

## II.

## MISCELLANEA ROMANO-CALEDONICA. II.

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## 1. ROMAN COINS FOUND IN SCOTLAND.

Sufficient material has accumulated to justify the publication of a third supplement to my original list of Roman coins found in Scotland. It will be convenient to adhere to the arrangement hitherto followed.<sup>1</sup>

## (A) ISOLATED FINDS FROM ROMAN SITES.

*(a) South-Eastern Scotland.*

NEWSTEAD.—In February 1934 I was shown a *denarius* of Tiberius (Coh.<sup>2</sup> i. p. 191, No. 15, with inverted spear instead of sceptre), which had been picked up at Newstead. It had seen much circulation.

INVERESK.—*The Scotsman* of 27th August 1938 reported the discovery of a *denarius* of Hadrian, in good condition, during the extension of the churchyard, which partly overlies the Roman fort. To judge from the description, it may have been one of the group Coh.<sup>2</sup> ii. p. 136, Nos. 353–56.

*(b) South-Western Scotland.*

BIRRENS.—Twelve coins were recovered during the excavations of 1936 and 1937.<sup>2</sup> Unfortunately, all of them were in poor condition. Nothing whatever could be made of three *denarii* and three “second brass.”<sup>3</sup> But Mr Percy Hedley was able to identify two *denarii* of Trajan (Coh.<sup>2</sup> ii. p. 31, No. 120, and p. 38, No. 190), two “second brass” of the same Emperor with obliterated reverses, and two “second brass” of Pius, both with the type of Britannia seated (Coh.<sup>2</sup> ii. p. 282, No. 117). Mr Birley further records that he saw in private hands a worn bronze of Domitian and an *antoninianus* of Victorinus, both said to have been found in 1895. As indicated *infra*, p. 272, I am unable to accept the alleged provenance of the latter.

CASTLEDYKES.—Miss Anne Robertson, of the Hunterian Museum, has shown me a “second brass” of Hadrian (Coh.<sup>2</sup> ii. p. 170, No. 754),

<sup>1</sup> *Proceedings*, vol. lii. (1917–18), pp. 203–76; vol. lviii. (1923–24), pp. 325–29; and vol. lxviii. (1933–34), pp. 27–40.

<sup>2</sup> *Proceedings*, vol. lxxii. (1937–38), pp. 339 f.

<sup>3</sup> I have seen the coins and have nothing to add to Mr Hedley's excellent report, except perhaps that the size and general appearance of the illegible pieces showed that none of them was necessarily later than the reign of Commodus.

recovered during her excavations on this site in April 1939. A second, much corroded, piece of bronze was probably also a coin of Hadrian.

(c) *The Antonine Wall.*

BEARSDEN.—A *denarius* of Hadrian (Coh.<sup>2</sup> ii. p. 135, No. 335) was dug up in the garden of Mrs Brownlie, Ardenraig, Thorn Drive, Bearsden, on or near the line of the Wall and not far from the site of the fort, in May 1938. It was 1½ foot underground.

(d) *Scotland North of the Antonine Wall.*

ARDOCH.—In December 1935 the local schoolmaster showed me at the Museum a "first brass" of Hadrian (Coh.<sup>2</sup> ii. p. 186, No. 973), found at Ardoch.

FENDOCH.—The description, given in the *New Statistical Account*,<sup>1</sup> of a *denarius* from this site enables it to be identified with virtual certainty as one of the "autonomous" issues of Galba (Coh.<sup>2</sup> i. p. 348, No. 406).

(B) ISOLATED FINDS FROM NATIVE SITES.

EDGERSTON (Jedburgh).—Mrs Oliver kindly sent for my inspection a *denarius* of Trajan (Coh.<sup>2</sup> ii. p. 46, No. 276), found in 1938 during excavations at "The Camps."

TRAPRAIN LAW.—In June 1939 two additional *denarii* from this site reached the Museum. They had been discovered by Mr Cruden during his investigation of the structure of the rampart. One was too much corroded to admit of certain identification, but it may have been a Hadrian. The other belonged to the Republican period, having been minted by L. Valerius Acisculus c. 45 B.C. (*B.M. Cat.*, i. p. 536, Nos. 4110 ff.). This is the earliest coin yet recorded from Traprain.

(C) ISOLATED FINDS FROM SITES OF INDETERMINATE CHARACTER.

UPPER TEVIOTDALE.—I owe to the late Mr J. M. Corrie a reference to *Trans. Hawick Arch. Soc.*, 1902 (p. 7), where it is stated that many years ago an *aureus* of Domitian, "as fresh and beautiful as the day it came from the Roman Mint," was unearthed in "the encampment on Rigghill, close by the present farmhouse of Caerlenrig." The site has not yet been visited by the officers of the Royal Commission, but the O.S. Map marks a small rectangular entrenchment in the position indicated.

RUBERSLAW (Roxburghshire).—In December 1936 Mrs Kelsall presented to the Museum a "first brass" of Vespasian (Coh.<sup>2</sup> i. p. 399, Nos. 419–21), found on Ruberslaw "more than 60 years ago" by Alfred Bald.

<sup>1</sup> *Perthshire*, p. 273. The passage, which was written in 1837, is quoted by Mr Richmond, *supra*, p. 146.

One naturally recalls the Roman-dressed stones observed on and near the summit of the hill by Dr A. O. Curle.<sup>1</sup>

COLDINGHAM (Berwickshire).—Dr Douglas Simpson has drawn my attention to a footnote on p. 13 of Carr's *History of Coldingham Priory*, which records that "a small brass coin of Titus Vespasian has been found by Mr James Belaney, surgeon, on the farm of Ayton Law, about fifty yards distant from the site of a Roman encampment, now very much defaced."

(D) ISOLATED FINDS WITH NO RECORDED ASSOCIATIONS.

NEW LUCE.—In September 1936 I was shown a "small brass" of Constantius II. (Coh.<sup>2</sup> vii. p. 446, No. 44), which had been found in the bed of the River Luce near New Luce.

MAXTON (Roxburghshire).—In April 1936 I had seen another "small brass" of Constantius II. (Coh.<sup>2</sup> vii. p. 492, No. 335), found on the banks of the Tweed near Maxton by Colonel Danford.

JEDBURGH.—In the autumn of 1934 a billon coin of Alexandria, issued in Year 2 of Maximianus and having Nike on the reverse, was found about 4 feet below the surface by workmen digging a drain in the old Horse-market, Jedburgh.

NORTH BERWICK.—In November 1934 Mr J. S. Richardson kindly showed me a much-worn "first brass" of Pius and a "small brass" of Constantine the Great (Coh.<sup>2</sup> vii. p. 290, No. 519), both said to have been dug up at North Berwick.

GULLANE.—In 1935 Mr H. J. Younger came upon a "small brass" of Theodosius I. (Coh.<sup>2</sup> viii. p. 159, No. 41) in excavating a kitchen-midden at Gullane.<sup>2</sup>

ECCLEFECHAN.—In his Report on Birrens Mr Birley mentions a bronze coin of Maxentius (Coh.<sup>2</sup> vii. p. 168, No. 27 or 28) "in mint condition," found about 1935 near Ecclefechan.<sup>3</sup>

IRVINE.—In October 1937 Miss Anne Robertson identified a "middle brass" of Constans I. (Coh.<sup>2</sup> vii. p. 407, No. 18), which had been dug up in a garden at Irvine.

COATBRIDGE.—In November 1938 Miss Robertson showed me a bronze coin which had been found some weeks previously in Whifflet, a district of Coatbridge, by Mr J. M. Davidson. He had picked it out of the upcast of a trench 3½ feet deep, which was being cut to lay an extra water supply. It proved to be an imitation of a Claudian *as*, one of the group dealt with by Mr C. H. V. Sutherland in No. 65 of the American Numismatic Society's *Notes and Monographs*. The head on the obverse and some of the letters of the inscription could be made out readily enough, but the reverse was

<sup>1</sup> *Proceedings*, vol. xxxix. (1904-5), pp. 225 ff.

<sup>2</sup> *Proceedings*, vol. lxx. (1935-36), p. 339.

<sup>3</sup> *Proceedings*, vol. lxxii. (1937-38), p. 340.

rubbed smooth. So far as I am aware, this is the first imitation Claudius recorded from Scotland, but it is matched by the Antonia from Norrie's Law, which (as I pointed out in my second supplement <sup>1</sup>) is also an imitation.

KNIGHTSWOOD (Glasgow).—In April 1935 I had submitted to me a much-worn "second brass" of Hadrian (Coh.<sup>2</sup> ii. p. 161, No. 642), dug up 6 feet below the surface in this Glasgow suburb.

CARLUKE.—In May 1934 Mr J. Nairn, Braidwood, Carluke, sent to me for identification two coins which he had thrown up when "digging in the orchard." They had both been minted at Alexandria, one in the first regnal year of Diocletian with Hope as a type, the other in his fifth with Eusebeia. An inquiry as to the depth at which they were lying elicited no response. One cannot, therefore, discount the possibility that they were recent importations, thrown aside as valueless.<sup>2</sup>

STIRLING.—In the early summer of 1939 a workman came upon a "second brass" of Tiberius (Coh.<sup>2</sup> i. p. 54, No. 228) while demolishing the foundations of premises, now a garage, at the corner of Upper Craigs and Goosecroft Road.

AUCHTERARDER.—I am now able to correct and amplify the reference in *O.S.A.* to the coin "of the Emperor Titus Vespasian" found here before 1792.<sup>3</sup> In 1938 Mr John Ritchie disinterred in the archives of the Perth Museum a letter written, on 11th Sept. 1784, by John Gillies, a well-known bookseller and antiquary in the city, to the Rev. James Scott. Its purpose was to enclose "a drawing of a Gold Medal of the *Emperor Vespasian* which was found last week in digging the foundation of the old Church of Auchterarder." The drawing shows that it was not an *aureus* but a "second brass" (Coh.<sup>2</sup> i. p. 381, No. 181).

SKYE.—When in Skye in 1772, Pennant was presented with "a *Denarius*, of the Emperor *Trajan*, found on a moor near the shore of *Loch-Grisernis*."<sup>4</sup> Loch Grisornish is just over 6 miles N.E. of Dunvegan Castle.

#### HOARDS OF SILVER.

##### (b) *South-Western Scotland.*

KIRKINTILLOCH.—In May 1939 I was shown two stragglers from the hoard of *denarii* found here in 1893<sup>5</sup>—a Domitian (Coh.<sup>2</sup> i. p. 474, No. 51) and a Nerva (Coh.<sup>2</sup> ii. p. 3, No. 25).

##### (c) *Scotland North of the Antonine Wall.*

FALKIRK.—I have had an opportunity of examining four stragglers from the great hoard found at Falkirk in 1933.<sup>6</sup> They were *denarii* of Vespasian

<sup>1</sup> *Proceedings*, vol. lxviii. (1933-34), p. 30, footnote.

<sup>2</sup> Cf. *Proceedings*, vol. lxviii. (1933-34), p. 31.

<sup>3</sup> *Proceedings*, vol. lii. (1917-18), p. 246.

<sup>4</sup> *Tour in Scotland*, 1st ed. (1772), p. 344; 2nd ed. (1776), pt. i., p. 344.

<sup>5</sup> *Proceedings*, vol. lii. (1917-18), pp. 262 f.

<sup>6</sup> *Proceedings*, vol. lxviii. (1933-34), pp. 32 ff.

