings eventually succeed, just as occurred in the Leinster lists (Sellar 1985, 39-40). This means that in the Pictish context sons of Drest, Gartnait, Bredei, Talorcan, etc., would appear somewhere on the lists, and that those common male names would surface as often for fathers as for sons. Except for one son of Talorcan very late in the list, that simply does not happen (Skene 1867, ci-ciii; Gray 1996, 7). Once again this is powerful evidence that will not disappear simply because Smyth ignored it.[16] Instead, stripping prior androcentric assumptions away, the simplest explanation is that the names on the parental side, for the most part, are mother's names, leaving nothing but solid evidence of matrilineal succession to be found in the Pictish King Lists, whether or not kingship oscillated between different kin groups (Gray 1996).

## Bede

The cornerstone of Smyth's anti-matriliny theory is his assertion that Bede's contemporaneous explanation of succession among the Picts limits recourse to the mother's line to cases of "exceptional circumstances," and, in any event, that the legend Bede relates to explain the method of succession is, after all, only a legend (Smyth 1984, 60-61). As Sellar pointed out, Smyth ignored the most useful of Bede's language in terms of assessing whether Bede was describing merely a legend or a legendary explanation for a current practice (Sellar 1985, 39). Bede stated that the Pictish practice of choosing kings from the female royal line, as is "well known," (*constat*) "has been preserved among the Picts up to the present day" (*quod usque hodie apud Pictos ... esse servatum*) (Garforth 1988, 29). Smyth does not even acknowledge this language of Bede's, which turns the most important part of Smyth's mere Irish legend into a fact that was "well-known" in the early 8th century.

To understand what Bede actually said, then, it is necessary to return to the source document. Just as Lovett found in her recent studies, earlier translations and interpretations do not tell the whole story (Lovett 1997; *see also* Silverblatt 1991, 161-163). Bede's Latin passage from his *Historia Ecclesiastica* reads as follows:

> Cumque uxores Picti non habentes peterent a Scottis, ea solum condicione dare
> consenserunt ut, ubi res veniret in dubium, magis de feminea regum prosapia quam
> de masculina regem sibi eligerent; quod usque hodie apud Pictos constat esse servatum.

(Garforth 1988: 29).

The first thing that jumps out from Bede's own words is the lack of any phrase comparable to Smyth's "exceptional" circumstances, for which we would expect to see a Latin word like *singularus* or *rarus*. The traditional translations of Bede's Latin likewise use no such overtly limiting phraseology. For example, Colgrave & Mynors (1969, 18) translated Bede's passage as follows:

> As the Picts had no wives, they asked the Irish for some; the latter consented to give them women, only on condition that, in all cases of doubt, they should elect their kings from the female royal line rather than the male; and it is well known that the custom has been observed among the Picts to this day.

The Penguin edition translation (Farmer 1990; Sherley-Price tr. 1955, 46) renders the passage somewhat differently:

> Having no women with them, these Picts asked wives of the Irish, who consented on condition that, when any dispute arose, they should choose a king from the female royal line rather than the male. This custom continues among the Picts to this day.

While both translations are fine on general readability grounds, they fail when it comes to an exact translation of Bede's own words – a task the above authors did not undertake, but one on which Smyth should have focused, given the importance of his reading of Bede to his theory. Carefully parsing Bede, I believe a textual translation is more accurately rendered as follows:

> And when the Picts, who had no wives, sought them from the Irish, they [Irish] agreed to give them only on the condition that, whenever the matter should come into question, they [Picts] choose a king for themselves from the female royal line rather than from the male; it is well known that this [practice] has been preserved among the Picts up to the present day.

The key difference between the above translations is manifest. The traditional readable version can give the sense of a "condition" – *i.e.*, use of the female royal line when choosing kings – that is triggered only in a time of "doubt" or "dispute," with the word *dubium* serving as a qualifier to the condition.

Alternatively, the more textual translation tells of a *matter* (*res*) which, whenever it should come in question (*veniret in dubium*), *always* triggers the condition of looking to the mother's line. And that matter (Bede's *res*) is the choosing (*eligo*) of kings. In short, the translation makes the difference between a preference for the female line in unspecified doubtful circumstances, as opposed to the exclusive use of the female line *whenever* (*ubi*) a king is chosen.[17]

Perhaps because of my legal training, the need to isolate Bede's *res* became clear immediately upon recourse to his actual words.[18] There are two possible choices for the precedent or antecedent to Bede's *res: condicione* (the "condition" of looking to the female royal line) or *regem eligerent* (the "choosing" of a "king"). Because word order is often no help in translating medieval Latin, Bede could conceivably be referring back to "the condition." This seems unlikely, however, because the clause following "whenever the matter" (*ubi res*) requires that something should come into question or doubt (*veniret in dubium*). If it is "the condition" itself that comes in question or doubt, why did Bede tell us in his final clause that it is still preserved among the Picts in his day?

And why did Bede put the verb *venire* in the imperfect subjunctive, a "cause and effect" tense coupled with a qualifier *ubi* ("whenever"), which gives a sense that the matter will arise in the future? If he meant to convey a rare exception, Bede could easily have used a tense – like the present conditional – and words that would have clearly said "*if ever*." But a reading along the lines of "if ever the condition should come in doubt" makes no internal sense because it is that very condition the Irish are imposing upon the choosing of kings among the Picts *in return for* giving them women. Why would the people who insisted on the condition in the first place allow that it could ever be questioned? Or more importantly, why would Bede report an explanatory legend that fails to explain? The traditional translations (and Smyth) do not address this problem because they leave *res* untranslated and missing in action.

If *res*, instead, is addressed head-on and translated as a precedent to the subject "choosing of a king," there is no problem with either the textual meaning or Bede's selected verb tense. Because of the limited nature of human life spans, "the matter" (*res*) of electing a king will arise in the future, again and again, in the cause and effect manner (*i.e.*, "whenever this, then that") anticipated by the imperfect subjunctive tense and *ubi*. In short, having failed to focus precisely on Bede's *res*, the translations which make "the condition" – rather than "the king-choosing process" – conditional upon question or doubt arising, miss "the matter" to which Bede himself was referring.

Indeed, the true failing in those translations is to impose a reading of "doubt" in the first place. The Latin verbal phrase *in dubium venire* is properly translated as "to come in question" (*e.g.*, Traupman 1981 – "*in dubium venire* – to come in question"). Perhaps because as English speakers we have our own words "dubious" and "doubt," the tendency is to render the Latin *dubium* to mean something similar. While, standing alone, *dubium* does mean doubt, when it is teamed with "in," the phrase more generally means "in question" (e.g. Glare 1996). Most importantly, when teamed with the verb *venire* ("to come"), the entire verbal phrase becomes an idiom that gives the sense of an issue (or matter) arising, or *coming* into question. For example, the 1979 Lewis & Short Latin Dictionary translates "*in dubium vocare*" as "to call in question," and compares that phrase with its close linguistic kin, "*venire in dubium*," to come in question.[19] The failure to give meaning to this idiom in its entirety, rather than to *dubium* alone, led Smyth down the primrose path to his own dubious translation of "exceptional circumstances."[20] Properly translated, Bede said nothing about exceptions or doubts. Instead, Bede described the practice of the Picts whenever the normal matter of needing to choose a king arose, *i.e.*, "whenever it should come in question."

Once Bede's *res* is fixed, and the clause in dubium venire given its proper idiomatic meaning, the rest of the translation falls neatly into place. Because *quod* ("this") in Bede's final clause is referring back to the previous clause which describes the condition imposed by the Irish, there is a perfectly fitting translation for the phrase "*apud Pictos ... esse servatum*." While the verb *servo* can mean "observed" in

the sense of "seen," it is more generally used to mean "observed" in the sense of "followed or *preserved*." In particular, the Oxford Latin Dictionary gives a meaning for *servo* of "to keep, observe (a law, custom, practice, etc.)," which fits hand-in-glove with "the condition" of matrilineal succession that Bede reported as "a law, custom or practice." The perfect passive tense of *esse servatum* gives the sense of "has been preserved," which then leaves open the question of preserved by whom? Bede tells us: *apud Pictos* or "among the Picts."[21]

Thus, instead of matrilineal succession that happens in Smyth's theory only in "exceptional circumstances," Bede's actual words speak of a condition of choosing kings from the female line that is triggered "whenever *that* matter (*i.e.*, choosing) comes in question," a practice which was still "preserved among the Picts" in Bede's time. This translation not only gives meaning to Bede's own words, but is also consistent with other corroborating evidence. First, it is virtually certain from later existing versions that Bede's early Irish source (which we do not have), did not contain a qualifier to the factual assertion that the Picts solemnly swore *always* (*co bráth*) to choose their kings from the female line (Wainwright 1955, 26; MacEoin 1964, 152).[22]

Second, as one of my favorite historians often says, "documents are text; history is context."[23] The King List and corresponding history of the Picts provide important context in understanding Bede's words. Pictish kings, it seems, rarely succeeded to their kingship by killing their predecessors (Sellar 1985, 40; Miller 1982, 151-52). This was not true for their patrilineal neighbours. Dark Age Irish and Anglo-Saxon kings, for example, generally succeeded to the throne by killing the immediately prior king, usually in battle (Id.). In the memorable words of James Campbell, "[a] king, no matter how great, was quite likely to end ... slain in battle, with his head and hands stuck up on stakes on the battlefield" (Campbell 1982, 56).

Why was this not true for the Picts – when historically only one king (Drust, in 729) seems to have been killed by his successor? (Sellar 1985, 40). It is unlikely that the Picts were simply a more peace-loving people – indeed there is plenty of evidence of their war-like nature against outsiders. Bede's words, combined with modern anthropological observations, provide the answer. Bede tells us that the Picts "chose" (*eligo*) their kings. While Smyth focused on "succession" in the context of one man having more of a right to the kingship than another, the Picts seem to have had a different concept (Smyth 1984, 60).

In matrilineal societies, the leader is often chosen by a person or persons other than the current leader from all eligible males of the royal bloodline, a process in which women frequently play a role (*e.g.*, Schönenberger 1991, 174-75; Aiyappan & Mahaderan 1990, 23; Brown 1977, 239; Schneider & Gough 1961, 279).[24] In a system in which kings are elected, "the matter" of choosing kings would "come in question" – or be in doubt, for that matter – "*whenever*" (*ubi*) a new king was needed, *e.g.*, through death or deposition, because no one particular person would be expected to succeed. And if no particular male – whether through some sort of *tanistry* or form of hereditary succession – felt he had a "right" to rule, there would naturally have been fewer succession wars. In fact, anthropologists have observed that, when comparing matrilineal societies with their patrilineal neighbors, there is a lower level of in-fighting among the matrilineal groups, with most aggression focused outward (Schönenberger 1991, 172; Divale 1984, 170-71).

