



I.—JOHN HORSLEY AND HADRIAN'S WALL.

BEING THE SECOND HORSLEY MEMORIAL LECTURE, DELIVERED
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BY PROFESSOR R. G. COLLINGWOOD, V.P.S.A., F.B.A.

When I accepted the invitation to deliver the second Horsley Memorial Lecture, I did not hope to throw new light on Horsley's work by research comparable to that which was so brilliantly done by the first Horsley lecturer, Sir George Macdonald, or to illuminate the circumstances of his life by a knowledge of his country and his times like that shown by our beloved and lamented late president, R. C. Bosanquet, in the paper that follows Sir George's lecture in *Archæologia Aeliana*⁴ x. So far as I am concerned, those two studies shall stand alone as a source for Horsley's biography; I have neither the wish nor the power to supplement them. My aim was, and is, a more modest one. As following Sir George Macdonald, it was natural and proper for me to concentrate on some one aspect of Horsley's work. I chose his study of Hadrian's Wall, partly in the hope that this subject would be especially interesting to a Newcastle audience, but partly also because it is a subject in which, even yet, Horsley has not received his due. Whether you look at his field-work or at his theoretical thinking, Horsley's work on the Wall is the best that has ever been done. Through neglecting to consult it, recent archæologists have had to rediscover for themselves, over and over again, things which stand in black and white on the pages of his book. This neglect, even when it has not thus directly hampered their own

work, has led them to underrate the greatest of the men who have made that work worth doing. A distinguished scientist said to me the other day, "What makes a subject important or unimportant is the kind of people who work at it." Hadrian's Wall is an important subject because great men have studied it. If it is to remain an important subject, the spirit of those great men must not be allowed to die. Horsley is the greatest of them; and his chapters on the Wall are the great classic from which all sound study of it should begin.

There are three of these chapters, nos. 7, 8, and 9 in book I; a mere sixty pages of text. Chapter 7 is mainly concerned with the ancient names of the Wall forts; chapter 8 with the history of the frontier-works; chapter 9 with their present state as visible in the field. The order of exposition often reverses the order of discovery; but the commentator must restore the original order. I shall therefore deal first with Horsley's field-work; as recorded in chapter 9. Then I shall return to chapters 7 and 8.

I. HORSLEY'S FIELD-WORK.

Horsley's work on the Wall was done at a time when several others were interesting themselves in the same subject. I need only mention the *Observations* on it in Gibson's *Camden*, Gordon's *Itinerarium Septentrionale*, and the *Iter Boreale* of Stukeley. The contrast between Horsley and all these other writers is startling; and the more closely it is studied, the more striking it becomes. The difference, when one analyses it, is not a mere difference of degree—Horsley's being immeasurably the best—but a difference of kind. These other writers were travellers, visiting ancient remains and recording what they happened to see. Horsley studied the Wall in a quite different way: as a field archæologist, approaching the remains with definite questions in his mind, and returning to them again and again until he could read the answers. If we thus dis-

tinguish between antiquarian travel and field archæology, we must recognize in Horsley's study of the Wall the first piece of field archæology done in this country, and in many ways the best.

The record of his field-work is contained partly in his text and partly in his map. In order to estimate that work at its true value, and even to decipher it, we must know something about the map. It was made not by himself (Horsley was a poor draughtsman and evidently did not trust himself as a surveyor), but, as he tells us, by his order. Crawford Hodgson identified the surveyor as George Mark, who may have been an assistant in Horsley's school at Morpeth. All we know about the method used is that it was a trigonometrical survey, in which 164 fixed stations were employed (p. 121).¹ They are not shown on the map; but from internal evidence we can discover something of the way in which the work was done. The distances from fort to fort were trigonometrically established. From Wallsend to Housesteads, they are given with a very fair degree of exactitude. After that, they become less trustworthy. The distance from Housesteads to Stanwix is two miles too short; that from Stanwix to Bowness $2\frac{1}{4}$ miles too short, an error of 16 per cent. Evidently the triangulation deteriorated in quality, perhaps through being less frequently checked, in the less-visited western region.