(Coh.<sup>2</sup> i. p. 384, No. 226), Titus (Coh.<sup>2</sup> i. p. 443, No. 158), Hadrian (Coh.<sup>2</sup> ii. p. 187, No. 989), and Commodus (Coh.<sup>2</sup> iii. p. 295, No. 504).

PORTMOAK (Kinross).—Shortly after the publication of my last supplement a parcel of 26 *denarii*, belonging to a trust estate, were brought to the Museum for identification. A note preserved with them said that they had been found at Kirkness (Kinross) in 1851, in a moss. As Kirkness is in the Parish of Portmoak, there can be no doubt as to their having formed part of the hoard of 600 or 700 discussed in my original list.<sup>1</sup> Here is an inventory of them: M. Antony (Coh.<sup>2</sup> i. pp. 41 f., Nos. 26 ff.—legionary number illegible), Vitellius (Coh.<sup>2</sup> i. p. 359, No. 45), Vespasian (Coh.<sup>2</sup> i. p. 396, No. 373), Titus (Coh.<sup>2</sup> i. p. 442, No. 153), Domitian (Coh.<sup>2</sup> i. p. 504, No. 397), Nerva (Coh.<sup>2</sup> ii. p. 7, No. 59), Trajan (Coh.<sup>2</sup> ii. p. 27, No. 87; p. 57, No. 372; and p. 77, No. 575), Hadrian (Coh.<sup>2</sup> ii. p. 198, No. 1108, and p. 224, No. 1425), Pius (Coh.<sup>2</sup> ii. p. 287, Nos. 155 and 156; p. 288, No. 164; p. 304, No. 344; and p. 332, No. 631), Faustina Senior (Coh.<sup>2</sup> ii. p. 414, Nos. 1 (two specimens) and 11; p. 421, No. 108; and p. 425, No. 159), M. Aurelius (Coh.<sup>2</sup> iii. p. 12, No. 102; p. 56, No. 543; and p. 63, No. 628), Lucilla (Coh.<sup>2</sup> iii. p. 215, No. 7), and Commodus (Coh.<sup>2</sup> iii. p. 269, No. 311).

BRIGLANDS (Kinross-shire).—In the early summer of 1938 Lord Clyde asked me to identify for him three *denarii*, which had been found at the mouth of a rabbit-burrow on his property of Briglands. The exact spot was on the bank of the Devon, about half a mile from Rumbling Bridge. They proved to be of Otho (Coh.<sup>2</sup> i. p. 353, No. 17), Vespasian (Coh.<sup>2</sup> i. p. 395, No. 366), and Pius (Coh.<sup>2</sup> ii. p. 292, No. 197). In returning them I suggested that they were probably "strays" from a hoard, and that a systematic search might be worth while. The whole area was accordingly carefully cleared, with the result that six others were recovered. They were of Julia Titi (Coh.<sup>2</sup> i. p. 466, No. 14), Trajan (Coh.<sup>2</sup> ii. p. 29, No. 156, and p. 44, No. 248), Hadrian (Coh.<sup>2</sup> ii. p. 198, No. 1102), Pius (Coh.<sup>2</sup> ii. p. 299, No. 284), and Commodus (Coh.<sup>2</sup> iii. p. 342, No. 879). Although the coin of Commodus was struck in A.D. 183, considerations of locality suggest that this little hoard was not buried until the early years of the third century.<sup>2</sup> In other words, the likelihood is that it belonged to the same "late" class as the very much larger hoard from Kirkness, and that it had been concealed under stress of the same set of circumstances.

## 2. A NEW INSCRIPTION FROM MUMRILLS.

I take this opportunity of putting on record the text, so far as it has been preserved, of the inscription on the portion of an altar discovered by Mr Samuel Smith at Mumrills in the early winter of 1937-38,<sup>3</sup> and kindly presented to the National Museum by Mr Forbes of Callendar.

<sup>1</sup> *Proceedings*, vol. lii. (1917-18), pp. 264 f.

<sup>2</sup> *Proceedings*, vol. lii. (1917-18), p. 275.

<sup>3</sup> See Mr Smith's "Note."

It is part of a dedication to the Mother Goddesses by a certain Cassius, the *signifer* or standard-bearer of a regiment that had once garrisoned the fort. Unfortunately, the area of the die is too restricted to have admitted any mention of the unit to which he was attached. The fragment has a maximum height of some 14½ inches and a maximum breadth of 10 inches. As it probably represents three-quarters of the original, the altar has been a small one. This, combined with the rudeness of the workmanship (Pl. LXXX), indicates that the oblation was anything but costly. The letters, which have an average height of about 2 inches, read:

CASSIVS  
SIGN  
MATRIBVS

On the assumption that there were five lines in all—it does not look as if there could have been more—the fifth was doubtless **VSLLM**, the stock formula, while the fourth would be occupied by one of the numerous epithets that are found attached to the noun in similar dedications. It has been suggested<sup>1</sup> that the missing word may be *Campestr(ibus)*, as the *Campestres* are often conjoined with the *Matres* and are twice mentioned, though without the *Matres*, on inscriptions from the Antonine Wall.<sup>2</sup> Equally, however, it may have been one of those presumably local designations which are so common on the Continent and an example of which (*Alatervis*) occurred on a lost altar from Cramond.<sup>3</sup> It would be idle to guess further, for there are nearly 130 such designations to choose from.

To the short account of the Mother Goddesses appended to my description<sup>4</sup> of the relief from Colinton, now removed to Fort Augustus, I take this opportunity of adding references to Pauly-Wissowa, *Realencyclopädie*, vol. xiv. (1930) pp. 2213 ff.—where, however, the Colinton relief has escaped the writer's notice—and to *Festschrift für August Oxé* (1938), pp. 164 ff.

### 3. GENERAL MELVILLE'S PAPERS.

The story of General Melville's discovery of the great camps in Strathmore has already been told in the *Proceedings*.<sup>5</sup> In 1917 Mr E. W. M. Balfour Melville, a collateral descendant, published a biographical sketch of the General, written by John Dougall, his secretary, appending to it a series of useful footnotes.<sup>6</sup> Some years later he placed in my hands a small package of the General's papers, dealing mainly with Roman

<sup>1</sup> *Journ. of Roman Studies*, vol. xxviii. p. 204.

<sup>2</sup> *Roman Wall in Scotland* (2nd ed.), pp. 428 and 433. Cf. *A Roman Frontier Post*, p. 140.

<sup>3</sup> *C.I.L.*, vol. vii. No. 1084.

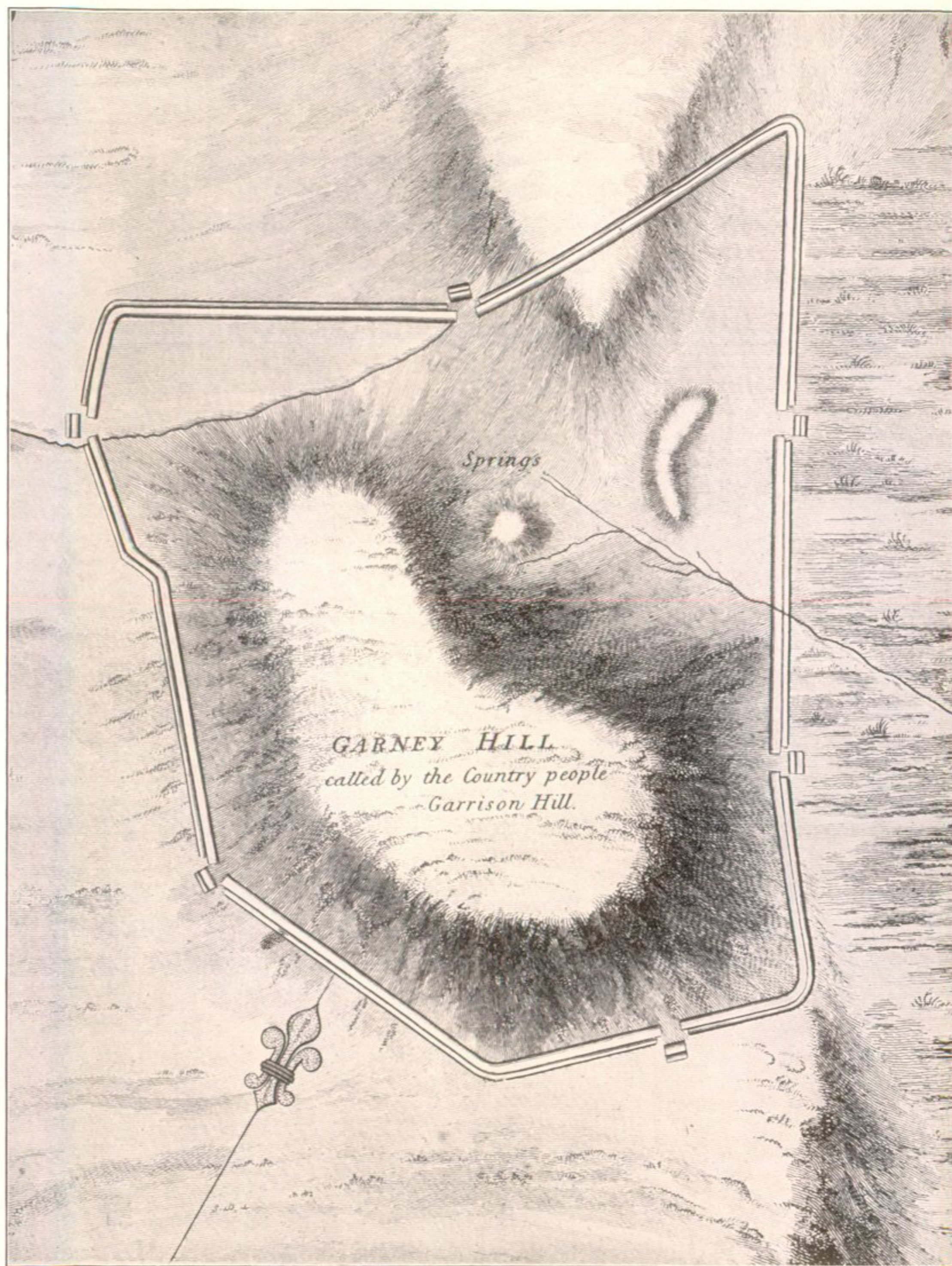
<sup>4</sup> *Proceedings*, vol. lii. (1917-18), pp. 38 ff.

<sup>5</sup> Vol. xxviii. (1893-94), pp. 48 ff. Cf. *Archæologia*, vol. lxxviii. pp. 169 ff.

<sup>6</sup> *Scottish Hist. Review*, vol. xiv. pp. 116 ff.



Altar found at Mumrills, 1937.



Plan of Raedykes, from Roy's *Military Antiquities*.





Scotland. My first impression was that two or three of them might be worth reproducing *in extenso*. A more leisurely examination, which has unfortunately had to be postponed until now, has brought a change of mind. A single, comparatively short one will be printed below. For the rest a very general description should suffice.