Finally, in the context that Bede himself set for his document, there is simply no reason to look for a qualifier to the stated matrilineal condition. As a general introduction to his *Historia*, in Book I, chapter 1, Bede set out to explain the situation of Britain "at the present time" (*haec in praesenti*), giving a paragraph or so about the other three races who lived there – Irish, Picts, Britons – with the rest of the *Historia* to focus, for the most part, on his own English race. It would be strange in that limited context for Bede to have told his readers something about the Picts that rarely happened, instead of providing an explanation for something – in his own words – that was "well known" (*constat*) about them and continued to be true "up to the present day" (*usque hodie*).

On Bede's own words it is clear that *he* believed the Picts were a matrilineal people. It seems unlikely that such a careful – often circumspect – reporter of details would so emphasize the continuing nature of a matter in his time if he was not certain of that fact. Rather, it is much more likely that although Bede's *explanation* for the "well-known," still current fact of Pictish matriliny is mere legend, that *fact* itself is correctly reported. Indeed, Bede tells us as much by leading off his legendary explanation with the qualifier *ut diximus* ("it is said that") – a qualifier which does not follow through to his later, imperative phraseology of *constat... usque hodie* ("it is well-known ... up to the present day") (Garforth 1988, 29).

In other words, just as Bede reported in the same chapter an unlikely legend of *why* the Scots in Britain were in his day (*usque hodie* once again) called by the name *"Dalreudini,"* he correctly reported the fact of the name *"Dál Riadan"* (Id. 30; Laing & Laing 1993, 39). Because the name *Dál Riadan* is so well-attested to in later sources, no one suspects Bede of having gotten this "fact" wrong, even though the explanation he gives for it is legendary. Bede's "fact" of Pictish matriliny is also well-attested to in later sources (e.g., MacEoin 1964). The different treatment of these two Bedean facts – which Bede dealt with in a strikingly similar manner, including making identical word choices – speaks volumes about how androcentric and ethnocentric assumptions have wrongly led to the theory of matriliny being assessed under a higher and more difficult (if not impossible) to meet burden of proof.

On an even playing field, Smyth's "exceptional circumstances" argument – the cornerstone of his anti-matriliny thesis – does not hold water. To paraphrase Smyth himself, reading Bede in such a manner represents "an introspective approach to Pictish studies in a relentless quest for the elusive [universal] [p]atriarchy" (Smyth 1984, 82). Stripping away androcentric projections and returning to the source document, Bede's matrilineal condition is properly understood as applying whenever the matter of electing a king comes in question; *i.e.*, "whenever the 'throne' becomes vacant," or as the later Irish sources tell us, "always" (Henderson 1967, 31; MacEoin 1964, 152). In short, Smyth's patrilineal thesis is a failure on all fronts. Instead, as Skene correctly reported 100 years ago, the only evidence we have about Pictish succession customs clearly supports the theory of matriliny.

## "New" Evidence of Pictish Matriliny

As a final matter, I will now turn to two pieces of previously slighted evidence of Pictish matriliny that take on new importance in the light of the teachings of current anthropology.[25] As noted by Peters and Lovett, carefully read, travelers tales can give useful information about matrilineal societies (Peters 1997; Lovett 1997). Early Irish literature gives us many such tales, including the intriguing late 7th century versions of *Conall Corc and the Corco Luigde* and *Tochmarc Emire*.

The earliest (7th century) version of *Conall Corc* tells of Conall being exiled to Pictland, where he marries the king's daughter, has three sons by her, and then leaves for Ireland, never to return. This is clearly a story of a temporary marriage of an Irish man with a Pictish woman, who keeps their children as part of her matrilineage. Interestingly, the Irish scribe never calls the king the "father" of Conall Corc's wife, and the Irish term for daughter, *ingen*, can mean daughter or maiden (Hull 1947, 895; Ó Dónaill 1992). Moreover, the daughter's mother is called the "mother of Conall's wife," *not* the generally used Latin phrase *uxor* ("wife") or "queen of the king." (*Id.* 896; and Patterson 1994, 300). In matrilineal societies, a maternal uncle relates to his sister's daughter as the heir who will carry his bloodline into the future, a relationship often seen as more important than that of father/child (*e.g.* Peters 1997, 129-30). Thus, a "sister's daughter" could easily have been mistaken for a daughter (*ingen*) by a patrilineal observer.[26] Given the carefully non-specific terms used by the 7th-century scribe, it seems likely that the king, his daughter and her mother of Vernon Hull's English translation, were instead the king, his sister and her daughter. This is particularly so in light of the noted tendency of patrilineal travelers – and translators – to misconvey or mistranslate matrilineal affiliation terms (*e.g.*, Schneider 1984, 74-92; McFeat 1966, 49-57).

In any event, the most fascinating part of the tale is the old Irish dialogue of what happens when Conall Core decides to leave Pictland. First the mother, then the king, then Conall's wife pledge him to stay for an extra year apiece. The first negotiation takes place between the mother and Conall, who in an obscure phrase, promises not to abandon *her* (Hull 1947, 896). Although translator Vernon Hull found this passage difficult to understand, it makes perfect sense in light of the anthropological evidence that mothers in matrilineal societies often negotiate their daughter's marriage contracts, and that sons-in-law perform brideservice for their mothers-in law while living matrilocally with their wives' kin (e.g., Brantley 1997, 153; Peletz 1994, 143; Schönenberger 1991, 168; and Brown 1977, 241). Moreover, there is no mention of a dowry or brideprice in this tale, a lack which would be expected where a husband performs brideservice. It is not until the later 9th century version that mention of a dowry is shoe-horned into the story, and the mother is edited out (Hull 1941, 1949). All-in-all, the earliest "Conall Corc" tale gives much evidence of precisely what a traveler would be expected to report about a 7th-century matrilocal, matrilineal Pictish society.

One of the *remscéla* of the Cúchulainn saga, *Tochmarc Emire* ("The Wooing of Emer") is even more enlightening. This tale has the hero Cúchulainn traveling to Pictland[27] where he is to be trained in arms by the warrior-woman Scathach. In a nutshell, Scathach's daughter, Uathach, sees Cúchulainn and decides to bed him. Her mother approves. No men are involved in this decision to take a mate (other than Cúchulainn himself, who readily acquiesces). The dun where Scathach and her children live is obviously Scathach's property. After their initial tryst, Uathach secretly instructs Cúchulainn how to arrange a temporary marriage with her, including how to avoid having to pay a brideprice.[28] Cúchulainn then kills one of Scathach's men and offers to serve in his stead in what looks suspiciously like "brideservice."[29] Scathach's children – both sons and daughter – are referred to with matronymics like *"mac* Scathach," as are the sons of the other women in the story, Ilsuanach and Eiss Enchend.[30] After tricking the warrior-woman Aiffe, Cúchulainn sleeps with her outside her dun, again with the choice made totally between Aiffe and Cúchulainn, not by any male kin of Aiffe. Cúchulainn then returns to Emer in Ireland – the woman he intends to make his permanent bride – and leaves both Uathach and Aiffe behind, with Aiffe keeping their child to be raised among her kin (Meyer 1890, 446-53; N. Chadwick 1958, 77-79).

While most scholars have rejected the long-cherished notion that the Cúchulainn saga is "a window into the Iron Age," it is well-accepted that such stories can provide useful information about the time in which they were written down (*e.g.* Ó Cróinín 1995, 45; W. Miller 1990, 44-51). As explained by Wormald in discussing *Beowulf*, such literature can tell us much about its own time "precisely because it is not primarily concerned to tell us anything" (Wormald 1978, 35). In this one 7th century story about an Irishman's trip to Pictland we find the elements of brideservice instead of brideprice, matrilocal residence; temporary marriage arranged by women; children raised by matrikin with no patronymics, property owned by women, and female autonomy, including in sexual choices.

It would be difficult to make up a story with more of the "clusters of characteristics" that modern anthropology tells us to look for in a matrilineal society than those found in *Tochmarc Emire* (Peters 1997, 139). Does it really make sense to simply assume that a patriarchal monk used only his imagination to so hit the mark in describing a matrilineal society? Or instead, do these Irish stories of Pictland give us a glimpse, albeit a skewed one, of the same matrilineal Picts described by Bede half a century later? The answer seems obvious. Now that anthropology has helped us know what to look for, the evidence of Pictish matriliny – both inside and out of the sources Smyth would erroneously hold us to – is suddenly quite compelling.

CONCLUSION

In summary, Smyth offers no evidence of patriliny among the Picts, and his attack on the traditionally accepted evidence of matriliny fails to convince. In the light of the new teachings of anthropology, including a textual translation of Bede, not only does the traditional evidence take on greater weight, but other evidence adds to the growing stack of proof that the Picts were a matrilineal people. It is to be hoped that we can now enter the new millennium with this old question stamped "solved," free to turn to new issues that may finally lead to a better understanding of the Picts, who Isabel Henderson rightly called "quite the darkest of the peoples of Dark Age Britain" (Henderson 1967, 12).

NOTES

[1] The one dissenting voice on Pictish matriliny in the earlier period was John Fraser. In an attempt to show that the Picts spoke an Indo-European language, Fraser suggested in 1927 that the theory of Pictish matriliny (what he called "matriarchy") relied upon Bede and the King Lists, which he attempted to explain away (Fraser 1927). Fraser's arguments persuaded few, if any, of his contemporaries in the field of Pictish studies. Rather, from the 1930s through the early 1980s, every major work on the Picts assumed matrilineal succession as given (Sellar 1985, citing sources).

[2] In the 1890s, German scholar Heinrich Zimmer used Skene's evidence of Pictish matriliny as proof of his own belief that all human societies follow a "Darwinian" evolutionary path from "primitive promiscuity" to "matriarchy" to "patriarchy" (Zimmer 1894). From the 1930s onward, anthropologists attacked this societal evolutionary theory so successfully that it is no longer believed to have any credence (e.g., Peters 1997, citing sources). While Skene flirted with that theory, it is his textual evidence that has always formed the backbone of the theory of Pictish matriliny, particularly in the post-Wainwright era.

[3] Jackson himself has been the subject of sharp criticism on various issues (e.g., Driscoll, 1984). Driscoll discussed Smyth's thesis briefly in his review of Jackson's *The Symbol Stones of Scotland*, calling the issue of Pictish matriliny an "open" one. (Id., 61).

[4] Anderson was referring to Fraser's 1927 article, discussed above in Note 1 (Anderson 1987, 10). Despite the striking similarities between their anti-matriliny hypotheses, right down to reliance on the same Leinster king list, Smyth does not cite to Fraser in his list of suggested further reading.