Intermediate points between the forts were generally triangulated or chained; we shall see an example in the neighbourhood of Benwell; but in many cases they must have been sketched in by eye, on the assumption that milecastle-intervals were roughly equal. This produced one disastrous result close to Whittledean, where Horsley mistook certain mounds for a milecastle, and his surveyor, instead of showing him that he was wrong by chaining the distance to the adjacent milecastles and thus demonstrating that there was no room for it, obligingly thrust them apart and compressed all his milecastles between Rud-

¹ These references are throughout to the pages of *Britannia Romana*.

chester and Halton Shields, so as to get it in. Regarded as a record of field-work, the map is a priceless document; as a piece of surveyor's work, it is a poor performance. A chain and compass traverse would have been better.

One consequence was that Horsley gives the total length of the Wall as 68 miles 8 furlongs—five miles short. Gordon had made almost the same error; I have been tempted to wonder whether Mark cooked his measurements so as to agree with Gordon. Its original height, with his usual caution, Horsley refrained from estimating; but on p. 122 he gives its breadth in three places: (1) near Harlow Hill, 7 feet 6 inches above the foundations; (2) at another place, unspecified, over 7 feet at 6 feet above ground; (3) near Bowness-on-Solway, nearly 9 feet. The first two represent the standard "narrow gauge" of about 7 feet 6 inches. It is, I think, generally regarded as doubtful whether at Harlow Hill² the superstructure of the Wall was built to this gauge or to the "broad gauge" of about 9 feet 3 inches (a gauge nowhere mentioned by Horsley); but the point should surely be regarded as settled by this testimony. The measurement near Bowness makes Horsley the first (though long unrecognized) discoverer of the "intermediate gauge" of the westernmost sector.³

Horsley was the first person to study the fabric of the Wall. "The inner part of this wall is filled after a remarkable manner. The filling stones are pretty large, and mostly broad and thin. These are always set edgeways, and usually not erect, but somewhat obliquely. Upon these the running mortar or cement has been poured, and by this contrivance (together with the great strength of their cement in most places) the whole wall has been bound as firm as a rock" (p. 123). He calls attention to the resemblance in this respect between the Wall and the fortifications of such Roman towns as Silchester. He dis-

² *Arch. Ael.*⁴ VIII, p. 311.

³ Recognized by E. Birley, *C. & W. Trans.*, N.S., XXXI, p. 144. In modern times this gauge was first recorded by Haverfield at Drumburgh in 1899; *ibid.*, O.S., XVI, p. 80 *seqq.*, with no reference to Horsley.

cusses the question where the Romans found their building stone for the various sectors of the Wall (*ibid.*). He even asks from which end it was built, and decides that it was begun from the west, because the easternmost milecastle is placed at less than the standard distance from Wallsend (p. 124). This argument shows an admirable grasp of method. I need hardly remind this audience that the same argument is used in the *Northumberland County History*, vol. XIII, pp. 537-8, to prove that the Wall between Wallsend and Newcastle is an addition to the original scheme.

This brings me to one of Horsley's most brilliant feats: the working-out of the milecastle-system. The idea of such a system had been familiar ever since the time of Camden; for it was he who quoted and applied to the Wall the words of a fourth-century writer, advising that frontier-works should be provided with towers a mile apart.⁴ But no one before Horsley used this idea as a clue to systematic field-work; although, partly perhaps because Camden's quotation had become a commonplace and partly because it had already been observed that ruined fortlets actually stood on the Wall at about that distance, the name milecastles had already become current in Horsley's day, having been used in Gibson's *Camden*.⁵ But no one else (except Gordon, who here as elsewhere was no doubt working on what Horsley told him) even troubled to count the visible milecastles, let alone inferring the positions of those which were no longer visible. This is what Horsley did; with the result which he himself thus describes: "I was pleased to find that there had been just eighty-one *milliary castella* upon Severus's wall, and consequently eighty intervals between the *castella*. So that if the Romans in a general way called every interval a mile . . . this might be looked on as a plausible reason why the historian should say it was eighty miles long" (p. 122).

Unfortunately for this pleasing result, Horsley's map

⁴ Ph. Holland's translation, p. 793.