The most voluminous of the documents is the unfinished draft of a letter which Dougall intended to address to Mr Robert Whyte Melville, son of Mr John Whyte or Whyte Melville of Bennoch, the General's cousin and heir. The draft, which, though unfinished, covers no fewer than 25 closely written folio pages, is dated 12th July 1813. It shows that, soon after Melville's death in 1809, his successor had arranged that Dougall should prepare for publication in a sumptuous volume the numerous essays on military history and tactics which had been included in the legacy. A "List of M.S.S. &c. Lent Mr Dougall from General Melville's Library" bears date 15th June 1810, and throws an instructive light on the variety of topics that the proposed book would have touched upon. From the draft letter we can gather that Mr John Whyte Melville had himself died in the interval, and that his son was growing more and more impatient at what he regarded as Dougall's procrastination. Although the General had been dead for more than four years, and although the editor had from time to time been receiving instalments of the stipulated honorarium, the progress made seems to have been virtually negligible. There is nothing to indicate why the letter was never completed. We know only that the whole enterprise collapsed.

Dougall's defence of himself does not concern us here. It is more to the purpose to mention that a specimen of the contents of the projected volume has survived. It extends to 61 quarto pages of manuscript, and has on the outside the title, "Agricola's Camps in Scotland, by General Melville." The heading inside is "Appendix No. . . See Memoirs, &c., page . ." When carefully scrutinised, it proves to be little more than a rehash by Dougall of Melville's contribution to Gough's edition of Camden's *Britannia*,<sup>1</sup> the only fresh fact being a record of a *denarius* of Pertinax found in Fife.<sup>2</sup> In this connection considerable sentimental interest attaches to a much-worn half-sheet of notepaper endorsed, apparently in the General's own handwriting, "1754 Sketches of the Roman Camps near Brechin and near Forfar in Angus." On one side is a rough sketch-plan, with dimensions, of the camp at Battledykes, and another of a small oval fort " $\frac{1}{2}$  Mile West of Cloghton." On the other are similar sketch-plans of the temporary camp at Keithock and of the two Caterthuns.

It will be remembered that it was in the summer of 1754 that Melville made his memorable discoveries. It is, therefore, permissible to believe

<sup>1</sup> Vol. iii. pp. 414 \* ff. in the ed. of 1789, and vol. iv. pp. 158 ff. in the ed. of 1806.

<sup>2</sup> *Proceedings*, vol. lii. (1917-18), p. 238.

that these may be the jottings which he set down, if not actually upon the ground, at all events as soon as he had pen and ink within reach. The same may be true of three other half-sheets, with similar sketch-plans, endorsed respectively "1754 June Roman Station at Innerpeffry," "1754 Sketches of Entrenchments near Aberbrothick in Angus," and "July 1754 Remains of an Entrenchment on Downhead hill near Arbirlot in Angus." All these places would be included in Melville's itinerary of 1754, when he "made a walking tour through a great part of the country by the West Highlands to Fort William, across to the eastern shore at Fort George, and then southward to Montrose, from which through Angus westward into Perthshire, and thence returned to Edinburgh."<sup>1</sup> "Innerpeffry" is, of course, what is now usually called Strageath. The remaining two, like the Caterthuns and the fort near "Cloghton," are of native origin. There is no reason to believe that Melville thought otherwise. They have, therefore, nothing to do with Roman Caledonia, although the sketches may merit the attention of the Ancient Monuments Commission by and by, seeing that they represent the fortifications as they were nearly two centuries ago.

From the handwriting it seems evident that the descriptive notes on the back are not contemporary, but were added much later, and the conclusion is confirmed by a fifth half-sheet, endorsed "1754 Remains of Roman Camps near Ardoch," which has on the inner side a plan of the fort and the two temporary camps at Ardoch, very much as they appear on pl. xxx. of Roy's *Military Antiquities*. This cannot possibly be original, because Melville himself tells us<sup>2</sup> that it was Roy who discovered the temporary camps at Ardoch and that the discovery was not made until 1755. He must have had an opportunity of copying Roy's drawing long before it was published, for he was practically blind by the time the *Military Antiquities* appeared. That he should have had such an opportunity is in no way surprising, for the two were on friendly terms for many years and were both resident in London. Proof that they kept in touch with one another is furnished *inter alia* by a scrap of paper, obviously given or sent to Melville when he was contemplating a tour. He has endorsed it "1778 June. Note from Col. Roy concerning antiquities in the North of Scotland." There is a further endorsement by Dougall—"1778 Memorandum from Col. Roy for Travelling North." Inside are two lines in Roy's handwriting, "Barra Hill near old Meldrum"<sup>3</sup> and "Hills near Forres seem to have Entrenchments." Then follow jottings by Melville himself of one or two other places which he evidently thought of visiting. These include Burghead.

<sup>1</sup> Gough's *Camden*, *loc. cit.* The Strathmore camps were discovered later in the summer, in the course of a journey undertaken specially to look for them, as the result of a careful reading of the *Agricola*.

<sup>2</sup> *Ibid.*

<sup>3</sup> See *Proceedings*, vol. lxxi. (1936-37), p. 377.

Apparently the plan of a tour was never carried out. But from a booklet of 16 double sheets, octavo size and stitched in a cover, we learn that a journey of the kind had been made in the year preceding. The contents are entirely in Melville's own hand. Of the 32 available pages 6 are blank and 22 are occupied by "Cursory Heads of a short Trip made by Lt Gen<sup>l</sup> Melvill & John Whyte Esq<sup>re</sup> of Bennochty into the Shires of Angus, Kincardine & Aberdeen begun on the 19th & ended on the 30th of September 1777." The cousins went north by Brechin and Stonehaven and thence up Deeside and Braemar, returning by the Spittal of Glenshee. The "Heads," which are in diary form, are very slight, being little more than lists of the houses where they stayed and the people whom they met. They spent two nights with Lord Monboddo, who had entertained Johnson and Boswell four years previously and whose theories about primitive man drew from Johnson the sarcastic comment that he was "as jealous of his tail as a squirrel." Their host joined them in an excursion which they made with Mr Barclay of Ury to the camp at Raedykes, about the Romanity of which Melville had no manner of doubt.<sup>1</sup> He also accepted a Roman origin for a rectangular fort near Fordoun,<sup>2</sup> referring to it as follows: "There are very distinct remains of a Roman Castellum at present planted with firs abt 300 y<sup>ds</sup> east from the House of Fordoun—we paced it and it was about 80 in length & forty in breadth." I do not know whether this entrenchment has ever been looked at with critical eyes since, but, in view of Melville's opinion, it deserves more than a passing glance.

The remaining 4 pages of the booklet contain disjointed notices of the routes followed in journeying to and from Scotland in the years between 1776 and 1781, both inclusive. From these it may be gathered that the writer came at least as far as Edinburgh annually. He was also in correspondence with various people interested in Scottish antiquities, among others with Lord Buchan, the founder of our Society. The only actual remnants of this in Mr Balfour Melville's packet are, however, an original letter from John Gillies, the Perth bookseller and antiquary,<sup>3</sup> dated 7th July 1785, and five copies of letters from Melville himself, two of them addressed to Gillies in 1785 and three addressed to Captain Shand, the discoverer of Glenmailen,<sup>4</sup> two in 1788 and one in 1789. None of these is of any importance except the letter to Shand of 12th May 1788, long ago printed (from the original) in the *Proceedings*.<sup>5</sup> Save for the two documents to be dealt with under the next heading, the catalogue of papers is now almost complete, the only others being (a) a translation of a few sentences from

<sup>1</sup> It seems, therefore, that the uncompromising opinion he expressed in Gough's *Camden* referred only (as I suggested in *Proceedings*, vol. I. (1915-16), p. 319) to the connection of the camp with the battle of Mons Graupius.

<sup>2</sup> Chalmers, *Caledonia*, vol. i. p. 177, footnote (d).

<sup>4</sup> See *Proceedings*, vol. lxxi. (1936-37), pp. 373 ff.

<sup>3</sup> See *supra*, p. 244.

<sup>5</sup> Vol. vii. (1866-68), pp. 29 ff

the *Agricola* made on 22nd March 1778; (b) what appears to be the beginning of a list of phrases from Livy in which words like *acies*, *agmen*, *pugna*, *legio*, and so on occur; and (c) a set of notes, dated 14th October 1773, and endorsed "Sketches from Armstrong's Map of Northumberland relative to Fields of Battle"—all three eloquent of that devotion to the study of military history which was the consuming passion of Melville's life.

#### 4. THE ROMAN CAMP AT RAEDYKES.

In describing the excavation of this camp more than twenty years ago, I endeavoured to clear up the confusion in which the different plans

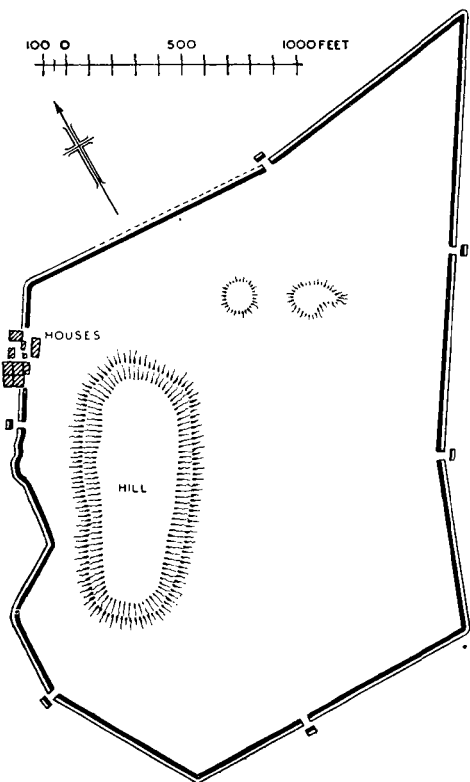


Fig. 1. Plan of Raedykes, made in 1778.

Danes were pursued to their ships with great slaughter. There have often been discovered among the sand of the seashore, human bones of an uncommon size. There is a secure Bay & commodious Harbour at Stonehaven.

Some are of opinion that this was Agricola's Camp, from which he attacked Galgacus. *Vid.* Tacitus de vita Agricolaë.

published in the latter half of the eighteenth century were involved.<sup>1</sup> Two documents in Mr Balfour Melville's packet throw fresh light upon the matter. If the first of them adds a further element of mystery, the second finally settles what was, after all, the most important point at issue—the source of Roy's information.

The earlier, unfortunately anonymous, is entitled "Plan of a Camp called Rê-dykes, on the Grampian Hills near Stonehaven, survey'd, August, 1778." It is reproduced in fig. 1, letters referring to a series of descriptive notes at the sides being omitted, as the notes are hardly relevant to our purpose. Beneath are the sentences:

The universal tradition of the country is, that this was the Camp of the Scots, previous to an engagement with the Danes, which certainly happened near this place. The Battle is said to have been fought hard by Stonehaven & the

<sup>1</sup> *Proceedings*, vol. 1. (1915-16), pp. 319 ff.

On the back is endorsed, in Dougall's handwriting:

This plan & description was sent by Robert Barclay of Urie, Esq<sup>r</sup>, proprietor of the land, to General (then L<sup>t</sup> Gen<sup>l</sup>) Melvill, who had, with him, Lord Mondboddo, and John Whyte of Bennochty Esq<sup>r</sup>, in summer 1777 visited this Camp—and G<sup>l</sup> Melvill gave a copy of it to his friend Major General Roy in whose splendid posthumous work it appears.