[5] Because they do not tell us in any detail, it is difficult to asses why Smyth's converts find his hypothesis persuasive. My own surmise is that allegiance to the only modern "anti-matriliny" theory has as much to do with the peculiarities of academic politics as anything else. I suggest this because of the way Ronald Hutton's book, *The Pagan Religions of the Ancient British Isles*, tied the issue of Pictish matriliny to his attack on the work of the late UCLA archeologist, Marija Gimbutas, and her followers (Hutton 1991). Hutton called Gimbutas' theory that sky god-worshipping Indo-Europeans destroyed an earlier goddess-worshipping "Old European" culture "a radical feminist tract" based on the discarded Victorian-era theory of an "evolutionary matriarchy" (*Id*. 39-44, 346). In attacking Gimbutas, Hutton stated – with no citation to Smyth's many critics – that Smyth had "shattered" the theory of Pictish matriliny, and suggested that anyone who failed to agree must be in the pro-Gimbutas camp (*Id*. 149, 339). That is a tag some scholars (although by no means all) clearly want to avoid. *See, e.g.*, Billington & Green (1996, 1), wherein the authors apparently felt compelled to guarantee that their book, *The Concept of the Goddess*, contains contributions that "have not been biased by specifically *feminist attitudes* but are based entirely upon observations resulting from research" (emphasis added). Given the *apologia* being made by anthropologists and archeologists at basically the same time for the masculinist attitudes that

had so biased earlier studies in those fields, the irony of this controversy is obvious (see note 7, below and accompanying text). I take no sides in the Gimbutas debate, but wish merely to point out the effect I believe it has had on the issue of Pictish matriliny, particularly since it was only after publication of Hutton's book in 1991 (and the publication of Gimbutas' *The Civilization of the Goddess* in that same year) that Smyth seems to have begun to gain converts.

[6] When I presented this paper at the 11th International Congress of Celtic Studies, I was unaware of Alex Woolf's paper on Pictish matriliny that had appeared in the Innes Review in late 1998 (Woolf 1998). He graciously gave me a copy. Although discarding some of Smyth's least persuasive arguments, when he wrote his paper, Dr. Woolf was firmly in Smyth's anti-matriliny camp. Where his arguments differ significantly from Smyth's, I have attempted to deal with them briefly in the body of this article.

[7] During this same period of the 1980s and 1990s, archeologists joined anthropologists in critiquing and rejecting the "projection of our own androcentric notions into the past" (Driscoll 1984, 60). The need to "engender" the past and avoid androcentric assumptions has remained an ongoing issue in both anthropology and archeology. (*e.g.*, Sanday & Goodenough 1990; Gero & Conkey 1991; di Leonardo 1991; Schönenberger 1991, Gilchrist 1994; Wright 1996; Classen & Joyce 1997; Peters 1997)

[8] The anti-matriliny theories which rely on the "rarity" of matrilineal succession fail to deal with this clear anthropological evidence to the contrary (*e.g.* Woolf 1998: 164).

[9] John Koch believes the term "mapon" found in *Y Gododdin* and The Annals of Ulster may be a Pictish kinship term meaning "young son of" (Koch 1997, 167).

[10] Because I am not a Celtic linguist, I hesitate to suggest the following. However, at the risk of saying something foolish, I would like to point out that P-Celtic modern Welsh has a series of words with an apparent root *mag* – meaning "nurture" – that may lend support to the theory that there is a matrilineal lineage meaning behind the *meqq/maqq* inscriptions. In his 6th-century "Life of Columba," Adamnán used the Latin term *nutricius* for King Bridei's advisor/magus Broichan (Book II, 33). This could be a term for Bridei's maternal uncle, the senior male of his matrilineage – the mother's bloodline in which he was *nurtured*. In other words, *nutricius* (nurturer) may be a Latin cognate chosen by Adamnán for a P-Celtic tribal affiliation term related to *maqq* on the Pictish Ogam stones, both with the same root of "nurture." (*Cf.* Modern Welsh *magu*, meaning "to breed, to nurse") (Evans & Thomas 1989).

[11] *Cf.* The lists of mothers kept by scribes in *patrilineal* Ireland (*e.g.*, Ó Rian 1985, 722.1 ff; and Dobbs 1930).

[12] Smyth himself relies on just such literary sources, *e.g.*, the 6th-century poem *Y Gododdin*, and the 14th-century poem, *The Prophecy of Berchán*, in support of some of his theories about Dark Age Scotland (Smyth 1984, 65, 182-84; 190-92). It seems it is only when the issue is matriliny, that Smyth so limits his term "sources."

[13] "Virilocal" is the term used by anthropologists to describe a husband and wife living with the husband's kin, as opposed to "uxorilocal" or "matrilocal," which describe the situation, common in matriliny, where the wife and husband live with the wife's kin.

[14] Woolf, relying on a forthcoming paper by Thomas Owen Clancy, provided further proof that Derile was indeed the mother of Nechton and Bridei, and that their father was "a Cenél Comgaill dynast" named Dairgart (Woolf 1998,149-50).

[15] The old Irish law tract *Cóic Conairi Fuigill* specifically provides that a potential king "must be the son of a king," and preferably the (paternal) grandson of a king as well (Ó Cróinín 1995, 70). Woolf has

shown that while the majority of Irish and Anglo-Saxon kings of the relevant period were the sons of kings, some were not (Woolf 1998). However, none of his examples had a father who was a foreigner in the sense Bili (a Cumbrian), Eanfrith (an Anglo-Saxon) and Dairgart (an Irishman), would have been foreigners to the Picts. As Bede explained, there were four peoples in Dark Age Britain – the English, British, Irish and Picts (*E.H.* I.1). While sons from different kingdoms of those various peoples became kings where their fathers had not ruled before them, as far as I am aware there is no evidence of the son of an Anglo-Saxon father (as opposed to mother) becoming king in any of the Irish or British kingdoms, of the son of an Irish father becoming a king in a British or Anglo-Saxon kingdom, or of the son of a British father becoming a king in an Irish or Anglo-Saxon kingship. Pictish succession, then, was truly distinct from the practices of their neighbors in a way Woolf's evidence cannot explain, but in a manner perfectly consistent with matrilineal succession. The same is true for Woolf's Gwynedd evidence. However, it should be pointed out that some scholars have suggested that the early succession pattern in Gwynedd is actually evidence that, like the Picts, the northern Welsh practiced matrilineal succession in the early Dark Ages, with remnants of that custom being visible in the 9th-century succession pattern detailed by Woolf (*e.g.* Ganz 1976, 16; Chadwick 1958, 64-67).

[16] Woolf's discussion of the king lists also fails to come to grips with this naming pattern evidence (Woolf 1998).

[17] Special thanks to David Van Amburgh, Ph.D. candidate in Roman History at the University of Michigan, for his invaluable assistance with the translation of Bede's Latin.

[18] Lawyers – on both sides of the Atlantic – are constantly required to determine what the *res* is when dealing with such hoary common law doctrines as *res judicata* (the matter has been adjudicated); *res ipsa loquiter* (the matter speaks for itself) and in *rem* jurisdiction (adjudicatory authority based solely on what and where the *res* is).

[19] Historic Latin texts such as Livy's *History of Rome* use the clause *in dubium venire* in the idiomatic connotation of a question arising. For example the phrase "*summan pecuniae quantum aequum esset promitti, ueniebat in dubium,*" refers to the matter having come in question of what sum would be fair to be assessed against a certain wrongdoer, with that question then being referred to the Senate (Livy, 3.13).

[20] Woolf translates the phrase "when the issue might become uncertain" (Woolf 1998, 147). This rendering suffers from the same failings discussed above. Because Woolf relies on "uncertainty" to prove his thesis, I believe it fails on Bede's own words. I wholeheartedly agree with Dr. Woolf, however, that "Bede was not a stupid man," and that because his is contemporaneous evidence, it is vital that Bede's actual words be given meaning (*Id.*, 153).

[21] The alternative "in the country of" translation for *apud* given by the Oxford Latin Dictionary may actually best fit Bede's own evidence. Bede never used the term *Pictavia*, which later became a Latin shorthand for the place where the Picts live(d). He did talk at one point in the *Historia Ecclesiastica*, however, about southern and northern Picts – which, *inter alia*, has led some (including Smyth and this author) to argue that there was no single Pictish kingdom, *per se*, but at least two, maybe more, possibly with an "overking" for some periods of time (Bede III.4; Bannerman 1997, 52; Gray 1998, 30; Smyth 1984, 68-75). If that is true, Bede may have used *apud Pictos* to convey that the practice of matrilineal succession "has been preserved in the country of the Picts up to the present day" because he lacked an easier word choice like *in Pictavia*, and found himself needing instead to resort to the passive voice to convey that all the Picts, north and south, followed the custom of matrilineal succession.

[22] MacEoin lays out all the various versions of the story that the Picts received women from the Irish, including one – perhaps the oldest – which has the Picts stealing women from the Irish, but makes no

mention that they did not have women of their own. While the later Irish versions seem to be "propaganda pieces" aimed at showing an Irish right to Pictish territory, the underlying core – that the Picts practiced matrilineal succession – remains fairly constant. MacEoin showed that Bede's version was likely based on an Irish tale that began to be told around 600 C.E., approximately the same time that Áedán mac Gabhráin was engaging in various attempts to gain Pictish territory, including an Irish *Dál Riadan* victory over the Pictish *Miathi* (MacEoin 1964, 153).

[23] Personal conversations with Dr. David Emmons, author of *The Butte Irish: Class and Ethnicity in an American Mining Town*, 1875-1925 (Univ. of Illinois Press 1990).

[24] Scottish folklore may contain a remnant of just such a Pictish system. As Stuart McHardy has explained, there is an oral tradition that Pictish kings were elected inside the stone circle at Granish Moor, and that wise women played a role in this. (McHardy 1994, 34). This may be related to the story in Wyntoun's Chronicle, which has "[t]hre werd sisteris" tell Macbeth that he will be king, a tradition later borrowed by Shakespeare, who has his three "weird sisters" choose Macbeth as the next king of Scotland (Amours 1914, 274).

[25] I presented other slighted or unnoticed evidence of Pictish matriliny at the 1998 Pictish Arts Society conference, and will not repeat that evidence here (Gray 1998).

[26] It has been suggested that the Old Irish *ingen*, from the proto form *\*eni-gena*, means "born within," a possible matrilineal term for a daughter who is born within the matrilineage and transmits that lineage into the future (Battaglia 1993, 46). Old Irish had two terms for nephew *–nia* and *gormac* – which were designations solely for "sisters' sons," not "bothers' sons" (*e.g.*, Charles-Edwards 1971, 107).

[27] Cúchulainn is said to travel to the "east of Alba," which can only be Pictland (Meyer 1890, 445). The other kingdoms in the east of Britain were English, and there seems to be nothing Germanic about the story of Scathach and her *dun*.

[28] Kuno Meyer's translation actually says "dowry." However, the Irish term used was *tindscra* (Meyer 1890, 448-49). *Tindscra* is an old term made obsolete by the later *coibche*, which came to mean dowry, and is clearly a word for payment made by the groom's family to the bride's family – in anthropological terms, "bridewealth" or "brideprice" (Ó Corráin 1985, 16-17). The context and word used make it clear that by giving us the story of Uathach's secret instructions to Cúchulainn, the scribe was explaining why Cúchulainn did not have to pay a brideprice, something unseemly in 7th century patrilineal Ireland. For example, the 7th-century portion of the *Cáin Adomnáin* sets a large fine for the transgression of making a woman pregnant "without bride-price" (Márkus 1997, 22). Indeed, the Irish scribe of *Tochmarc Emire* would likely have known that in his country, brideprice was paid by a husband even in entering the secondary, transitory unions recognized by the Irish law codes (Patterson 1994, 297). In matrilineal societies, to the contrary, bridewealth is rarely given where matrilocal residence and brideservice are practiced (e.g., Lovett 1997, 174-76; Brantley 1997, 153; Peletz 1994, 143).