⁵ In the *Observations on the Picts Wall*, added in ed. 2; vol. II, p. 193.

divides the Wall not into eighty of these reputed miles but into eighty-one. The eighty-two milecastles thus implied are made up as follows: milecastles regarded as coinciding with forts (namely at Wallsend, Newcastle, Carrawburgh, Great Chesters, Bowness), 5; milecastles which Horsley claims to have seen, 57; milecastles inferred, 20; total, 82. In order to explain this discrepancy between text and map, we must go into the matter in some detail.

The first category, milecastles coincident with forts, is easily disposed of. No discrepancy arises here. Actually, the coincidence at Carrawburgh is imaginary; and, to Horsley's credit, he leaves it an open question in his text: the milecastle, he says, "has either been very near the station . . . or just fallen in with it" (p. 145). At Newcastle the milecastle and fort are so close together⁶ that only a much more accurate survey than Horsley's could have suggested the possibility of their not coinciding.

Among milecastles actually seen by Horsley there is one which, in point of fact, was imaginary. And here again his text leaves the question open. Between 17 (Whittledean) and 18 (East Wallhouses) he saw certain mounds which he thought represented a milecastle, "but somewhat obscure, and of an uncommon shape; two of the sides being about double the usual length, namely those which lie east and west; and the south side of it reaches very near the north *agger*. . . . I believe this has been one of *Hadrian's* exploratory *castella*; but the north side of it falling in exactly with the line of *Severus's* wall, it has been used also as a *castellum* by him" (p. 141). I have already explained how Horsley's surveyor let him down over this imaginary milecastle, cooking his measurements to include it instead of demonstrating that there was no room for it. For purposes of reference, it may be numbered 17/18.

We have now a milecastle too many. But you will recollect that in the western part of the Wall Horsley's

⁶ cf. *N.C.H.* XIII, p. 498.

surveyor greatly under-estimated the distances. In this part, only one milecastle was visible; all the rest had to be inferred; and owing to this under-estimate Horsley inferred one too few. Between Stanwix and Bowness there should be fourteen milecastles; Horsley's map shows one visible and twelve inferred, thirteen in all.

So we are all square again. And this must have been the state of things when Horsley wrote the passage I have quoted. But after he had written it he made a discovery which, owing to lack of final revision, has left no trace in his text, but is duly recorded on his map. He saw no milecastles between no. 5 (Quarry House) and no. 8 (West Denton). The actual distance between these is 5,294 yards; and hereabouts, it is evident, his surveyor was doing some conscientious chain-work, for the distance shown on his map, as near as I can measure it, is 5,280 yards. Now, thinking of the Wall as $68\frac{3}{8}$ miles long, Horsley must have regarded the average milecastle-interval as about 1,485 yards. If then there were only two milecastles between 5 and 8, he would have calculated their average interval as 285 yards too long; if three, as 160 yards too short. His map was originally engraved to show two; but a third, on the left bank of Denton Burn, has been inserted as an afterthought, the usual abbreviation *cast.* being crowded into the plate in an unnatural position.

Why did Horsley change his mind, insert an extra milecastle in this length, and so destroy his entire scheme? There must have been some powerful motive. The real reason why the distance from 5 to 8 is so long is because of the so-called "Long Mile" of *c.* 2,058 yards, instead of *c.* 1,620, between 7 and 8, which was discovered in 1928 and 1929. It is in this Long Mile that Horsley has placed his extra milecastle, which may therefore be referred to as no. 7/8. What induced him to insert it, we can only guess; but it may have been something like this. Study of his surveyor's measurements told him that the distance from 5 to 8 was better suited for three intermediate mile-

castles than for two. So he went back to the ground and searched it again. He may have seen some faint trace of 7 on the top of Benwell Hill, and recognized that the distance from here to 8 was excessive; in other words, he may have discovered the Long Mile. That would lead him to search for traces of another milecastle somewhere near Denton Burn; and these he fancied himself to see at the place where still stands the well-known fragment now owned by the Newcastle Corporation. Evidently he was not sure enough of it to mention it in his text; but a map is different; you have to commit yourself one way or the other; so he put it in, but without the symbol indicating that he had identified it on the ground to his own satisfaction.