That no reliance can be placed on the accuracy of the endorsement will be apparent from a glance at Plate LXXXI, which is certainly not a reproduction of fig. 1. At the same time the latter is quite unlike any of the other plans illustrated in my original article. The date and the fact that it was in Melville's possession suggest that it may be that of Professor Stuart, since we know that his survey was made in 1778 and that his plan was sent to the General.<sup>1</sup> On the other hand, fig. 1 differs more widely from the plan in the *Bibliotheca Topographica Britannica*, against which Stuart has no complaint to make, than it does from that in Roy's *Military Antiquities*, which he criticised adversely.<sup>2</sup> Moreover, the sentences quoted above indicate that the draughtsman believed Raedykes to belong to the days of the Scandinavian raiders, whereas Stuart considered it to have been the camp of Galgacus. The puzzle, I fear, must remain unsolved. Luckily the answer is of little importance.

The second document, however, is of considerably more interest. It is a double quarto sheet, the first two pages of which are covered with a note in Roy's bold handwriting. It is headed "Memoranda to Gen<sup>l</sup>. Melville concerning the Position of the camp of Rae-Dykes near Ury," and it reads:

The Sketch on transparent paper, which accompanys these Remarks shews the general & relative situations of some places near Stonehaven & Ury, as taken from the Map of Scotland.

From the plan and Mr Barclay's [*sic*] description it seems to me that the Camp is situated on the rising ground between the Farms called *Springhill* and *Eastertown*.

Query? Is the meridian, drawn on the plan of the camp, that of the Magnet, taken with a Theodelet or other such Instrument, or is it the true meridian found by any other means?

Into what Burn or Brook doth the little Rill run which rises within the camp?

In order to fix the position of the camp on our Map, if a Theodelet is placed on the top of the Garneyhill, and Bearings are taken from thence, to the principal places in the Neighbourhood, marked on the sketch, viz<sup>t</sup>. Stonehaven, Kirk of Fotressee, Margy House, Cowie Ness &c., the camp may be inserted into the Map, with sufficient accuracy.

Would Mr Barclay be so good as order these Bearings to be taken, and transmit them; or order the camp to be drawn in its proper situation on the sketch, & return it?

It is to be observed that the camp is not so large as that at Battle-Dykes

<sup>1</sup> *Op. cit.*, p. 325.

<sup>2</sup> *Ibid.*

near Forfar; nor even so great as the large one at Ardoch in Strathallan. It is undoubtedly one of Agricola's; probably one that he occupied after the Battle with Galgacus, and after he had made the Detachments which were sent on board the Fleet to sail round Britain.

If this conjecture be founded, the Field of Battle should still be sought for between Kiethwick & Ury, somewhere about Fettercairn or Montboddo.

On the third page is a postscript in Melville's hand:

N.B. The sketch on transparent paper is taken from the G<sup>t</sup> Map or survey plan of Scotland made from an actual survey by order of Government which was never published, but is just now in Col. Roy's possession, it is executed upon a scale of a 1000 yards to an inch.

The first of these passages confirms my surmise that Roy had never seen Raedykes, and that his plan is the result of a survey carried out early in 1785 under Mr Barclay's directions,<sup>1</sup> Melville being the intermediary, for there can be no doubt as to its being Plate LXXXI, which is the subject of the "Memoranda." That is made certain not only by the conspicuous position of the meridian, but still more by the presence of "the little Rill . . . which rises within the camp," a feature that appears in none of the other plans. In his will Roy put the manuscript map, of which Melville speaks, at the disposal of "a most gracious sovereign."<sup>2</sup> It passed into the King's Library and is now housed in the British Museum, where it is known as the Duke of Cumberland's Map. As I was a little puzzled by the spelling of the place-names in Roy's note, as printed above, I invoked the help of Mr John Allan, the Keeper of Coins, who kindly examined the original sheet from which the tracing was made. He tells me that there Fetteresso appears as Foteressy," that there is no "Margy House" or "Cowie Ness" but only "Margy" and "Cowie," and that the former from its position must be the modern Mergie.

##### 5. DEALGINROSS.

In the first set of these *Miscellanea*<sup>3</sup> I reproduced from the archives of the Perth Literary and Antiquarian Society a paper which had been read to the members in 1788 by Captain Alexander Shand. By the courtesy of Mr John Ritchie I am now able to print a "Plan and Description of the Roman Camp at Dalginross, from a Young Gentleman Residing in its Neighbourhood." There is no name attached, but the date is 1786. Fig. 2 shows the plan as Mr Calder has redrawn it from a tracing by Mr Ritchie. The relevant portion of the accompanying "Observations" runs as follows:—

Whether this be the Camp possessed by Galgacus and recorded by Tacitus, I know not, however it seems very ancient, for at the mark +,

<sup>1</sup> *Op. cit.*, pp. 330 f.

<sup>2</sup> *Archæologia*, vol. lxxviii. pp. 208 f.

<sup>3</sup> *Proceedings*, vol. lxxi. (1936-37), pp. 373 ff.

the water has carried away a corner of the camp, and at this present time it is 8 or 10 acres distant to the westward.

In the North Camp the better sort had staid, where there is a trench within a trench, the innermost of which is very much defaced. At the same mark, +, there is a tradition that the water was made to run eastward through the camp by a kind of subterraneous passage, but I imagine this

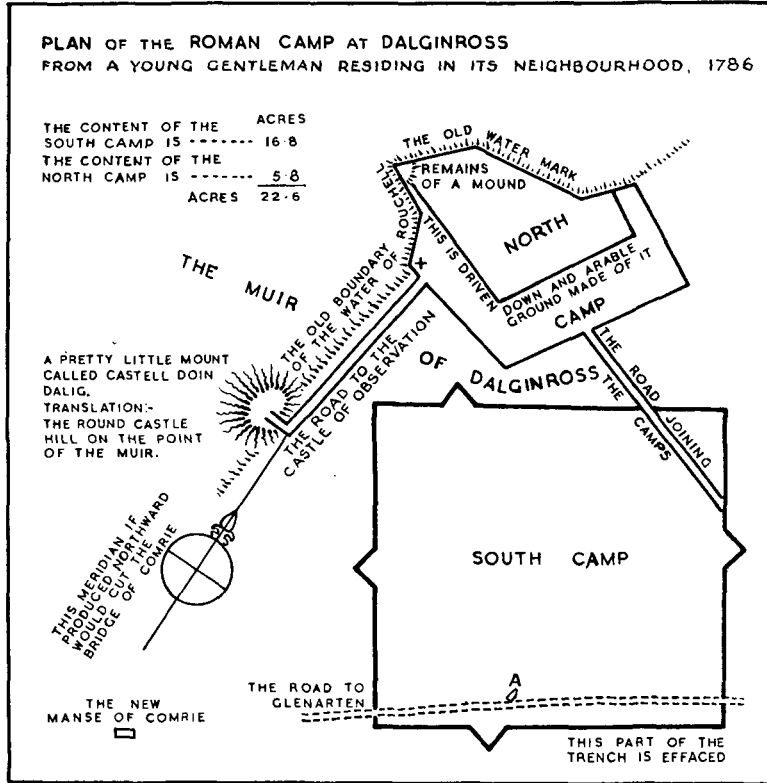


Fig. 2.

to be a fable, however the South Camp was supplied with water by a small aqueduct from Rouchell water which came east some where about the New Manse, and entered the Camp at the S. west corner.

These old roads which appear on the plan, are still very neat and perfectly regular, being caused on each side by a double row of stones to keep up the gravel.

A gold medal was found here, with the impression of Titus Vespasian on one side thereof, together with a Hammer and a Spear. At A there is a large hard stone 20.61 Square foot for a base and 8 feet high, and if I calculate right, it weighs 437 stones.

In view of recent discoveries at Fendoch and at Birrens, the references to aqueducts are interesting. The first of them may be less of a "fable" than the writer supposes; the "tradition" may well be founded on the

exposure of an underground channel. It is, however, the plan that deserves most attention. Except that the *claviculæ* at the gates of the South Camp have been misunderstood, it corresponds wonderfully closely to Roy's plan, which was executed in 1755<sup>1</sup> but not published until 1792. Until it was made, therefore, antiquaries had nothing more reliable to guide them than the very unsatisfactory plate in the *Itinerarium Septentrionale* of Gordon,<sup>2</sup> whose main concern with the site was to prove that it had been the scene of the battle with Galgacus. The "Young Gentleman" evidently regarded what he calls "Castell Doin Dalig," or "the round Castle Hill on the point of the Muir," as an integral part of the Roman defences, since it is plainly "the Castle of Observation" to which the road from the west gate of the smaller camp leads. In a later and less adequate plan, prepared in 1802 by "George M'Farlane, Land Surveyor, Comrie," which Mr Ritchie has been good enough to send for my inspection, it is designated "Toum Chastell or Castle Knoll." If it is artificial—it still exists—it may be a prehistoric cairn, for a "Court Knoll," which appears on M'Farlane's plan about 140 yards E. of the N.E. corner of the South Camp, was opened some time before 1807 and found to contain a cist, in which was an urn with cremated remains.<sup>3</sup>

#### 6. BIRRENS RECONSIDERED.

Almost forty-five years have elapsed since the Society brought its excavations at Birrens to a close. In the interval experience elsewhere has taught us all a great deal. Nevertheless, the plan then recovered still retains much of its original value. I doubt whether the younger generation, standing as they do on the shoulders of their predecessors, are in a position to realise what a remarkable achievement it was. Although Mr Barbour, the architect in charge, had no preconceived ideas as to what he was likely to find, and although from first to last he had no comparable plan to guide him, yet his professional skill and his power of acute observation, backed as they were by exceptional care and conscientiousness, enabled him to map out the internal arrangement of a typical Roman *castellum* with a completeness that had no parallel in this country in its day. It would, however, be unreasonable to suppose that, in the circumstances, he could have exhausted all the possibilities of exploration. Accordingly those of us who have followed the steady development of the technique of "digging" were very glad to learn that the Dumfries Society, under the energetic direction of Mr R. C. Reid, were proposing to reopen Birrens in the hope that an application of the newer methods might yield further information.

<sup>1</sup> *Archæologia*, vol. lxviii, pp. 171 f.

<sup>2</sup> In my copy it faces p. 40. Horsley's plan, though later, is for some inexplicable reason worse.

<sup>3</sup> This is stated in two other papers on Dealginross, likewise in the Perth Museum.



Substantial monetary assistance was voted by the Council of our own Society, and a Report on the operations of 1936 and 1937 has just been published in the *Proceedings*.<sup>1</sup> As illness prevented my attendance at the meeting at which it was presented, I must crave the indulgence of the Society to offer a few belated observations on it now, my excuse being that no small part of it is devoted, not to an objective account of what the spade revealed, but to an elaborate attempt to rewrite the history of Roman Scotland on somewhat novel lines. Mr Birley, who was in immediate charge of the work and has therefore acted as editor, is, of course, fully entitled to form his own opinions on the wider subject and to do his utmost to get others to accept them. At the same time the address to which his challenge is directed is so plainly legible that I could not remain silent except at the risk of serious misunderstanding. Nor can I help regretting that he should have approached his very difficult task in what can only be described as a controversial mood. He makes no secret of the fact that it was his dissatisfaction with the current interpretation of the 1895 evidence from Birrens which led him "to initiate the excavations there in order to prove [his] point" (p. 278). When people dig up Roman or other remains in order to prove points rather than in order to ascertain facts, experience shows that archæology is seldom the gainer.