[29] The story of Cúchulainn killing a man in Scathach's household whose relationship to her is not at all clear, looks like another scribal insert to explain something which would have seemed particularly strange to a 7th century monk, the fact that after making a temporary marriage with Uathach, Cúchulainn enters the service of her mother. Such an arrangement would not be the least surprising to modern anthropologists, who have established that a tradition of "brideservice" instead of exchange of "bridewealth" is a hallmark of particularly thriving matrilineal societies (*e.g.*, Lovett 1997, 174-76; Brantley 1997, 153; Peletz 1994, 143).

[30] Interestingly, the side story of Eiss Enchend may contain a piece of a buried Pictish myth. Eiss Enchend literally means "the bird-headed one" in Old Irish. After Cúchulainn kills her sons, Eiss Enchend seeks

revenge. Apparently shape-shifting into the disguise of a blind old woman, she tries to push Cúchulainn off a cliff, but Cúchulainn uses tricks taught to him earlier by Scathach to kill her instead. There are bird-headed figures on several Pictish stones, including Murthly and Papil, which could represent this Pictish shape-shifter (Allen & Anderson 1903; reprint 1993, 11, 305-06).

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From ST Vigeans No 1
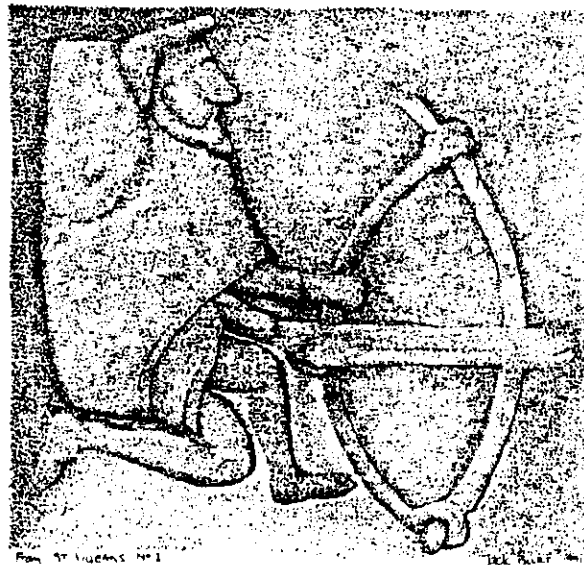
# The Breck of Hillwell Stone

## Alastair Mack

The Breck of Hillwell stone is to be found in Shetland Museum Library, Lower Hillhead, Lerwick (Exhibit No A59/1977)

This irregularly-shaped sandstone slab was found in 1997 in use as a field drain-cover at HU 372 146, at Breck of Hillwell, Quendale, Dunrossness Parish, South Mainland, Shetland. The slab, which is at present displayed upright at the back of a well-lit glass-fronted case, is slightly narrower at the top than at the base. The top is apparently broken; the base is natural.

Incised on the stone's lower half are the remains of what may have been two symbols. The upper is the left half and centre of a *crescent*. Just below this is a horizontal line and further down is a shorter line parallel to it. These may be parts of a *rectangle*.

In the *crescent*'s left wing is a low convex arc that rests on the lower arch and contains eleven strokes of hatching. Curling downwards from the upper arch is a spiral-ended whorl that hooks towards the *crescent*'s centre. This also contains some hatching. There may have been an opposed whorl on the right of the *crescent*'s centre. A faint spiral is just visible.

There is also what may be hatching in the possible. Below the left end of the upper line are the remains of three strokes like the three right-hand strokes of an M, and perhaps some of an arc to the right of these. Above the lower line is part of an upwards-curling arc with what may be two strokes between it and the line and a possible arc to the right. The rest of both these symbols is flaked away.

The broad end appears to have been the base of the slab. If the stone was erected with the narrow end as its base, the symbols would be upside-down. It is quite clear that the *crescent* is not part of a *double-crescent*, which would be reversible; the surface of the stone above the *crescent* is not flaked and is plainly without incisions. If the stone was erected with the broad end in the ground, most of the *rectangle* would be hidden. The distance between the lower line and the base of the slab is only c. 12 cm. This stone must have been a prone slab. The only other known Shetland symbol stone, the lost Sandness stone (Allen and Anderson 1903, 4), appears to have been an upright stone, but there are other northern stones which almost certainly lay prone. **Dunrobin 2**, the Dairy Park stone (Close-Brooks 1980) could not have stood upright, and the same may be true of **Dunrobin 1** (Allen and Anderson 1903, 42.)



Fig.1. Rough sketch of **Breck of Hillwell**, Quendale (Exhibit No.A59/1997).

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**Breck of Hillwell** is like **Sandness** in at least one respect; if its lower symbols is a rectangle, the two Shetland stones have (or had) a symbol in common. As for the *crescent*, which is quite certainly rod-less, it is although incomplete almost as good an example as the rod-less Daviot crescent (Allen and Anderson 1903, 161-2). Of the other possible Class 1 crescents without V-rods, the *'crescents'* on Dunnicaer 3 (Allen and Anderson 1903, 201; Mack 1997.fig 23, ) and Roskeen, (Allen and Anderson 1903, 30-1) are dubious.

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# The Sacred Tree

## J. Phillip Joss

Much has been written about the significance of the V-rod and Z-rod symbols. They undoubtedly look arrow-like or spear-like, with their (usually) pointed and flight-like extremities. I believe that the Pictish artists/sculptors intended the 'uninitiated' to see the rods as such. I am convinced, however, that they represent something on a much larger scale, and are stylised trees. Joseph Anderson (Allen and Anderson 1903, xxxv) briefly refers to this possible interpretation when he wrote of the serpent and rod symbol at Logierait "one of its ends appears as if producing buds or leaves. This seems to suggest that the idea underlying the representation of the rod was that of a living and leaf-bearing or blossoming branch or tree"

I see the rod symbol composed of, in its simplest for, three parts and in the more ornate form, four or five parts. The 'flight' end is the root system, mainly composed of a taproot and two lateral roots (figures 2 and 5) by adding 'webs' this element is disguised (figures 3, 6 and 7). In many cases the 'roots' are less obvious, but still have a symbolic 'ground anchor' or 'taproot' appearance (figures 1 &4). The 'arrow-head' end depicts the symbolic 'leader' or growing tip; the so-called 'curlicues' are symbolic branches or leaves, and the rod itself is the trunk. In Z-rod form, the 'root' end is mostly but not always at the bottom or in the direction of the ground. The significance of the V and Z rod bends eludes me, apart from the obvious comparisons with votive deposits of ritually 'broken' weapons etc.
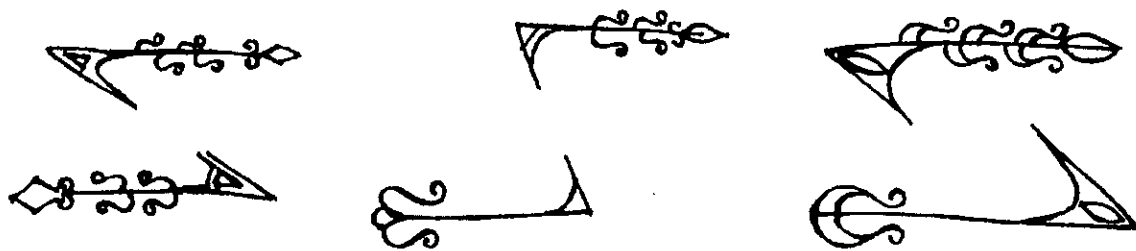


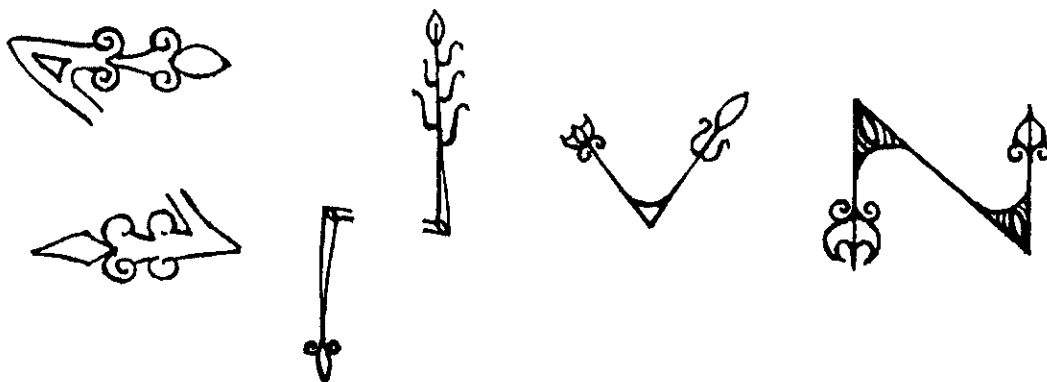Fig.1-3. (left to right) **Fourdon**, ECMS 202, **Kintore 2**, ECMS 173, **Dunnichen**, ECMS 206,



Fig.4-7. (left to right) **St.Vigeans 5**, ECMS 241, **Plaque from Norrie's Law**, ECMS 369, **Rhynie 6**, ECMS 184, **Arndilly**, ECMS 151,

We know that trees were sacred in Celtic ritual. In the case of the double disc, crescent and notched rectangle forms, I feel that their perceived associations with the 'otherworld' is the logical connection. Their roots were linked to the 'otherworld' and their branches reached to the sky. Stuart Kermack (1996) quoted Watson "A tree was the door to the Celtic afterworld". Trees have been found inside ritual shafts (Piggot 1968, 74; Eluere 1993, p. 104; Green 1986, 133). Miranda Jane Green states "the Goloring in Germany was a huge enclosure built in the sixth century B.C., at its centre was an enormous wooden post, forty feet (twelve metres) high, perhaps symbolic of a sacred tree".



Fig.8. **Gundestrup Cauldron** (detail), © J.Phillip Joss

On the Gundestrup cauldron, the stylised tree appearing to be carried on the spears of warriors (figure 8) is, to my mind, the most convincing clue to the V-rod, Z-rod enigma. The elements existing in the Pictish rods are all there — the three pronged root, the trunk, the leaves or branches and the 'leader'. This parade in my view represents the death/rebirth cycle where warriors in death, via the sacred tree, reach the otherworld, are resurrected in the cauldron of rebirth, and sally forth again to do battle led by the ram-headed serpent.

The vesica-shaped ornaments which are often depicted in the bends of the Z and V-rods (figures 3, 7) may be the "passports to Paradise" or samaras written about by Stuart Kermack.

Two cave symbols (figures 9 and 10), although devoid of the triple root/flight element, still in my view have a convincing "tree" appearance.

Two "straight" Z-rod types are known and a few have only very slight bends at 'tip' and 'root' (St. Vigeans no. 2: Allen and Anderson 1903, 239; Kirriemuir 2: Foster 1996, 55).