This, I admit, is conjecture. But one thing is certain: the existence of the Long Mile has somehow upset Horsley's calculation as to the total number of milecastles, and has led, though by what steps we cannot tell, to this afterthought destroying the scheme with which he had been so pleased. And this has happened too late to be recorded in his text; all that could be done was to introduce it as an engraver's correction in the appropriate plate.⁷

It was apparently Horsley who discovered the existence of the turrets. Gordon, indeed, mentions turrets and claims to have seen five of them; but Sir George Macdonald has already shown that Gordon's discoveries generally represent what Horsley had told him, and what Gordon says about turrets will, I think, convince any reader that

⁷ It may be worth while to append a schedule of milecastles, starting those which Horsley saw (or, in the case of 17/18, thought he saw) and bracketing those which he inferred. 1*, 2*, 3*; [4=Newcastle]; 5*, [6]; (Benwell:) [7], [7/8], 8*, 9*, 10*, 11*, 12*, 13*; (Rudchester;) 14*, 15*, 16*, 17*, 17/18*, 18*, 19*, 20*, 21*; (Halton;) 22*, 23*; 24*, 25*, 26*, [27]; (Chesters;) 28*, 29*, 30*; [31=Carrawburgh?]; 32*, 33*, 34*, 35*, 36*; (Housesteads;) 37*, 38*, 39*, 40*, 41*, 42*; [43=Great Chesters]; 44*, 45*; (Carvoran;) 46*, 47*, 48*, 49*; (Birdoswald;) 50*, 51*, 52*, 53*, 54*, 55*, 56*; (Castlesteads;) [57], 58*, 59*, 60*; (Watchcross;) 61*, [62], [63], [64], 65*; (Stanwix;) [66], [67], [68], [69], [70], (71 omitted); (Burgh:) [72], [73], [74], [75], [76]; (Drumburgh;) [77], 78*, [79]; [80=Bowness].

he was looking, not very skilfully, for something he had been told to expect. Horsley marks thirteen on his map, and in his text mentions three, two of which are not entered on the map. This shows that on the subject of turrets his text had not received its final revision when he died, and suggests that the whole subject was one of the last upon which he was working, and one on which we must not look for mature conclusions. This is borne out by other facts. His text contains no tabulated account of them, as it does of milecastles. The nearest approach to it is on p. 120, where he says that "the distance between two, where it was thought surest, was measured and found to be near 14 chains or 308 yards," suggesting a scheme of four turrets between each milecastle and the next ($1,485 \div 5 = 297$). Having conceived such a scheme, we should expect him to go over the ground verifying it: fitting the known turrets into their places and looking for others to fill the gaps, exactly as he did with the milecastles. It is plain that he never did this. And not only does the brief paragraph about turrets in chapter 8 (p. 120) read like a hasty insertion, it contradicts what ought to be a corresponding passage in chapter 9. On p. 120 it is said that in one place the distance between two turrets is one-fifth of the standard milecastle-interval. On p. 147 the same thing is said not about the interval between two turrets but about the interval between a turret (near Busy Gap) and the adjacent milecastle. Clearly the reference of these two passages is to the same fact. The map shows that the turret in question is Kennel Crag (36a); the distance to milecastle 36, which Horsley gives as 308 yards, is really 509. Either Horsley mistook a mound of wall-débris for a turret, or his surveyor was making a deplorable blunder. The second alternative is certainly the right one. Horsley was on his guard against mounds of débris (cf. p. 136, bottom), and he selects this turret as the one of which he is most entirely confident. We must, I am afraid, add this instance to the long score of misdeeds that must be laid to the surveyor's

charge. It was the surveyor's fault, either by gross error in measurement or (more likely) through misreading the entry 508 in his own field-book as 308, that led Horsley by generalizing from this one instance to propound a view of the turret-scheme so remote from the truth.

But we can hardly leave the matter there. Horsley was the last man in the world to publish a generalization based on a single instance without checking it by comparison with the rest of the evidence. Even granted that he was cut off before he could test his hypothesis on the ground, he might still have tested it on his own map. We can show, I think, that even this was prevented.