Put briefly, his "point" was that the fort had been an integral part of the Hadrianic frontier system, and that, broadly speaking, its history must have been the same as the history of Bewcastle and Netherby on the west, and of Risingham and High Rochester on the east. It hardly needed his sketch-map to demonstrate that such a theory is *prima facie* reasonable enough. As a matter of fact, it is nearly thirty years since I myself wrote that "geographically, [Birrens] belongs as much to Hadrian's Wall as to Scotland,"<sup>2</sup> and some ten years later I actually suggested that the fort might have started life as an outlier of that formidable barrier.<sup>3</sup> Nor can I claim to have been a pioneer, for the idea was mooted at least as long ago as 1840.<sup>4</sup> So far, then, as the possibilities of the beginning are concerned, Mr Birley and I are in agreement. But we part company as to the ultimate end. Whereas I believe that the fort was probably abandoned not later than A.D. 200, Mr Birley insists that it was occupied throughout practically the whole of the third century as well as during a considerable portion of the fourth. In this he might perhaps have appealed to the powerful support of Horsley.<sup>5</sup> But Horsley knew nothing of the evidence of 1895, and the evidence of 1895 is vital.

<sup>1</sup> Vol. lxxii. (1937-38) pp. 275 ff. In order to save undue multiplication of footnotes, references to this Report are inserted, within round brackets, in their appropriate places in text or notes, as the case may be.

<sup>2</sup> *Roman Wall in Scotland* (1st ed.), p. 399.

<sup>3</sup> *Dumfries. Soc. Trans.*, 3rd ser., vol. viii. p. 78.

<sup>4</sup> By Hodgson, who was, however, misled by wrong information as to the provenance of an inscription (*Hist. of Northumberland*, pt. ii. vol. iii. p. 251. Cf. also *Birrens and its Antiquities* (1897), pp. 68 f.).

<sup>5</sup> *Britannia Romana*, pp. 67 and 115.

In 1911 I drew attention to the significance of the circumstances in which the fragments of the well-known dated inscription, bearing the name of Julius Verus, had been found by Mr Barbour.<sup>1</sup> Some of them were in the well of the Headquarters Building, and others were scattered about the surrounding courtyard. With the story of Bar Hill before my eyes, the inference seemed to me irresistible: the Building had never been restored after the demolition in the course of which the inscribed slab was torn from its place and smashed in pieces. In other words, the fort of A.D. 158, whenever it may have been destroyed, represented the last effort of the Romans to maintain their hold upon the position. Mr Birley dissents, and he bases his dissent upon two propositions, to neither of which can I believe that he would have committed himself had he looked more carefully at the remains of the slab about which he was going to write.

The first is that "if the inscription had still been in position when the fort was abandoned, its fragments should have been found fallen upon the street to the south of the *principia*, for its original position must have been in front of that building, where it could be seen by people approaching from the *porta praetoria*" (p. 282). There is not the faintest shadow of justification for the categorical "must." The statement is a pure assumption, utterly inconsistent with the appearance of the fragments themselves. Not only does the lettering stand out almost as clearly as if it had been cut yesterday, but one can still see quite distinctly—what can seldom be seen in inscriptions—the thin, light lines, never meant to be permanently visible, which have been ruled horizontally across the stone to enable the workman to keep the individual letters of uniform size. According to Mr Birley (p. 347), the slab must have stood on the outside of the wall, facing the street, from A.D. 158 to A.D. 196. If it had done so, it is incredible that, after exposure to the rains and frosts and winds of nearly forty Scottish winters, the sandstone should have retained its original surface virtually intact.

Still harder to reconcile with the archaeological data is the second of the two propositions, which runs thus: "It can only be concluded that the inscription had been re-used, as was often the case, as a flag or flags in the paved courtyard" (p. 282). Although "often" is rather an overstatement, it is unquestionably true that building-inscriptions sometimes came to such base uses. But it is quite impossible that anything of the kind can have happened in this particular instance. To serve such a purpose the slab must have been placed either face upwards or face downwards. After what has been said in the preceding paragraph, there is no need to waste words in insisting that the face cannot have been "trodden under foot of man" for close upon a hundred and forty years, as Mr Birley's hypothesis requires, his dates being from A.D. 205 to A.D. 340 (p. 347). And,

<sup>1</sup> *Roman Wall in Scotland* (1st ed.), p. 399.

even had the stone been laid face downwards, the lettering would have been to some extent blurred through friction against the underlying soil, as witness, for example, the inscription from the floor of Jedburgh Abbey,<sup>1</sup> and the later of the two from the barracks at Birdoswald.<sup>2</sup> Moreover, that it was not laid face downwards is conclusively shown by the condition of what would then have been the upper side. This is lumpy and rough-hewn, sufficiently "scabbled" to be suitable for building into a wall but not bearing the slightest trace of any endeavour to make it approximately level, not to say smooth, for walking upon. And what about the altar from the Sacellum, dedicated by the same unit and obviously contemporary, which was also found in the well and whose fortunes were evidently linked to those of the building-inscription? Are we seriously asked to believe that it too, measuring as it does 3 feet 4 inches by 1 foot 11 inches by 1 foot 7 inches, had been "re-used as a flag or flags in the paved courtyard"? If not, where had it been lurking for the century and a half between A.D. 196 and A.D. 340?

Speaking of the building-inscription, Mr Birley tells us that "analogies are not far to seek" (p. 282). He is right. Unfortunately, there is nothing very definite to be said about the first of the two which he brings forward, the broken slab from Rough Castle—except, indeed, that the position in which it was found suggests that its original place was inside rather than outside of the Headquarters Building, and that its appearance precludes the idea of its ever having been used as a paving-stone.<sup>3</sup> The analogy from Bar Hill, on the other hand, is probably a good deal closer than Mr Birley quite realised. In the first place, both inscriptions were found in the well of the courtyard, and each had for company the altar from the Sacellum. Although the well at Birrens produced nothing comparable to the 64 linear feet of columns from the well at Bar Hill, it is quite certain that in both cases the contents represented debris from the *principia*, thrown down by the wreckers when the fort was abandoned. In the second place, a fresh examination of the inscribed fragments from Bar Hill proves that the paving-stone theory would have as little to say for itself there as it had at Birrens; the lettering on the face is sharp and clear, the surface of the back is rough-hewn and lumpy. At both forts, therefore, the building-inscription was still *in situ* when the end came.<sup>4</sup>

<sup>1</sup> *Proceedings*, vol. lvii. (1922-23), pp. 173 ff.

<sup>2</sup> *Trans. Cumberland and Westmorland Soc.*, N.S. vol. xxx. pl. x.

<sup>3</sup> I can see no evidence for the positive assertion (p. 283) that it was the actual slab set up at the time of the erection of the fort during the governorship of Lollius Urbicus. We simply do not know.

<sup>4</sup> I say "the end" advisedly. It has been suggested to me that at Birrens the aqueduct, discovered by Mr Richmond in 1937, offers another way out: the construction of this may have rendered it possible to rebuild and restore the *principia* without clearing the well at all. But to leave the well in the condition in which it was found by the excavators in 1895 would have been an unheard-of piece of slovenliness, besides involving the neglect of an invaluable reserve of water. And, apart altogether from such considerations, the suggestion is ruled out by the fact that some of the fragments were scattered about the floor.

The parallel between Birrens and Bar Hill can, however, be carried further. According to Mr Birley (p. 283), "there can be no doubt" that the building-inscription from the latter should be dated to the governorship of Lollius Urbicus. When I first published it,<sup>1</sup> I might have been disposed to agree. Now that we know so much more about the Antonine Wall than we did then, I differ very decidedly. I believe that the Bar Hill slab and altar may safely be ascribed to the same period as the slab and altar from Birrens—that is, to round about A.D. 158 after Julius Verus had suppressed the rising which led to the temporary abandonment, not only of the Dumfriesshire *castellum*, but also of the whole series of *castella* along the line of the Wall. It is certain that the Second Cohort of Tungrians rebuilt Birrens then, while a couple of altars survive to show that the First Cohort of Germans, called Nervana, was the regiment that had preceded it there.<sup>2</sup> It is no less certain that it was the First Cohort of Bætasians whose memorials stood in the Headquarters Building at Bar Hill when the fort was evacuated for the last time, and these were presumably set up when that Cohort took the place of the First Cohort of Hamians, the names of two of whose commanders occur on inscriptions found on the site. It is natural to suppose that at both places the change of garrison, thus epigraphically attested, was part of a reshuffle such as might very well follow the crushing of the rebellion. It may be objected that the Bar Hill slab was dedicated to Pius, and that there is reason to believe that the whole of the forts on the Antonine Wall had to be rebuilt some time after his death in 161. Bar Hill, however, is not one of those that have yielded proof of three periods. Moreover, unless there had been a further change of garrison—and for such a change there is not a scrap of evidence forthcoming—a second rebuilding, if it did take place, would not necessarily have entailed any alteration even in the date of the inscription. In the second century the tendency in these matters seems to have been conservative.<sup>3</sup>

But it is time to return to Birrens. To get the perspective there right, it must be recalled that the Report of 1895 recognised two stages in the history of the fort—a "primary" and a "secondary." It was during the earlier of these that the *principia* was laid out in the position and on the lines which it was destined to retain during the period that followed; such changes as were introduced affected the internal arrangements only. We are all agreed about that. It is over the date when the "primary" period began that trouble arises. If he is to prove his point, Mr Birley must show that it was in A.D. 158, and not (as I prefer

<sup>1</sup> *Roman Forts on the Bar Hill*, pp. 82 ff.

<sup>2</sup> Cf. *Dumfries. Soc. Trans.*, 3rd ser., vol. viii. p. 81.

<sup>3</sup> An extreme instance is the precedent set by Hadrian, who either left or copied the original inscription on all the numerous buildings he restored. A conspicuous example of this is the Pantheon at Rome, which still purports to have been erected by Agrippa, although it had been twice destroyed by fire before Hadrian's restoration.

to think) fifteen or sixteen years earlier, when the Antonine Wall was built: it is essential that he should keep the "secondary" period in hand for his third-century occupation.

One of the two main arguments which he draws from the 1895 evidence is embodied in his paving-stone theory, of which I have, I venture to hope, disposed. The other may be summarised as follows: "In Roman forts in Britain buttressing is reserved for the walls of granaries, where it is normal, and of bath-houses, where it is occasionally employed. But at Birrens the walls of the 'primary' *principia* were buttressed, and so were those of the immediately adjoining *prætorium* or Commandant's House. Although such a phenomenon is unparalleled anywhere else in our island, 'in Raetia, and so far as I have been able to discover in Raetia alone, that is a common method of construction.'<sup>1</sup> Now it so happens that a detachment of the Second Cohort of Tungrians is recorded as being in service in Raetia about the middle of the second century and as having been withdrawn shortly before A.D. 158. It is reasonable to presume that they then joined their comrades in taking over garrison-duty in Dumfriesshire. Must we not believe that it was they who imported into Scotland the novel architectural idea which found expression in the buttressed *principia*, or Headquarters Building, and the buttressed *prætorium* erected in the new quarters of the Cohort? If so, there is no escape from the conclusion that the 'primary' period began in A.D. 158."

Formulated in these terms, the plea is ingenious and plausible, even although it involves the implication that the Romans looked upon buttressing as a form of architectural embellishment, whereas it was merely a practical device for strengthening walls that needed special support, for some such reason as the weight of the roof they had to bear or the instability of their foundations. On the other hand, when the premises on which the argument rests are examined, they are seen to be hopelessly unsound. Let me quote them in their latest form: "We know of no other fort in Britain where such buildings are treated in this way, but in Raetia there are several instances" (p. 282). The statement regarding Raetia is inaccurate and misleading, while that regarding Britain is untrue. This is a hard saying, but the facts do not permit of a judgement less uncompromising.