The Z-rod in conjunction with the serpent may have a different significance, such as the "Tree of Knowledge."
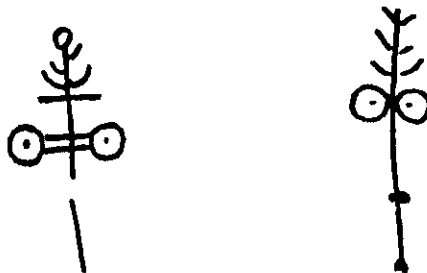


Fig.9-10. Pictish Symbols at **Court Cave, East Wemyss**

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# The Tummel Bridge Hoard

## Craig Cessford

In the late nineteenth century a group of three silver brooches and fragments from a bronze hanging bowl and cup were discovered at Tummel Bridge, Perthshire. Little is known about the circumstances of the discovery but the objects are mainly of Pictish manufacture and were probably deposited in the late seventh or early eighth century. They are likely to have been intended for recycling and may indicate the presence of a contemporary settlement site where metalworking occurred.

The hoard was discovered at Tummel Bridge in Dull parish (NN762 591) on the river Tummel at the western end of Loch Tummel, between it and Loch Rannoch. It is one of the smallest and least studied of the known Pictish hoards of metalwork: for example, it is not mentioned in the most recent general work on the Picts (Foster, 1996) or in the index to a recent bibliography of Pictish Studies (Burt, 1995). This omission is largely due to the fact that the objects from Tummel Bridge are quite plain and undecorated: the study of Pictish art has tended to focus upon stylistic and decorative elements and ignore plainer objects

## DISCOVERY

The hoard is first recorded among the recent purchases listed in the *Proceedings of the Society of Antiquaries of Scotland* (1887-88), meaning that it was acquiredby the National Museum of Scotland between the 30th November 1887 and the 23rd April, 1888. The entry (Anon, 1888, 268) reads:

> Three Silver Brooches, and Fragments of other objects of bronze found with them at Tummel Bridge, Perthshire. The Brooches are of the penannular form with expanded ends ... the most entire of the three, measuring 27/8 inches diameter, the pin extending to 41/2 inches in length. Of the two other brooches, one is slightly larger and the other a little smaller. Both want the punctuated ornamentation on the margin of the expanded terminal parts of the ring, but the larger one has the pin so ornamented. Among the fragments of bronze objects found with them are portions of the rims of two bronze dishes, one of which must have been of pretty large size, and a circular object, which may have been a harness ornament. They were found some years ago in the earth under the roots of a tree which had blown down.

This report is the primary published record of the discovery and encapsulates almost all we know about the hoard.[1] The items are also listed in the edition of the Catalogue of the National Museum of Antiquities of Scotland published in 1892. The entry in the catalogue reads (Anon 1892, 208):

> **FC 162-170.** Collection from Tummel Bridge, viz.- (162-164) silver brooches formed of round rings with expanded flattened ends, one ornamented on flattened ends by two rows of dots round the edges, another with two rows of dots on pin, the third plain; (165-170) fragments of bronze vessels, &c. Purchased 1888.

The finds languished in relative obscurity, for example being ignored by J Romilly Allen in his work on hanging bowls (1898, 50), until featured in E.T. Leeds' classic work *Celtic Ornament down to A.D. 700* (1933, 145, Fig. 37).

CATALOGUE

The material from Tummel Bridge consists of nine elements:

1) Silver type H or Hb penannular brooch (Fig. 1). The edges of the terminals of this brooch have simple punched decoration made by using a two pronged instrument. The ends of both terminals are broken, the right worse than the left, and the left terminal is also cracked. Diameter of ring: 7.5cm. Pin 12 cm long [FC 162].

Fig.1.

2CM
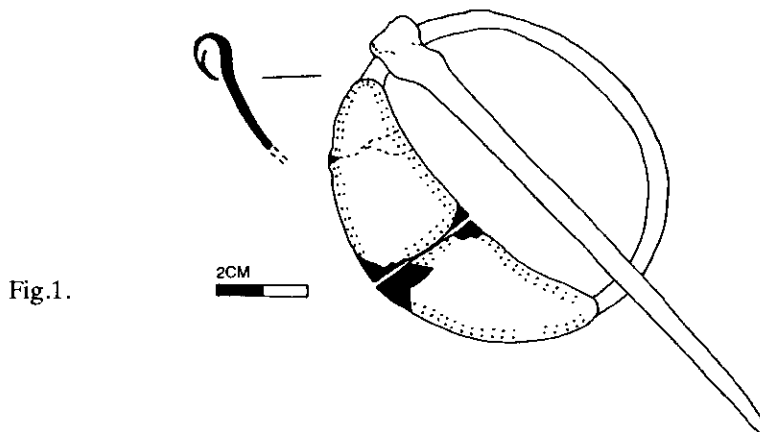
2) Silver penannular brooch (Fig. 2), same type as 1). The terminals are undecorated but there is punched decoration on the pin head and its widened centre similar to that on the other brooch. The end of the right hand terminal is quite badly broken. Diameter of ring 7.8 cm. Pin 12 cm long [FC 163].

Fig.2.

2CM
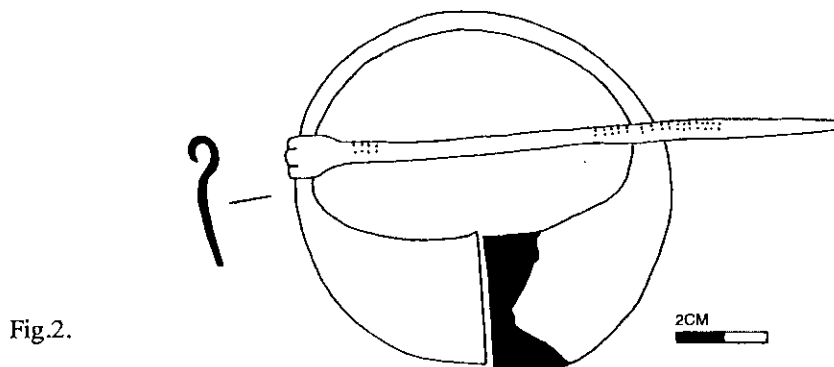
3) Silver penannular brooch (Fig. 3), same type as 1) but not decorated. The end of the right hand terminal is broken and both terminals are cracked. Diameter of ring 6.6 cm. Pin 9.1 cm long [FC 164].

Fig.3.

2CM
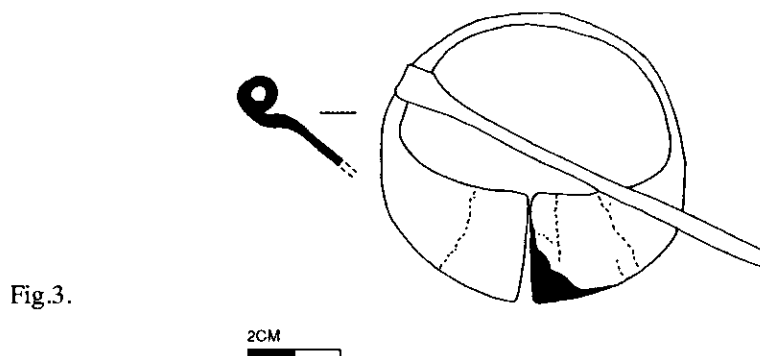
4) Hanging bowl escutcheon (Fig. 4), described as a 'harness ornament' in the original report, decorated with a simple four peltae openwork design. Only the lower third of the escutcheon remains, diameter 4.2 cm [FC 168].

5) Rim and body fragment of a bronze hanging bowl (Fig. 4), 17.6 cm around the rim by 5 cm vertically. The rim is of the developed inturned variety and there is a mark on the fragment where an escutcheon the same size as item 4 would have been attached [FC 166].
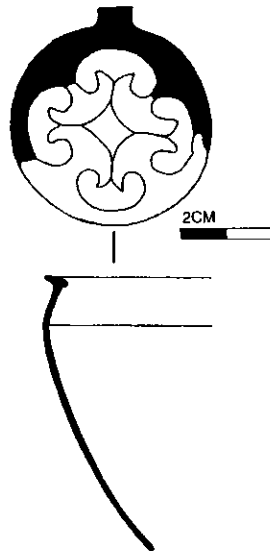
Fig.4.

6 and 7) Two smaller rim fragments from a hanging bowl (Fig. 5). These are 5.3 cm and 6.2 cm around the rim by 0.6 cm vertically [FC 169/170].

8) Base fragment of hanging bowl, 2.9 cm by 6.4 cm [FC 165].

9) Rim and body fragment of a smaller bronze vessel with stepped side (Fig. 6), sometimes described as a cup; 2.5 cm around the rim by 2.3 cm vertically.
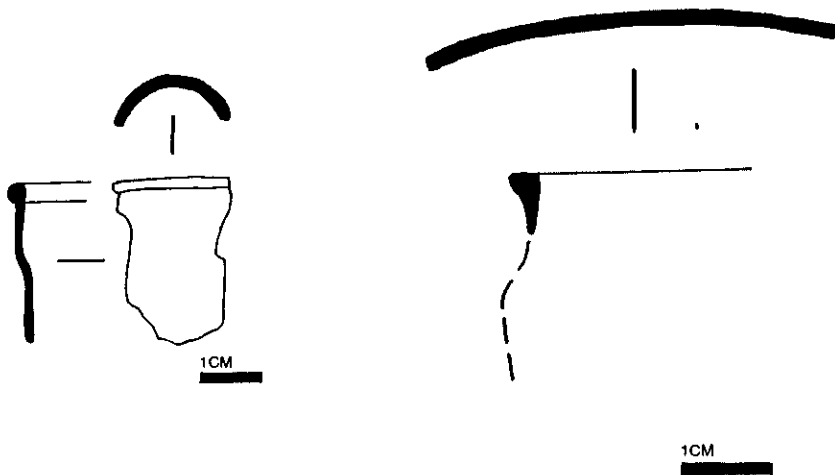
Fig.5.                                    Fig.6.

This material can be split into two groups: the three brooches and the five vessel fragments plus escutcheon, which probably come from only two vessels. It is impossible to say whether or not this represents the entire original assemblage, although the recovery of the relatively small and nondescript vessel fragments suggests that no disturbed material was overlooked. Although it is possible that other material lay deeper and was not uprooted by the falling tree, there is no evidence of any other silver finds being melted down as happened with many nineteenth-century discoveries, such as the Norrie's Law hoard, Fife (Graham-Campbell 1991; Laing 1994).