Thirteen turrets are entered on his map. The first is almost midway between milecastles 11 and 12, and therefore will not fit any scheme which makes the number of turrets per "mile" an even number. The second is almost exactly two-thirds of the way from 12 to 13, a little too far east for precision, but much nearer the $\frac{2}{3}$ mark than the $\frac{3}{5}$ mark. The third is a trifle over $\frac{1}{3}$ of the way from 14 to 15; again, nearer the $\frac{1}{3}$ mark than the $\frac{2}{5}$. The fourth is nearly midway from 15 to 16. The fifth is a trifle under $\frac{1}{3}$ from 19 to 20; but much nearer $\frac{1}{3}$ than $\frac{1}{5}$ or even $\frac{1}{4}$. The sixth is $\frac{3}{4}$ of the way from 22 to 23. The seventh is $\frac{1}{3}$ of the way from 28 to 29; the eighth is $\frac{5}{8}$ of the same distance, and must be badly displaced or else wrongly identified. The ninth is a little under half-way from 29 to 30. The tenth is between $\frac{1}{4}$ and $\frac{1}{3}$ of the way from 36 to 37. In the text it is called one-fifth; which makes us wonder still more whether the measurement there quoted was not a mere misreading from the surveyor's notes. The eleventh is just about $\frac{1}{3}$ of the way from 41 to 42; the twelfth, a trifle over $\frac{1}{3}$ from 45 to 40. Finally, the thirteenth is placed at just over $\frac{1}{6}$ of the distance from 78 to Bowness; it falls, therefore, pretty accurately at the $\frac{1}{3}$ mark between 78 and 79.

From these figures it appears that, of the thirteen marked turrets, seven fall at or very close to the $\frac{1}{3}$ or $\frac{2}{3}$

mark, and nine so close to these marks that anyone studying the map would ask himself whether that is not their right place. There is not one which falls, with any precision, into a scheme based on dividing the milecastle-interval into fifths. The evidence of the map is therefore strongly against the scheme Horsley propounded; and if he had been able to check that scheme by the map, he would certainly have realized that the evidence there stated points to a quite different scheme, namely that propounded by Percival Ross in 1904 and now recognized as the true one.⁸ Thus, in spite of his surveyor's shortcomings, it was only Horsley's untimely death that prevented him from settling the true system of the turrets; and had he been able to employ a better surveyor the truth would have been obvious as soon as his map was drawn or the materials for it tabulated. The fact that so many generations of archæologists failed to see the true turret-system

⁸ *Bradford Antiquary*, vol. iv, part 9, July 1904. Ross knew of twenty-one turrets; i.e. six had been found since Horsley's death, for two mentioned in Horsley's text alone must be added to the thirteen on his map. Ross finds a place in his scheme for every one of Horsley's turrets: and this can be done without difficulty by assuming a degree of inaccuracy in his map which we already know to be there. The hypothesis that he was deceived by mounds of rubbish is thus unnecessary: On this assumption, the turrets recognized by Horsley will be the following:

- (1) 8a (West Denton). "Not very far from" West Denton milecastle (p. 138; not in map).
- (2) 11b. Map; shown rather farther from Throckley than from Heddon milecastle.
- (3) 12b (North Lodge). Map.
- (4) 14a. Map.
- (5) 15a. Map; rather nearer 15 than 16.
- (6) 15b. Not in map; but "I thought I saw hereabouts the remains of two turrets" (p. 140).
- (7) 19a. Map.
- (8) 22b (Stanley). Map.
- (9) 28a (Walwick). Map.
- (10) 28b (Tower Tye). Map.
- (11) 29a (Black Carts). Map.
- (12) 36a (Kennel Crag). Map; text, p. 147.
- (13) 41a (Caw Gap). Map.
- (14) 45a (Walltown Crags East). Map.
- (15) 78a (Kirkland). Map.

Gordon appears to have seen, of these, nos. 9, 10, 11, 13, and 14. In other words, these were probably the turrets Horsley had found by 1725.

as implied in his map is merely another instance of the neglect in which his work lay for nearly two centuries.