I will begin with Raetia. Here we are referred for proof to five *castella* in that province and two in Germania Superior (p. 282, footnote). A scrutiny of the plans of all seven results in a picture very different from that which Mr Birley conjures up. The search for a buttressed *prætorium* is everywhere fruitless. Only at Urspring and at Ruffenhoffen are there structures that could by any stretch of imagination be supposed to be such, and both of these are described by the excavators as granaries, a

<sup>1</sup> *Dumfries. Soc. Trans.*, 3rd ser., vol. xx, p. 162. That was written in 1937. By 1938 examples in Upper Germany (p. 382, footnote) had been discovered.

diagnosis amply confirmed by the accompanying text. The quest for a Headquarters Building remotely resembling that at Birrens is equally vain. At Urspring the outline of the *principia* is well preserved, but it is completely innocent of buttresses. At Ruffenhoffen, on the other hand, not a trace of it is left. Sulz is in almost as evil case, the remains there being fragmentary and the buttresses, if any, doubtful. At the other four forts there are certainly buttresses on the *principia*—one at Schierenhof, three at Murrhardt, four at Gnotzheim, and five or six at Niederberg. Except at Niederberg, however, where five are spaced at equal distances along the back wall, which plainly needed strengthening, all are planted quite irregularly at obvious points of weakness. Such is the sum total of the evidence for the assertion that Raetia provided a model for the Birrens *principia* and the Birrens *praetorium* with their schematically arranged buttresses, twenty-three—originally twenty-four—in the one case, and twenty in the other.

Turning to Britain, I can only suggest that Mr Birley's voyage of discovery must have been restricted to a very limited area. The number of *castella* excavated in our own island is, of course, considerably smaller than the number excavated in Germany, and one might have expected buttressed headquarters to be relatively fewer. Yet, by merely running my eye along my own bookshelves and picking out plans at haphazard, I have actually been able to muster more examples than Mr Birley cites from Raetia and Germania Superior combined. South of the Border he could have found buttresses against the walls of the *principia* at Gellygaer, at Elslack, and at Templeborough. Or, if he had ventured farther into the wilds of Caledonia, he would have seen them at Bar Hill, at Balmuildy, and at Camelon. The last-named site is specially instructive. Although at one point, if not at more, there are unmistakable signs of previous disturbance, the subsoil was evidently somewhat treacherous to begin with. That is plain from the fact that of the nine buttresses reared against the walls of the bath-building in the "South Camp," as many as four are afterthoughts. The building beside it, though incomplete, is clearly neither a suite of baths nor a granary, and yet it has had at least nine buttresses.

As a rule, the buttressing in Britain is obviously designed to serve the same practical purpose as the buttressing in Raetia and Germania Superior. Only in the "North Camp" at Camelon is there a hint of anything comparable to what Mr Barbour uncovered at Birrens. The *principia* of the Antonine fort there has been buttressed, and so has the building adjoining it on the south. In both cases the buttresses are, in the words of the Report,<sup>1</sup> "intermittent," but their position is such as to indicate that they may possibly represent the remnant of a regularly arranged series. It must be remembered that the ground within the "North Camp" had been

<sup>1</sup> *Proceedings*, vol. xxxv. (1900-1), p. 365.

intensively cultivated, and that sometimes the walls could be followed only by picking up the traces of the clay in which they had been bedded. In these circumstances it is easy to believe that more buttresses might have been noticed, if it had occurred to the excavators to look for them. They are not, however, essential. Even without them there is ample evidence to dispel the Raetian mirage.

We have next to consider what weight can be attached to such arguments in favour of a prolonged occupation as have been drawn (p. 279) from objects discovered at Birrens prior to the recent excavations. The gold coin of Constantius Chlorus, an imperfect description of which had misled both Horsley and Haverfield, is frankly and rightly abandoned. The piece of cut glass, too, tentatively assigned to the third century in the 1895 Report, is apparently surrendered (p. 279), although with a reluctance (p. 335) which it is not easy to understand, seeing that Dr James Curle, who was responsible for the original dating, is no longer prepared to defend it. The gap thus left is filled by three inscriptions "which, though they are not dated, should belong to the third century rather than the second." Since reading this explicit statement, I have several times scanned all three carefully in a vain endeavour to find any justification for separating them from their companions. I admit that I am no epigraphist, but my opinion, for what it may be worth, is fortified by the fact that to my personal knowledge Haverfield more than once scrutinised the whole Birrens group very closely without feeling it necessary to differentiate between the dates of its constituent members. However, it will be only fair to hear what Mr Birley has to say.

Let us take first what he calls "the altar set up by the architect Amandus in honour of Brigantia" (p. 279). The words suggest some doubt as to whether he has examined the stone itself or even the illustration of it in the 1895 Report. Had he done so, he could hardly have failed to notice that it is not an altar at all. It is a statuette of the goddess, standing inside a miniature temple.<sup>1</sup> No explanation whatever is vouchsafed as to why the "altar"—the word is repeated—"should belong to the third century," but we are told that the attribution is "confirmed by Mr S. N. Miller's convincing identification of the dedicator with the Valerius Amandus attested on a German inscription of 208."<sup>2</sup> To those who have no axe to grind, conviction may well come more slowly than Mr Birley would desire. Mr Miller himself is content to call the identification "possible," or, at the most, to claim for it "some degree of prob-

<sup>1</sup> "*Cippus in formam cediculae exornatus*," as it is put in the *Corpus*, to which Mr Birley himself refers us (*C.I.L.*, vol. vii. No. 1062).

<sup>2</sup> As Mr Miller points out (*J.R.S.*, vol. xxvii. p. 208), the earliest admissible date is the end of 209, when Geta became Augustus. He rests his case for the identification on the chance of there having been building at Birrens in 210. Matters are obviously not made any easier by Mr Birley's assumption of a reconstruction in 205 (p. 347).

ability," very properly reminding us that "Amandus is not an uncommon *cognomen*." It may be added that *cognomina*, like *nomina*, ran in families, and that, from the days of the ancient Egyptians onwards, technical professions have tended to be hereditary. This being so, is it not at least equally probable that—if the two were in any way connected—the Valerius Amandus, who was an apprentice architect (*discens*) with the First Legion Minervia at Iversheim in A.D. 209, may have been not the same man as, but a descendant of, the presumably full *arcitectus* Amandus who honoured Brigantia at Birrens? As things stand, then, the dedication really carries us no further.

Nor are "the two altars to Mercury" more helpful. Incidentally, they are not really "altars." They are pedestals, on one of which has stood a "*signum*" and on the other a "*sigillum*." "Style," we are given to understand, is the criterion by which a third-century date for them has been established. This is amplified by the explanation that "the complicated ligatures on one of them and the abbreviation of a rare *nomen* to its first three letters on the other cannot lightly be ignored." Those who are familiar with the originals, or with the illustrations in the 1895 Report, will not hesitate to take the risk. Rather, they will rub their eyes when they read of "complicated ligatures." On the stone in question<sup>1</sup> E & R are ligatured twice, while E & I and E & N are each similarly treated once, all three ligatures being of the very simplest form. Even so, the inscription is too long to be comfortably accommodated on the die, and recourse has accordingly been had to an entirely different but equally common expedient for economising space; one of the ligatures and six of the other letters have been cut very small, so small that four of them are made to nestle in the embrace of an immediately preceding C or G. There is nothing characteristic of the third century about these devices. In fact, on the surviving fragments of the slab of A.D. 158 there are five ligatures at least as "complicated" as any on the pedestal, while the whole of the letters in the fourth and fifth lines have had to be substantially reduced in size, and the R of HADR, which comes at the end of the first, has been cut sufficiently small to admit of its being placed inside of the D. The "abbreviation of a rare *nomen* to its first three letters" is, if that be possible, of still less value as an index of date. Whether he lived in the second century or in the third, the dedicator had the inscription carved for the information of people to whom his name would be as familiar as his features. He was not thinking either of the antiquaries who were to rediscover the pedestal in 1731, or of their more recent successors.<sup>2</sup>

<sup>1</sup> *C.I.L.*, vol. vii. No. 1069.

<sup>2</sup> I may be allowed to quote the opinion of Professor Collingwood, whose twenty years of labour in preparing a *corpus* of the Roman inscriptions of Britain have given him an unrivalled knowledge of their lettering. When the foregoing criticism was written, he was in the Far East and out of reach



So far, then, as the earlier evidence—whether structural or other—goes, Mr Birley's arguments do little or nothing to strengthen his case. Nevertheless, realising the danger of dogmatism, I endeavoured to approach his account of the excavations of 1936 and 1937 with a perfectly open mind. The task of doing so was not, I confess, rendered simpler by a perusal of the Preliminary Report.<sup>1</sup> I am unfortunately old enough to remember 1895 very well. I was then too much occupied with another branch of classical archaeology to interest myself actively in Birrens; but I heard much talk "about it and about," and I have a clear recollection of the almost superstitious reverence with which the remains of the actual handiwork of the Romans were regarded. It was, therefore, far from easy for me to accept a picture which represented Mr Barbour, whom I knew personally, as ruthlessly demolishing the walls which he had planned. Indeed, I cannot but think that, even at this distance of time, my feeling will be shared by all who care to look (or look again) at his own admirable Report.<sup>2</sup> A structure in the north gateway had certainly been removed bodily, but that was because it was, rightly or wrongly, supposed to be post-Roman.<sup>3</sup> Otherwise, I hesitated to believe that the ruins had suffered any damage beyond the inevitable disintegration that must have taken place while the trenches lay open, as they did for a good many months.<sup>4</sup>

When I came to read the fuller Report which is printed in the *Proceedings*, I was faced with the same tale of wanton destruction.<sup>5</sup> Before I had made much progress, however, I was afforded an opportunity of testing its accuracy. The operations at the western entrance are the first to be described (pp. 284 ff.). A full-page illustration (p. 285) shows two quite different plans, placed in juxtaposition and entitled respectively "West Gate 1895" and "West Gate 1936." The latter is a carefully measured drawing of a mass of stonework which was exposed by Mr Birley in the year named, and which Mr Richmond acutely recognised as the

of correspondence, but since his return I have been able to consult him. He is in full agreement with my estimate of the value of Mr Birley's specific arguments in favour of a third-century date for the three inscriptions under discussion. On the general question he writes that the Birrens inscriptions form a more or less homogeneous group, some of which may be later than others, although all of them must belong to a period extending from c. A.D. 150 to c. A.D. 225. "The building-slab, however, is firmly dated to A.D. 158; none of the others is very far away from it in style; the group as a whole represents a local and unbroken tradition of workmanship and design; it is very difficult to suppose that such a highly individual tradition could survive a major disaster c. A.D. 196; therefore, failing any positive evidence for placing any of them after that date, one is inclined to place them all before it, if about that date Birrens suffered a major disaster. . . . I do not see how anyone can distinguish, say, Marcus Aurelius from Pius on the one hand and Severus on the other. I feel it quite possible that all this group might be M. Aurelius."

<sup>1</sup> *Dumfries. Soc. Trans.*, 3rd ser. vol. xx. pp. 57 ff.

<sup>2</sup> *Proceedings*, vol. xxx. (1895-96), pp. 109 ff.

<sup>3</sup> *Op. cit.*, p. 102.