## The Brooches (Figs. 1-3)

The three silver penannular brooches are all very similar and belong to Fowler's group H (1964, 109-11). Type H penannular brooches are distinguished by having a large hoop, large flattened and expanded terminals, which nearly meet, and simple pins bent around the hoop. Type H brooches have recently been reclassified by Laing who places the Tummel Bridge brooches in his group Hb (1993, 18) which have plain flaring terminals. This type of brooch is very simple, requiring relatively little skill to manufacture and is difficult to date precisely, with most authors favouring either a fifth or a sixth century date (Fowler 1963, 110-111; Longley 1975, 10) or a seventh century date (Graham-Campbell 1977, 279; Henderson 1992, 211; Stevenson 1976, 250). Few type H brooches come from well-dated contexts; much of the dating evidence for penannular brooches is provided by their presence in Anglo-Saxon burials. Although Fowler placed a number of brooches from Anglo-Saxon burials in type H I (1963,142), more recent reclassification shows that no true type H brooches occur in Anglo-Saxon burials (White 1988,20-21). The best parallels from Scotland are two similar silver items from the Norrie's Law hoard, with twisted hoops and plain terminals; probably torcs whose design was based upon type H penannulars (Cessford 1995, 237-38). The Norrie's Law hoard torcs are definitely atypical examples of class H penannulars which could cast doubt upon using them as evidence to date the Tummel Bridge brooches (Fowler 1964, 110). Nevertheless, their form, the use of silver and the lack of decoration suggest that the Norrie's Law and Tummel Bridge brooches are probably of similar date. The Norrie's Law hoard has recently been dated to the late seventh or early eighth century (Graham-Campbell 1991, 256), though a date in the early fifth century had also been proposed (Laing 1994, 35). On balance the later dating to the late seventh or early eighth century seems more probable, based on stylistic similarities between the beast heads on the two oval plaques from the Norrie's Law hoard and the dog heads in the Lindisfarne Gospels (Graham-Campbell 1991, 256-56). The two plaques are probably among the latest items in the Norrie's Law hoard and the atypical penannulars are likely to be of a slightly earlier date. The lack of complex decoration on the brooches also hinders a typological dating; it may suggest that the brooches belong to a period when previously highly decorated styles caused a "revulsion towards simplicity" and the use of a "plain style" (Stevenson 1974, 30; 1976, 250). Unfortunately, there is not enough evidence to date this supposed period and in any case there may have been more than one period when lack of decoration became fashionable.

It has long been noted that type H brooches often occur on sites which also produce imported E-ware pottery (Fowler 1964, 111). E-ware is impossible to date precisely, but the evidence suggests that its main period of importation into Western Britain and Ireland was in the late sixth and seventh centuries (Cessford 1994, 9; Thomas 1990). Longley states that it may be a mistake to use the presence of E-ware as dating evidence for type H brooches found at the same sites (1975, 10), but provides no evidence to back up this statement and it is difficult to understand why he should believe this. This all suggests that the Tummel Bridge brooches are of late sixth to early eighth century date with the seventh century being the most likely.

There are a number of brooches from Ireland which have similar punched dot decoration to the Tummel Bridge brooches (Graham-Campbell 1976, 259). A type H penannular from Lagore Crannog, County Meath, with a single row of punched dots on the terminals was discovered stratified in the phase 1a deposits (Fowler 1963, 142; Hencken, 1950, no 1009 p 59, fig 6). Hencken believed that phase 1a

related to the construction of Lagore, but it is now interpreted as a series of sunken and compacted occupation levels (Edwards 1990, 38-39; Lynn 1986). This means that the brooch cannot be closely dated by its context but the crannog was probably constructed no earlier than the seventh or early eighth century (Warner 1986) so the brooch is unlikely to date earlier than the seventh century. The only example from Ireland made of silver is a large brooch, diameter 12.4 cm, with very large terminals and a single row of punched dots from Lough Faughan crannog, Co Down (Archaeological Survey of Northern Ireland, 1966, 138 pl 83), a site which has produced E-ware pottery and which is believed to be of similar date to Lagore (Collins 1955). It has been suggested that this brooch was a Pictish export (Laing 1976, 17), but it is at least equally likely that it is an Irish product. There is also a similarly decorated brooch from Lough Ravel crannog (Derryhullagh), Co Antrim (pers comm J Graham-Campbell, 1966), a site which was badly investigated in the 1850s. The idea of constructing crannogs was a apparently introduced to Ireland from south-west Scotland in the late sixth century (Crone, 1993; Edwards, 1990, 36-7), so that these three type H from Irish crannogs must be of late sixth century date or later. A small bronze type H penannular with a single row of punched dots on the one surviving terminal was discovered at Dalkey Island, Co Dublin ((Fowler 1963, 142; Liversidge 1968, 114 figure 27). There are two phases at Dalkey Island: a midden which predates the fortification of the promontory and a later settlement within a bank and ditch (Edwards 1990, 41-2). The earlier phase is dated by the presence of B-ware amphorae which belong to the mid-fifth to the mid-sixth century, whilst the later phase is dated by the presence of E-ware pottery. The brooch was not discovered in a location which can be related to the bank or ditch (Liversidge 1968, 114) but it is not obviously related to the early midden and probably belongs to the second phase of occupation. The Dalkey Island brooch had traces of a white metal, either tin or silver, on the terminals (Liversidge 1968, 114) which suggests an attempt to copy a silver brooch in base metal. An Irish type H penannular brooch in the Ashmolean museum (Fowler 1963, 142) provides a close parallel for the Tummel Bridge brooches (pers comm J Graham-Campbell, 1966). It is made of bronze with a single row of punched dots on the terminals and a plain pin. Its diameter was probably originally around 7 cm and the pin is 8.3 cm long, so in size and decoration it is closely related to the Tummel Bridge brooches. This brooch originally came from the collection of Lord Antrim and it may come from County Antrim although this is not certain (pers comm A MacGregor, 1966).

Typologically, type H brooches are a development from earlier type Aa penannular brooches which are mainly found in Scotland and later brooch types which developed from type H also mainly occur in Scotland. In particular, a number of Scottish type Aa brooches have flattened terminals, a feature which presages type H brooches (Fowler 1964, 110). Type Hb brooches also occur in Ireland, Wales and England but they are concentrated in Ireland and Scotland. Scottish examples include two from North Uist and one from Loch na Berrie, Lewis (Laing 1993, 42-5), as well as the brooches from Tummel Bridge and Norrie's Law. One of the examples from North Uist and the brooch from Loch na Berrie have punched dot ornamentation on the terminals, although the dots are rather irregular and there are other forms of decoration on the terminals so that their ornamentation does not closely parallel that of the Tummel Bridge brooches. There is also a number of clay moulds for this type Hb brooches from the Pictish sites of Clatchard Craig, Fife (Close-Brooks 1987, 156-8; Laing 1993, 45), and the Brough of Birsay, Orkney (Curle 1982, 111; Laing 1993, 45-7). Moulds for large panelled type H5 brooches were found at Dunadd in phase III deposits dated to the seventh century (Campbell & Laing, 1973). There is no mould evidence for large type H brooches, such as those from Tummel Bridge, because these were produced by hammering rather than casting. The Irish parallels show that the brooches from Tummel Bridge could conceivably have been produced in Ireland but this is unlikely. It has long been recognised that there are close links between Pictish and Irish penannular brooches (Laing 1976) and it is likely that Pictish brooches were exported to Ireland and vice versa, possibly via the Irish kingdom of Dal Riata in Argyll, exerting strong reciprocal stylistic influences. The pivotal role of Dal Riata has been supported by finds from Dunadd, Argyll (Campbell & Lane 1993), where an amalgamation of Celtic and Germanic styles took place. The close similarities between the three Tummel Bridge brooches plus the use of silver, which is more common in Pictish than in Irish brooches at this period, indicate that they were manufactured in Pictland.

All three of the Tummel Bridge brooches have similar cracking damage to their terminals. These terminals were produced by hammering the silver flat and type H brooches have much larger terminals than earlier types of penannular brooch. It appears that the craftsman's attempt to maximise terminal size and visual display for a minimum amount of precious metal resulted in inherently weak terminals which were too fragile to wear. Later brooch types recognised this problem and had smaller and thicker terminals.

The three Tummel Bridge brooches are of very similar design and all three have similarly damaged terminals. It is likely that they were the products of a single Pictish craftsman or workshop and that their flaws meant that when deposited they were intended for melting down to recycle their valuable silver content. Such valuable objects would have been securely stored and the Tummel Bridge brooches are best interpreted as part of a seventh-century metal worker's hoard.

## The Hanging Bowl (Figs. 4 & 5)

The four bowl fragments and escutcheon from Tummel Bridge probably come originally from a single hanging bowl with a developed inturned rim and four-peltae openwork escutcheons. The surviving pieces are very small and fragmentary representing only a small part of the original bowl. Hanging bowls with openwork escutcheons are poorly dated and suggested dates have ranged between the fourth and seventh centuries (Brennan 1991, 13). It has recently been suggested that all hanging bowls belong to the mid-sixth to mid-seventh centuries and that openwork escutcheons primarily belong to the late sixth century (Ibid, 65-75). This narrow dating range for all hanging bowls is based upon the dates of their deposition in Anglo-Saxon graves and it is likely that some were produced both prior to and after the dates suggested by Brenan (Laing 1993, 21-3). Openwork escutcheons probably belong to the late sixth and early seventh century. The rim of the Tummel Bridge bowl shows distinct signs of flattening which are then taken a stage further by a similar bowl from Castle Tioram, Highland (Fowler 1968, 292; Kilbride-Jones 1937). This suggests that the Tummel Bridge bowl is earlier than the example from Castle Tioram which can be dated by its similarity to a mould from Craig Phadrig, Highland (Brenan 1991, 103-5; Bruce-Mitford 1987, 32-3; Stevenson 1972; Youngs 1989, 52), a site which is dated by the presence of E-ware pottery. A date in the late sixth or early seventh century is therefore likely on the evidence of both the bowl's rim form and its escutcheon.

There is good evidence that the hanging bowls were produced by the Picts as a clay mould has been recovered from the fort of Craig Phadraig and a possible die was found at the Brough of Birsay (Brennan 1991, 305). The Craig Phadrig mould would have been used to produce two-peltae openwork escutcheons identical to those on the bowl from Castle Tioram (Youngs 1989, 52). This bowl is similar to the example from Tummel Bridge and the Craig Phadrig two-peltae openwork mould is clearly related to the four-peltae openwork escutcheon from Tummel Bridge. There can therefore be little doubt that the Tummel Bridge hanging bowl is a Pictish product.

The hanging bowl from Tummel Bridge is a late-sixth or early-seventh century Pictish product. It is probably of slightly earlier date than the brooches from the hoard and this, combined with its fragmentary nature, suggests that it was an old and damaged item intended for recycling. While the majority of hanging bowls have been found in Anglo-Saxon graves most of those recovered from Scotland have come from settlement sites (Brennan 1991, plus a new example from Buiston Crannog, North Ayrshire – Crone 1991, 295). The only hanging bowl found in a hoard is the atypical and late silver example from St. Ninian's Isle, Shetland (Small et al. 1973, 55-7), for there is no evidence to support Brennan's assertion that the Castle Tioram bowl came from a hoard (1991, 104) in preference to the original suggestion that it came from a kitchen midden (Kilbride-Jones 1937, 206). In contrast, fragments of hanging bowls are known from the settlements of Buiston Crannog (Crone 1991, 295), the Brough of Birsay, Clatchard Craig, Craig Phadrig and Dunadd (Brennan 1991, 303-5).