<sup>4</sup> Work began in May 1895, and the trenches were not filled in until the following January; see *op. cit.*, p. 91.

<sup>5</sup> "As we were to find elsewhere at Birrens, the remains had been badly robbed after their planning had been completed" (p. 290).

rubble filling of a timber framework, such as has frequently been observed in the gateways of Roman forts abroad. The former is an enlargement to five or six magnitudes of the gateway shown on Mr Barbour's plan. On the strength of this enlargement, supported by a quotation from the 1895 Report to the effect that "the masonry was good, the stones, of various sizes but generally small, being squared and well fitted in bonded courses,"<sup>1</sup> we are informed that "the gateway structures planned in 1895 had been wholly removed after the planning of them had been completed" (p. 284).

At first the conclusion seemed inevitable. For the reasons already explained, however, I was a little reluctant to take it at its face value, and accordingly I thought it well to scrutinise somewhat narrowly the process by which it had been reached. Beginning with p. 285 I could not but recall a warning once given me by an architect of experience: a good plan by another hand, he told me, could always be reduced with safety, whereas the converse process was never free from the risk of distortion, no matter how exact the original might be. Then, when I turned to the 1895 Report, I noticed that Mr Birley's quotation was incomplete. The whole sentence runs: "The remains were only from a foot to 18 inches high, and rather ruinous, but where best preserved the masonry was good, etc. etc." It was obvious that this might quite well have been written of the rubble filling, the phrase "where best preserved" referring merely to the facing.<sup>2</sup> At this juncture I asked Mr Richmond—without giving him any hint of the object of my question—at what depth the rubble filling had been encountered. When he replied that it "was on a level with the bottom of the rampart," I knew that my suspicions were well founded, for these are the *ipsissima verba* of the 1895 Report. I thereupon told him what was in my mind, and he at once agreed that I was right. All that had been done in 1936 was to lay bare the stonework that had been carefully covered up by Mr Barbour more than forty years before!

That is a disquieting mistake, hardly calculated to inspire implicit confidence in what follows. It would never have been made if the 1895 Report had been thoroughly digested before the new digging had begun or while it was in progress. Nor is this the only sign which seems to point to a neglect of that elementary precaution. Thus, in the efforts to distinguish between "primary" and "secondary" walls, I can find no reference to those differences in methods of construction which Mr Barbour was careful to emphasise,<sup>3</sup> and I can see no indication of heed being paid to

<sup>1</sup> *Op. cit.*, p. 103.

<sup>2</sup> Since the above was written, the apparent inconsistency has been still further reduced by a letter which has reached me from a friend who saw the stonework in 1936 and who has now read the Report. While accepting Mr Richmond's explanation, he thinks the word "rubble" somewhat inappropriate. "The stones," he says, "were all hammer-dressed, so that their faces were rectangular."

<sup>3</sup> *Op. cit.*, p. 118.

his warning that, for reasons which he states, "the general tints in the plan probably embrace a considerable proportion of secondary work, which it has not been possible to show in its proper colour."<sup>1</sup> Readers who are conscious of these omissions will not feel altogether comfortable about the account that is given of the excavations undertaken at four points in the interior. I at least have found it uncommonly difficult to understand the various descriptions, nor am I at all certain that I have succeeded in doing so.

Any attempt at detailed criticism would therefore be futile. I must content myself with saying generally that, while still prepared to consider the question at issue without prejudice in one direction or another, I have so far seen no single fragment of structural evidence for the existence of a building that was necessarily later than the end of the second century. The photographs of three successive flagged floors (pp. 288 f.), for instance, leave me unimpressed. Elsewhere Mr Birley himself has argued cogently that the laying of one floor on the top of another may merely mean "renovation and not restoration after destruction,"<sup>2</sup> and in the present case, if he is right in thinking that the building concerned was a stable, renovation would undoubtedly be required, for no flagged floor could possibly have survived the stamping of horses' hoofs for periods ranging from nearly forty to more than ninety years. I am not sure, however, that those who share my hesitation about accepting the theory of a third- or fourth-century occupation will wish to take this way of escape. They may think that, even on the hypothesis of "one floor, one period," all three (Levels II-IV)<sup>3</sup> can be quite satisfactorily accounted for on the supposition that the uppermost belongs to the time of Julius Verus.

If so, it will be to Mr Birley's own work that they are indebted for the alternative. What is, to my mind, by far the most interesting and valuable result of his excavations is the recovery of positive structural evidence for two periods earlier than the "primary" of 1895. During the first of these (his Level I) some at least of the interior buildings were of timber. During the second (his Level II) the interior buildings were of stone, while the area of the whole fort was substantially smaller than it afterwards became. In his Preliminary Report he was disposed to date the timber period to the first century.<sup>4</sup> Without committing himself definitely, he now inclines to assign it to the reign of Hadrian. In either event he believes that the smaller fort lasted until A.D. 158, when it was rebuilt on a larger scale by the Second Cohort of Tungrians. The life of this enlarged fort (his Level III) he regards as covering the "primary" period of 1895, the "secondary" period (his Level IV) representing a "Severan reconstruction,"

<sup>1</sup> *Ibid.*, p. 114.

<sup>2</sup> *Arch. Ael.*, 4th ser., vol. vii. pp. 172 f.

<sup>3</sup> Level V is a purely imaginary reconstruction, the validity of which can only be maintained by flagrantly disregarding Mr Barbour's warning as to the reliability of the tinting.

<sup>4</sup> *Op. cit.*, p. 68.

which he supposes to have taken place *circa* A.D. 200, and to have remained in use during the whole of the third century. It will be time enough to refer to his Level V when structural proof of its existence has been obtained.

The chronological scheme just outlined is put forward on a provisional basis only (p. 290), but a moment's reflection will show that even on such a basis it cannot possibly stand, unless and until the evidence of the building-inscription has been disproved by some explanation more plausible than that embodied in the paving-stone theory. On an equally provisional but perhaps a less obviously precarious basis I would venture to put forward the following alternative as a "working hypothesis." It does not seem to be necessarily inconsistent either with such structural evidence as has so far come to light or with the little that we know of the historical setting.

*Period I, c. A.D. 80.*—Agricola's invasion; area of the fort unknown; timber used for some at least of the interior buildings.

*Period II, c. A.D. 120.*—Construction of Hadrian's Wall, with Birrens as an outlier; area of the fort uncertain, but smaller than at present; interior buildings of stone.

*Period III, c. A.D. 142.*—Construction of the Antonine Wall and re-occupation of the whole of Southern Scotland, with consequent enhancement of the importance of Birrens; fort enlarged to accommodate the First Cohort of Germans, which was *milliaria equitata*—that is, 1000 strong, with a mounted detachment.

*Period IV, c. A.D. 158.*—Revolt suppressed by Julius Verus; fort rebuilt on the same lines after a temporary abandonment, the First Cohort of Germans being relieved by the Second Cohort of Tungrians, which was likewise *milliaria equitata*.

How far this "working hypothesis" will stand the test of future investigation depends mainly upon the results of the further examination of the rampart. Mr Richmond's description of the three sections which he cut through it (pp. 302 ff.) is a model of careful statement, and his drawings give all the information that is necessary to make it intelligible. But he agrees that it is not yet possible to associate the history of the buildings with the history of the defences. Let us trust that what he calls "the remaining chance" of doing so will not be irreparably destroyed by over-hasty action. As matters stand, it is beyond cavil that the rampart which we know is of Antonine date, like the buildings which it enclosed, but that it has yielded unmistakable proof of two periods of construction—one when it had a breadth of about 20 feet only, and another when an extension some 10 feet wide was added at the back. The fact that the extension partly overlay an *intervallum* road left no room for doubt that it had been accompanied by a rebuilding of the interior.

Could one have stopped there, everything would have been plain sailing—the two stages of the Antonine rampart corresponding to the "primary"

and "secondary" masonry of 1895. It is not, however, quite so simple as all that. At the front there was evidence of some refurbishing which, as Mr Richmond points out, may or may not have been contemporary with the extension at the back. Further, there were indications that the extension itself had been reinforced by a later revetment which, at the one point where it was uncovered, seemed to take the form of a ramp for stairs. Was the addition of this revetment a change made in the course of an unbroken period of occupation, or is it part of a complete reconstruction of Severan or subsequent date? If the latter, it becomes imperative to remove the stumbling-block of the building-inscription by some reasonable explanation.

It will be a delicate matter to determine the full significance of the various changes and to correlate them with those that took place in the buildings in the interior. But if it can be done—Mr Richmond is evidently a little doubtful—we shall know very much better where we are. Another but a less difficult task will be to ascertain the precise purpose of the posts and horizontal beams of wood which Mr Richmond encountered in the body of the rampart—a discovery, by the way, which did something towards restoring my own sense of self-respect. Some years ago I drew attention to the record of a post-hole preserved in one of the sections of the rampart which had been made in 1895.<sup>1</sup> I am ready to admit that my tentative interpretation of its presence will hardly survive the emergence of the new data, and that it is far less probable than the one put forward in the Report (p. 305). Nevertheless, it was comforting to find that my suggestion that it *was* a post-hole had deserved a little more consideration than was implied in the observation that it was "based on a misunderstanding of the conventions employed by Mr Barbour."<sup>2</sup> The recognition of the aqueduct, already alluded to in a footnote, is another interesting advance. Now that it has been brought to our notice (pp. 306 ff.), we can all see that an installation of the kind must have been a most valuable, if not an indispensable, adjunct to a fort in which there would be several scores of horses to water.

Coming next to the finds, which can be dealt with much more briefly, I will begin, as Mr Birley does, with the section dealing with the pottery. Apart from Mr Richmond's two short contributions, to which I have just referred, this is likely to be the most permanently useful part of the whole Report (pp. 309 ff.). The description of the decorated Samian ware embodies some most instructive notes, as well as some admirable drawings, by Mr J. A. Stanfield, whom the Society will be glad to welcome as even an indirect contributor to its *Proceedings*. I doubt whether any one could usefully add to what he has said so clearly and so well. At the risk of being egotistical, however, I must register a mild protest against

<sup>1</sup> *Proceedings*, vol. lxxvii. (1932-33), p. 289.

<sup>2</sup> *Dumfries. Soc. Trans.*, 3rd ser., vol. xx. p. 67.

the odd misrepresentation of my own views to which the editor has given a prominence it hardly seems to deserve. Because I suggested that in all likelihood "the plenishing of the fort canteens was in the hands of some central authority,"<sup>1</sup> I am credited (p. 309) with assuming "that pottery was issued to regiments, and owned collectively by them," an "assumption . . . belied alike by what we know of the organisation of the Roman army and by the frequency with which vessels are found bearing the name of an individual owner."