**The Cup** (Fig. 6)

The bronze cup fragment is unparalleled in other finds of this period, but is similar to a Roman period item from Traprain Law, Lothian (Burley 1958, 187). Other Roman period objects are known from Pictish contexts, such as the folded-up spoon bowl from the Norrie's Law hoard (Laing 1994, 34; Stevenson 1958, 229), and it is likely that the cup fragment was grouped with the pieces of hanging bowl and intended for recycling.

DISCUSSION

The finds from Tummel Bridge appear to be a late-seventh or early-eighth century group of scrap metal designed for recycling. The latest element in the group consists of the three silver brooches which probably had a relatively short life before their inherent faults meant that they could no longer be used. Of earlier date are the remains of a hanging bowl and cup which were both probably in a fragmentary state before they were deposited. There is no reference to the hoard being in any form of container, but there is evidence that Viking hoards were sometimes placed within organic containers which could easily have decayed away and been overlooked especially during a chance discovery (Graham-Campbell 1995, 59-61). Another possibility is that the valuable silver brooches were stored in an old and damaged hanging bowl intended for recycling. The exact size of the Tummel Bridge hanging bowl is unknown but hanging bowls have an average diameter of over 23 cm. and an average height of 10 cm (figures based upon those in Brennan 1991, 171-299) which could easily have accommodated the three brooches form Tummel Bridge. The brooches and hanging bowl are all of Pictish manufacture and the presence in the vicinity of various pieces of sculpture shows that Tummel Bridge lay within Pictish territory. There is a Class I stone at Struan (Allen & Anderson 1903, 285-6) which is situated on the river Garry, a tributary of the river Tummel, and Class II stones have been found at Logierait and Dunfallandy on the Tummel itself (Ibid, 286-9; 291-2; Mack 1997, 50). The Dunfallandy stone, which is of early-eighth century date, is particularly interesting as the rear of the stone includes a unique depiction of a hammer, anvil and tongs which may well denote that the stone was erected by or for a skilled metalworker (Thomas 1964, 55, 80-1). The Tummel Bridge hoard was probably deposited around the same time by a Pictish metalworker, though of course the hoard and the carving need not be linked to the same individual.

It is possible that the brooches and the bronze vessels were not deposited as a single group and only became associated when the tree was uprooted. It was not uncommon for separate discoveries of metal work in the eighteenth and nineteenth centuries to become combined into one, for example, some coins and a Viking silver arm-ring fragment were spuriously associated with the Norrie's Law hoard (Laing 1994, 22-3; Graham-Campbell 1991, 243, 248-9; 1995, 257). Other Pictish hoards from the Broch of Burgar, Orkney, Norrie's Law and St. Ninian's Isle consist largely of precious metal and do not contain any bronze items. They are generally interpreted as collections of personal wealth deposited for safety during troubled periods, typically Anglo-Saxon or Viking invasions or raids. The hanging bowl and cup fragments are unlikely to have come from such a hoard. The only vessels found in other Pictish hoards are from the Broch of Burgar (Graham-Campbell 1985, 251-2) and St. Ninian's Isle (Small et al 1973, 47-55, 106-12), these two sets consist of complete bowls and are made of silver in contrast to the bronze fragments from Tummel Bridge. While the hanging bowl may conceivably have been used as a container for the brooches this does not explain the presence of the cup fragment, so even if the container hypothesis is accepted, the finds are still unlikely to have come from a typical Pictish hoard. In contrast, the silver brooches would be normal finds in a Pictish hoard of this period (Graham-Campbell 1985, 255 and table 2). It appears likely that the objects from Tummel Bridge represent a metalworker's hoard of items intended for recycling. The other known Pictish hoards were all deposited in locations where it would be possible to locate them again easily. The Broch of Burgar hoard was placed within the wall of one of the intramural cells of the Broch, the Norrie's Law hoard was found at the foot of a prehistoric cairn and the St Ninian's Isle hoard was found under a stone slab at the east end of a medieval church

where there is evidence for an early Christian foundation. It is likely that the Tummel Bridge objects were also buried as a temporary measure for safety in a prominent location, especially if they were intended to be recycled.

One possibility is that they were deposited on a contemporary settlement site where a metalworker was active, but as the exact location of the discovery is unknown this must remain speculation. Even if they were not, their presence suggests the existence of an important settlement in the vicinity. Such a site, if it exists, would probably be broadly similar to the small sixth- and seventh-century defended Pictish settlements at Clatchard Craig which produced evidence for metalworking in the form of moulds, including examples for penannular brooches, and crucibles, as well as a silver ingot and a possible hanging bowl escutcheon (Close-Brooks 1987; Brennan 1991, 303). Various ringforts or homesteads in the general vicinity were identified by William J Watson in the early part of this century (1913; 1915), such as Grennich Fort and Foss A and B at the western end of Loch Tummel and Queen's View, Lower and Upper Borenich and Balnabodach slightly further east. There is also the site of Dun Teamhalach (Tummel Fort) which is known only from place-name evidence (Watson 1926, 451). Any one of these may have been occupied at a time when the Tummel Bridge hoard was deposited, although the only excavated example, at Lower Borenich, failed to produce any closely datable finds (Watson 1915, 28-32). A nineteenth–century discovery from any of these sites could easily have been described in general terms as coming from Tummel Bridge. The river Tummel may well have been an important Pictish route from the east coast into central Scotland and possibly beyond to Loch Etive and Loch Leven on the west coast. This would explain the presence of Pictish sculpture along the river and suggest that there may well have been a strategically important settlement in the vicinity of Loch Tummel from which the metalwork originated.

The Tummel Bridge brooches which are decorated 'at most by an inconspicuous punched border' and with 'their simple heads' indicate the existence of a 'plain style' in Pictish art (Stevenson 1974, 32), studies of Pictish art which fail to take this into account and focus exclusively on more ornate styles are seriously flawed and present only a partial picture of the Picts.


## ACKNOWLEDGEMENTS

## NOTE

[1] As the items were purchased by the then NMAS they do not appear in the volumes of the museum's 'Scroll Book' recording donations but are in the 'Scroll Book' recording accessions, which is presently inaccessible. The PSAS entry, however, normally gives the details of the 'Scroll Bool' verbatim (pers comm Dr. Alison Sheridan, NMS, 1996)

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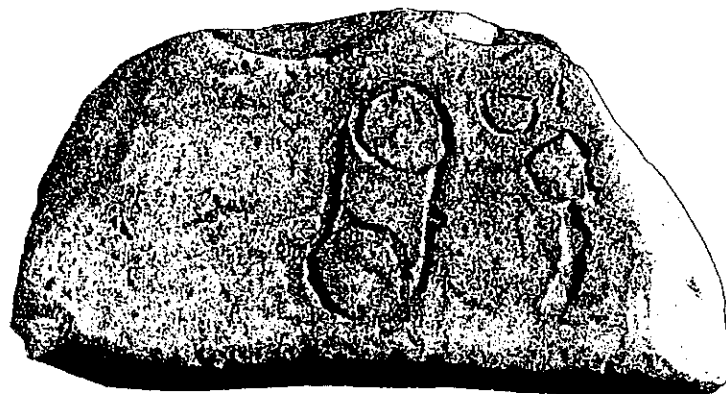
# BOOK REVIEWS

**The Language of the Ogam Inscriptions of Scotland**. By Richard A V Cox. £12
Dept. of Celtic, Aberdeen University AB24 3UB  ISBN 0 9523911 3 9
www.abdn.ac.uk/celtic/publctns.htm

In his introduction, Richard Cox states that these 'inscriptions represent a language which is phonologically Scandinavian' and that 'the formulae contained in the inscriptions are paralleled in runic inscriptions.' In his preface he cautions that his subject matter is 'iconoclastic by implication' and 'far reaching in significance.' This is most certainly true if his theory is correct. It will shut down some long pursued avenues of research into the Picts and open up others. The question is how correct is his theory?

The book is most admirably laid out. Cox first takes seventeen of the some forty ogam style inscriptions in Scotland and discusses each one in detail. Then he follows these individual discussions by discussions involving all seventeen (or nineteen if his two non-ogam inscriptions are included) as a group from the viewpoints of formulae, lacunae, orthography, phonology, morphology, syntax, and chronology. As a further aid, the discussions of the individual inscriptions are extensively cross referenced to the group discussions. However, despite all of this, unless the reader has some acquaintance of Scandinavian languages in general and some acquaintance with runic inscriptions in particular, the book has to be studied as if it were a manual rather than perused as a research report. This means that whatever the reader may know about ogam inscriptions in Scotland must temporarily be put to one side while carefully studying the discussion of each individual inscription. In addition the reader must meticulously check each cross reference to the group discussions as soon as it is encountered. While this procedure may at first seem tedious, the reader is rewarded by a fascinating flow of information about Old Norse (which Cox believes is the cognate language) and about runic inscriptions 'in Scandinavia' and 'in areas which were also once part of the Viking world.' When all the foregoing is done, the reader will understand the point that Cox is trying to make.

In discussing the individual inscription, Cox proceeds in four stages. First, he presents a standardised replica of the inscription, then a transliteration of the inscription into Roman letters, then an emendation of the inscription to take care of lacunae and finally an Old Norse rendering of the inscription which he translates into English and compares with corresponding runic inscriptions. The final stage of his procedure cannot be criticised. his problems, both micro and macro, occur when comparing what he does during the steps leading up to this final stage with work which has been done previously on the ogam inscriptions of Scotland.

This reviewer certainly cannot fault his choice of Old Norse as an appropriate area of inquiry. In reviewing the notes for a lecture I gave on 7 November, 1996 to the Pictish Arts Society, I noticed that I had arrived at similar translations for the Gurness, Ackergill, Golspie and Altyre inscriptions by employing Anglo-Saxon. However the translations of these same inscriptions by Cox using Old Norse are superior to those made by using a rough Anglo-Saxon patois.

Cox is heavily dependent on formulae to achieve his translations. Formulae of course have always been the most useful tools an epigrapher could have for deciphering inscriptions whether the epigrapher was Champollion with the Rosetta Stone or Rawlinson at Behistun. For example when deciphering Irish monumental ogam, the use of the MAQQ formula (or a variation thereof) for detecting the patronymic of the honoree of the inscription has proved to be a good starting point for decipherment. In working with the Scottish inscriptions, Cox has come up with two Old Norse words as formulae. These are EFT (or a variation thereof) meaning 'in memory of' and SEDD (or a variation thereof) meaning 'erected by.' One or both of these formulae occur in almost all of his decipherments.

However despite the excellence of his formulae, there is the feeling that an over dependence on EFT and SEDD sometimes leads Cox into a trap. For example in the first line of the Bressay inscriptions the words CROSCC NAHHTVVDDARRS DATTR ANN can be translated (using the aforesaid rough patois and considering the double letters to be some kind of an allophone of the single letter) to say that the stone is a 'cross in memory of Naddodd's daughter Ann.' This is similar to the translation that Cox obtains by using Old Norse. Continuing by the same rough patois to translate the second line of Bressay, the words BENNRISES MEQQ DDRROANN would be expected to say something like 'Benres son of Droan.' Instead, Cox has 'Bjarni erected me – may the Lord (save her) soul.' While this is not to say that translation by Cox of the second line is wrong, it does cause doubt to appear on what should be an impeccable horizon.

As a general rule, work on ogam inscriptions has been expected to measure up to what is sometimes referred to as the triple 'I' paradigm. That is the inscription is written in a script of Irish origin using an Indo-European language and engraved by an indigenous population. To the dedicated believer in the triple 'I' paradigm, the addition of letters beyond those actually contained in the inscription is beyond the pale. This belief is vividly illustrated by the invective directed at North American epigraphers who have the temerity to supply vowels for the otherwise all consonants transliteration of their ogam like inscriptions. However, Cox, in his determined drive to reach the EFT and SEDD formulae, emends what he sees as lacunae by supplying additional vowels and consonants with an abandon that would leave a North American epigrapher aghast. Thus the aforementioned second line of Bressay which he transliterated into Roman characters as BENNISES MEQQ DDRROANN is emended to read BE[R]NNI SES[I] MEQQ DDRRO[TTINN(...)]ANN[DU].

The justifications that Cox advances as a license for his lacunae emendations seem reasonable enough. One is the admitted necessity to use abbreviations when engraving on stone. Here he has ample precedence in contemporary Latin inscriptions. Another is that words evolve over a period of time. This has long been recognised even for the aforementioned rough patois (Skeat *Principles of English Entomology*, Oxford 1887). Then there is the frustration encountered when a script does not have enough characters to accommodate all the phonemes. This was the cause for the rise of forfeda in Irish ogam (Sims-Williams *The Additional Letters of the Ogam Alphabet*, CMCS 23, 1992). In as much Cox gives several other justifications for his emendations, the question arises as to whether the paradigm needs to be changed.

However if the triple 'I' paradigm is to be changed, what changes are to be allowed? Should a non Indo-European language be permitted? These days such a suggestion is going to receive short shrift (Forsyth *Language in Pictland*, Utrecht 1997). Then how about a non-indigenous people? That suggestion is simply not going to garner much support in certain archaeological circles (Simon James *The Insular Celt: Ancient Peoples or Modern Myth* , British Museum 1999). Then how about assuming the ogam inscriptions in Scotland are in a script of non-Irish origin?

Here a start can be made since the Auquhollie inscription has light heartedly been referred to as the only genuine Irish ogam inscription in Scotland. That is because it has notches (instead of perpendicular lines across the stem line) to indicate vowels. But the problems encountered with transliteration are more complex than this. I am sorry that Cox did not have a go at the Lunnasting stone. Here there are five different ways to indicate what is presumed to be the letter 'e' plus other items like word dividers, serifs, curly letters and bind ogam (that is ogam with a top and bottom line as well as a stem line). However, Cox did tackle the Burrian stone. Here he encountered odd shaped characters that look like hammer heads, hatch work, check marks, and IPA symbols. Moreover, what should have been the normal ogam characters are in bind ogam. Cox transliterated Burrian into roman letters by falling back on the aforementioned forfeda and, since he knows what he wants to achieve, by considered judgement.

Relative to the types of problems which are encountered in transliteration, I suggested (TNHC: *Tousist 6000*, Dublin 1999) while discussing the long multi-stroke ogam style inscriptions (of which Tollard house in Argyll is an example), that all ogam line inscriptions ought to be regarded as members of the same family. Then the ogam engraved on Irish monuments would be seen as an adaptation to cope with Early Irish by the expedient of adding notches to indicate vowels. The long multi-stroke ogam inscriptions themselves could be an earlier or even divergent stage in the evolution of the family. The script of the ogam inscriptions of Scotland would be the final stages of an evolution to accommodate the language in use there.

The little information which is available about the language of the Picts consists partly of some scanty place name evidence (Nicolaisen *Picts and their Place Names* Groam House Museum 1996), mostly in the northeast Scotland homeland of the Picts, and partly, it has been assumed, in whatever is contained in the ogam inscriptions of Scotland. These are mostly located in the northeast. Unfortunately for the line of enquiry that Cox is pursuing, onomastic evidence is not helpful for establishing Old Norse as a language of the northeast. Most Scandinavian place names occur in the west and southwest of Scotland (Nicolaisen *Scottish Place Names*, London 1976). However this reviewer cannot with a clear conscience gainsay existence of at least some Scandinavian elements in the northeast. My own clan, the Grants of Spey valley, are usually presumed to be Normans imported by David II with the name *Grand* evolving to *Grant*. However among the myriad of clan genealogical histories (*Genealogical Collections*, c1750) there is and alternate explanation that we are refugees from the 950AD devastation of the Eldgia volcanic eruption in Iceland with a Norse patronymic being shortened from *Hacken Grandt* to *Grant*.

In his chronology of the ogam inscriptions in Scotland, Cox boldly dates the carving of the ogam inscriptions in Scotland as occurring between 1050AD and 1225AD. He bases his dating on what appears to be reasonable diachronic linguistic evidence (E.V. Gordon Old Norse Oxford 1927). However Cox does say that ogam writing itself is based on a centuries old tradition'. Moreover he adds that '*during the period of its adoption and development for the Norse language the system offered an opportunity, so far as it was able to reflect more closely the speech of its practitioners*' [italics mine]. Elsewhere Cox states that the foregoing use of ogam is the reason for the absence of the runic inscriptions which would normally be expected in an area with a Scandinavian element. What then does the proposal by Cox imply for the belief that ogam inscriptions in Scotland are bearers of the Pictish language?

In the case of the ogam engraved on Irish monuments, the transliteration task was much simpler since Early Irish is a known language. (Damian McManus *A Guide to Ogam*, Maynooth 1991). As for the Scandinavian linguistic ambience, runes themselves underwent both a contraction from the 24 characters of the 'older futhark' to the 16 characters for the 'younger futhark' of the Vikings and an expansion to the 31 characters of the Anglo-Saxon runic inscriptions (R.I. Page, British Museum 1987). thus the comment by Cox concerning the 'adoption and development' of ogam in Scotland creates a double question :
>     (1) Are the potpourri of additional characters which appear in the ogam inscriptions in Scotland a development to accommodate Old Norse and therefore a proper subject for the scale of the emendation which Cox has to make; or
>     (2) are the potpourri of additional characters an adaptation to accommodate the language of the Picts with perhaps unrecognised phonemes, cartouches or a rebus lurking in the inscriptions?
An ancillary question would be whether some of the translations which Cox makes using a minimum of emendation (and this reviewer makes with a rough patois), are support for the theory that Cox puts forward or are they merely a synchronic linguistic coincidence? Finally it may be asked just what is the language of Pictland?

The source of the answers to these questions would appear to lie in raising the horizons of the enquiry. Elsewhere the intermingling of proto-Celtic with the non Indo-European scripts of Spain has been discussed (Kim McCone *Towards a Relative Chronology of Ancient and Medieval Sound Changes*, Maynooth 1996).

Moreover we are dealing with an area where, during the optimal weather conditions of Atlantic and Sub-Boreal climatic periods, there was a vibrant culture whose stoneworks stretched from the Clava stones near Inverness to the menhirs of Carnac in Brittany (Richard Bradly *Altering the Earth*, Edinburgh 1993) before succumbing to the present chill of Sub-Atlantic climatic conditions. Lastly it must not be forgotten that Bede was a linguist as well as historian and did recognise that the Picts had a distinct language.

As a postscript to his book Cox concludes that because of his theory that the ogam inscriptions of Scotland are of Old Norse origin, 'one long standing question has, hopefully, been resolved'. However it would seem to this reviewer that what Cox has done is to present a theory to show that the ogam inscriptions of Scotland and the Scandinavian languages have a synchronic linguistic connection (sub-stratum or super-stratum). As such this provides a much needed palliative to offset the belief that developments in Scotland have come up from the lower half of the British Isles (or form Ireland). The theory that Cox presents encourages looking not only in eastern and northern directions but also back to a past which existed for three millenniums before Celtic culture first arrived.

**Bill Grant**
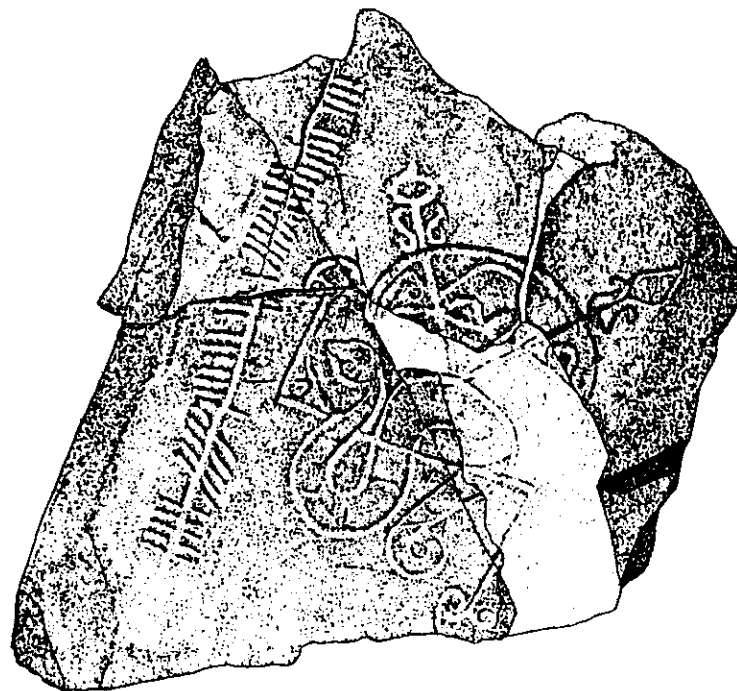
**Surviving in Symbols: A Visit to the Pictish Nation.** By Martin Carver £5.99 The Making of Scotland Series, Historic Scotland/Canongate, Edinburgh, 1999. ISBN 086241 873 3

In this slim, lavishly illustrated volume, Carver summarises much of what we know about the Picts from contemporary written sources and from what can be surmised from the archaeological evidence currently available. The text is laudably free from romantic speculation; indeed a judicious degree of caution is evident throughout. Rather than tackle the thorny question of the limits of Pictish territory, Carver contents himself with the plain notice "the eastern part of Scotland, called here Pictland.." before going on to describe the nature of the available evidence on Pictish culture. In short, concise sections on topics such as the meaning of the symbols or on where the Picts came from, he makes plain that there has been or is still a deal of scholarly debate in many areas of Pictish studies.

The artist's sketches illustrating aspects of Pictish life worked less well for this reader than the many maps, plans of sites and photographs of sites, stones and artefacts which are well chosen to bring to life the nature of the evidence which Carver presents.

The presentation and clarity of style combine to make this a very useful introduction to the current state of knowledge about the Picts, and one that can easily be enjoyed by any non-specialist.

**Sheila Hainey**

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