On the question of ownership my view has been quite unequivocally expressed. Speaking of the Samian from the Antonine Wall, I wrote "that a fair number of the fragments, as well as a few of the pieces of coarser ware, have scratched upon them what is presumably the name of the owner, indicating that the vessels of which they had formed part were the private property of those who habitually used them."<sup>2</sup> For the rest, after referring to "some central authority," I added that "of the working of the organisation we know absolutely nothing." Perhaps I should have said "I" rather than "we." But Mommsen's discussion of the supply-system of the Roman army<sup>3</sup> shows how utterly inadequate was the information accessible even to him, and Egypt has so far failed to throw much additional light upon the matter. At the same time, although the evidence is too scanty to yield detailed information, inscriptions of a *copiis militaribus*, a *copiis castrensibus*, a *praepositus copiarum expeditionis*, and a *dispensator rationibus copiarum* create a very strong presumption that, however it was managed in particular regions, supply of materials was a government responsibility. That being so, to me at least it seems incredible that the responsible officials left either the officers or the rank and file to pick up or to replace their equipment from such chance pedlars as happened to visit the neighbourhood of the *castellum* where they were stationed. Nor is it out of place to recall that one of the grievances of the mutinous legionaries of A.D. 14 was the arrangement under which the cost of "*vestem, arma, tentoria*" was deducted from their pay.<sup>4</sup>

Furthermore, I was rash enough to say that under such a system as I had envisaged, individual soldiers would be more restricted in their liberty of choice than would ordinary townfolk who were free to buy for themselves in the open market.<sup>5</sup> That, I should have supposed, was the merest truism. But it is denounced as "clearly tendentious," and we are told that "the present small series of decorated Samian from Birrens provides an admirable example of the variety of sources on which troops in garrison at a single fort could draw." This is, no doubt, true. Yet the mere handful of surviving fragments must represent so infinitesimal a fraction

<sup>1</sup> *Journ. Roman Studies*, vol. xxv. p. 197.

<sup>3</sup> *Staatsrecht*, vol. ii<sup>3</sup>, p. 1031, n. 2.

<sup>5</sup> *Journ. Roman Studies*, vol. xxv. p. 197.

<sup>2</sup> *Roman Wall in Scotland* (2nd ed.), p. 458.

<sup>4</sup> *Tac. Ann.*, Bk. i. c. 17.

of the total number of dishes in use during the long years of occupation, that it would have seemed to me idle to expect them to provide evidence on the main issue. Mr Birley's insistence, however, has made me look into the matter, with the result that it appears to me quite possible that, after all, they do. The Birrens excavations of 1895 yielded several fragments of a very remarkable and distinctive variety of decorated Samian, unlike anything I have ever seen from any other British site, civil or military, although I have noted several parallels abroad.<sup>1</sup> To my mind that is suggestive of "block-purchase" rather than of "*negotiatores cretarii*." More striking still is the fact that of the forty-six fragments of plain first-century Samian from Newstead, bearing stamps, as many as eight came from the pottery of VITALIS, whose wares have not been found anywhere else in Scotland.<sup>2</sup>

The account of the coarse pottery is Mr Birley's own, and it is but right to begin by acknowledging the value of the service he has rendered in accumulating such a useful store of comparative material. It is to be hoped that he will carry out his expressed intention of publishing a *corpus* of the mortarium-stamps of Roman Britain. No one could do it better. This acknowledgment made, I must go on to say that I neither accept nor reject what he himself must regard as the main conclusion to be drawn from his study of the coarse pottery; if I mistake not, it is upon this that he chiefly relies for proof of his "point." I have not seen the fragments and, even had I done so, I know too little of the niceties of the subject to express an opinion that would carry weight. On the other hand, in the light of the criticisms which I have been compelled to pass on the handling of some of the earlier evidence, I feel bound to suspend judgement on the age of the potsherds for which Mr Birley claims a third- or fourth-century date, until his verdict has been confirmed by some one who is at least as knowledgeable in these matters as he is himself, and is not exposed to the temptation inherent in having a "point" to prove.

I think I can promise not to treat the consultant's judgment on the individual pieces with the same light-heartedness as Mr Birley displays towards Mr Stanfield's dating of one of the fragments of decorated Samian (pp. 312 f.).<sup>3</sup> On the other hand, should it agree with Mr Birley's, I cannot undertake to accept it as proof of a Roman occupation of the site in the third and fourth centuries, until the building-inscription has been got

<sup>1</sup> *Op. cit.*, p. 199.

<sup>2</sup> *Proceedings*, vol. lxxv. (1930-31), p. 442.

<sup>3</sup> Mr Stanfield attributes the piece to a Trajanic potter and dates it to c. A.D. 110-115. Readers of the *Journal of Roman Studies* do not need to be told that an admission that such a fragment had been found at Birrens would be fatal to one of Mr Birley's most cherished beliefs. They will not, therefore, be surprised to see the Trajanic potter transformed, by a wave of the editorial wand, into a "border-line potter . . . whose work may be expected on Trajanic and on Hadrianic sites." The only moral I draw from the incident is a confirmation of the views I have expressed elsewhere as to the dating-value of Samian ware (*Journ. Roman Studies*, vol. xxv. pp. 187 ff.). Useful as it often is, it is not an instrument of precision.

out of the way and until the evidence of the rampart has been securely tied to that of the structures which it surrounds. In the meantime, I would point out that much searching on Mr Birley's part has produced no more than what it would be an exaggeration to speak of as a sprinkling of sherds which can, in his view, be regarded as late. In this and in other respects the excavations at High Rochester, Risingham, and Bewcastle—Netherby is as yet unexplored—seem to have yielded exactly what Birrens did not. Consider, for example, the collection of hammer-headed mortaria and other late pottery from the first of these sites, now preserved at Alnwick Castle. It is unnecessary to say more on the subject for the present, but it is worth recalling that, as is proved by the quantities found in the native settlement on Traprain Law, a great deal of late Roman pottery continued to find its way into Southern Scotland long after Hadrian's Wall became the frontier.

The only other section of the Report which calls for any remark at this stage is that which deals with the coins (pp. 339 f.). Eleven were found in 1937 and one in 1936, and these are excellently described, so far as description was possible, by Mr W. Percy Hedley. All of them were, as he says, in poor condition, and only six could be deciphered. The most interesting thing about them was that, as I have been able to satisfy myself by personal examination, even among those that were illegible there was none that suggested a date later than the second century. Their testimony was, therefore, in complete harmony with the view, generally accepted since it was advanced by Haverfield forty years ago, that the numismatic evidence proves Scotland to have been evacuated by the Romans some time in the reign of Commodus, never to have its peace disturbed by them again except for the expedition of Severus. That view, of course, does not fit Mr Birley's novel reconstruction of our history, and he is fully aware that, unless and until it is upset, he can hardly hope to make many converts. Accordingly he attempts to disprove it (p. 343).

The principle itself he summarises with a fairness and lucidity that could not be improved upon:

Briefly, the key-stone of the current archæological interpretation is the absence of coins later than the time of Commodus from all Roman forts in Scotland, with the exception of Cramond; this absence is held to justify the view that, with that exception, none of those forts was occupied in a later period.

On the other hand, if his only reply has not been penned in ignorance of the facts, then "clearly tendentious" would be too mild an epithet by which to describe it:

At first sight the argument may seem a sound one; but it should be remembered that the total number of coins from the Antonine Wall is not



very great, and it may be useful to point to the case of Housesteads fort on Hadrian's Wall, where the excavations of 1878 produced as many as 129 coins which did not include a single one between the time of Commodus and that of Elagabalus; yet that fort continued in Roman hands until the close of the fourth century, and it has produced fragments of a Severan building-inscription.

In the first place, no one is in a position to estimate "the total number of coins from the Antonine Wall." When the older authorities say "*denarii* of Domitian, Trajan, and Faustina," or "*denarii* of Trajan, Hadrian, and Antoninus Pius," they may mean a single specimen of each or they may mean a score. In the second place, the principle which Mr Birley rightly calls a "key-stone" does not depend for its validity, as he seems to suggest, upon the evidence from the Antonine Wall. It was formulated before a single one of the forts on the Wall had been excavated, and all that their excavation has done has been to confirm the soundness of Haverfield's original generalisation. Apart from two doubtful attributions to Commodus himself, the latest coin from any of the Forth and Clyde *castella* is a *denarius* of Lucilla from Old Kilpatrick, which cannot have been struck after A.D. 183. In the third place, if there was any serious desire to apply the test of a "very great" number, why were not the figures from Newstead cited? There the 260 coins actually identified did not include even a single Commodus. The latest was a *denarius* of Crispina, which would be of approximately the same age as the Lucilla from Old Kilpatrick.

Thus much for the presentation of the Scottish side of the case. The presentation of the English side is even more unsatisfactory. If the comparison was to be of any value, it should surely have been explained for the benefit of the uninitiated that, whereas in Scotland a yawning gulf stretches from A.D. 183 to infinity, the blank at Housesteads extends over no more than the thirty-four years which represent the difference between the date of the solitary coin of Commodus and the date of the solitary coin of Elagabalus. It should further have been pointed out that the imposing total of 129 for Housesteads is arrived at by including 44 which were illegible. There is perhaps just a possibility that one or two of these might have done something towards bridging the gap. On the whole, however, that seems to me unlikely. Had it been so, Professor Bosanquet could hardly have failed to allow for the contingency, due warning of which would have been given by the size of the pieces in question. Moreover, the general complexion of his list <sup>1</sup> suggests that the whole 44 were later than Gallienus. We are thus in a position to set out the numismatic argument from the two sites in the only form that is at once fair and intelligible. As Mr Birley has chosen to bring in Scotland as a whole,

<sup>1</sup> *Arch. Ael.*, 2nd ser., vol: xxiv. pp. 297 f.

I add the figures from Newstead, the single Scottish fort which has produced numbers worthy to stand alongside of those from Housesteads.

	Coins earlier than A.D. 200.	Coins later than A.D. 200.
Housesteads	14	115
Birrens <sup>1</sup>	18	None
Newstead	260	None

I think there will be general agreement that the comparison with Housesteads has been "useful." I shall be surprised, however, if anybody believes that it does much to help Mr Birley's case. If his reading of the history of Birrens is correct, the garrison of this "outlier" of Hadrian's Wall must have known how to keep the pockets of their trousers very tightly buttoned up during the last century and a half of the occupation, the very period when casual losses of money seem to have been most common on the Wall itself. The contrast is even more noteworthy than that between the pottery from Birrens, as dated by Mr Birley, and the pottery from High Rochester; the complete absence of third- or fourth-century coins is just as significant as the absence of third- or fourth-century inscriptions. It is needless to analyse the evidence further, and equally needless to discuss the section of the Report devoted to Conclusions (pp. 340 ff.). As the foundations of the architectonic scheme of periods therein embodied have been undermined, the whole structure must inevitably collapse like a house of cards. If it is to rise from its ruins, it must be built up anew on a much more stable groundwork. It may be that this can be done. I am not foolish enough to close my mind to the possibility of that or of other surprises which Birrens may have in store. None the less I am satisfied that the attempt I have been considering, courageous though it be, must be pronounced a failure.

<sup>1</sup> I exclude the six that were illegible, even although their appearance suggests a second-century date. I further exclude the Germanicus found "near the camp of Middleby" in the eighteenth century, and also, of course, the *aureus* of Constantius Chlorus (*Proceedings*, vol. lii. (1917-18), pp. 217 f.). On the other hand, I include the worn bronze of Domitian "said to have been found during the excavations at Birrens in 1895 by one of the workmen employed there," from whose daughter it was acquired by Mr A. Cunningham of Larchcroft, Ecclefechan (p. 340), although I am compelled to apply Mr Justice Stareleigh's famous dictum to the *antoninianus* of Victorinus, which is credited with a similar history. I have no record of the discovery of a single specimen of this denomination in Scotland, although it was common enough south of the Border. Even the great Falkirk hoard of 1933 contained no examples (*Num. Chron.*, 4th ser., vol. xiv. p. 26). I need hardly say that my decision involves no imputation on the *bona fides* of the present possessor of the coin or even of the vendor. Mr Birley himself admits that "its credentials might have been better."