The tracing of the Military Way was a problem which seems especially to have interested him. He describes it as a well-paved road with a general breadth of about 17 feet (p. 122). Between Wallsend and Newcastle he failed to discover it with certainty, but on careful search found "faint and obscure" traces of it in two places (p. 137). He notes it, but doubtfully, near Heddon-on-the-Wall (p. 139), and first identifies it with confidence near Harlow Hill (p. 140), after which it "begins to appear very plain and to measure about thirteen feet," and "now continues very conspicuous" (p. 141). Beyond Whittledean it mounts upon the north mound of the Vallum, where the supposed milecastle (17/18) leaves no room for it otherwise: where the two works gradually diverge it descends again and runs between them to Halton Chesters (p. 141). He does not notice the curious way in which from Carr Hill to Down Hill it follows the north berm of the Vallum. Beyond Portgate he describes it as running along the North Mound from milecastle 22 to 23, descending to touch the milecastle, and soon afterwards reascending again until no. 24. "It seems very curious," he remarks, "and I wonder it has not been more observed" (p. 143). On reaching Chesters he calls attention to the road which we now call the Stanegate. Westward from Chesters, he says, this goes to Newbrough and Carvoran and thence, he thinks, to Stanwix; at Chesters he regards it as crossing the North Tyne and so running north-east to join Dere Street. West of Chesters, he picks up the Military Way once more at Walwick, where on leaving the milecastle (28) it converges with the Vallum and ascends its North Mound as before, leaving it (as we can still see to-day) to reach the milecastle at Limestone Corner (30) and then rejoining it once more. At Carrawburgh it runs straight through the fort (p. 145); and beyond this point he notices a "smaller military way," running from

turret to turret. This mysterious feature still awaits explanation.

Towards Sewingshields, where the ground rises and the Wall and Vallum diverge, the Military Way begins to accompany the Wall along the crags; but it "does not follow every smaller winding of the wall, upon the tops of the precipice; but generally takes a shorter course, and passes along the slope of the hill from *castellum* to *castellum* in the shortest and most convenient line that it can" (p. 147). At Housesteads it passes through the fort and then continues in the same fashion, and he does not mention it again until he reaches the Irthing, where on the east side of the river he thinks it lies south of the Vallum, whereas west of Harrow's Scar he thinks it can be seen north of the Wall. In both cases his opinion is mistaken; but we must recollect that he gives it only with hesitation. He sees it again at Wall Bowers; for the last time, except possibly between Wormanby and Burgh-by-Sands (p. 156).

From the Wall I turn to the forts. Here again, Horsley's field-work is incomparably superior to anything that had been done before him, or was to be done for generations after him. He had, of course, an advantage over his successors, in seeing these remains before the great stone-robbings of the eighteenth and nineteenth centuries; but, unlike his contemporaries, he was able to make full use of that advantage. All I shall do here is to mention a few points in which his description of the forts is especially interesting. At Rudchester he notes not only the gate-towers and corner-towers, but the interval towers between them: two in the north side, one each in the east and west sides north of the *via principalis*, and others (he will not commit himself as to their number and position) in that part of the rampart which lies south of the Wall. This, I believe, is the first time that the interval-towers of a Roman fort were recognized, at least on any English site; and I do not think anyone describes them again until Clayton began his excavations. At Halton Chesters he

notes and describes the unusual shape of the fort, and records an aqueduct leading water to it from a spring near Portgate; the lead pipe in this aqueduct, he suggests, may have given rise to the old fable of an underground speaking-tube running along the Wall.

At Carrawburgh he describes the T-shaped junctions of the east and west ramparts with the Wall. At Great Chesters he records the multiple ditch on the west: he seems to have seen three ditches, for he speaks of a "double *agger*" as well as the rampart, where by *agger* I suppose him to mean the ridge separating one ditch from the next. At Birdoswald the interior buildings, which as a rule he describes somewhat sketchily, struck him as unusually good; he found their walls to be 28 inches thick and traced passages between them 32 inches wide. After this he identifies Castlesteads, and then takes into the series of Wall forts the small temporary camp, as Miss Hodgson and Mr. Richmond have lately shown it to be,⁹ at Watchcross. Westward from this point, he is the first to locate all the forts correctly. Even Gordon (who in spite of his six years' priority in publication was certainly using Horsley's material) failed to mention the site at Burgh-by-Sands as well as Halton Chesters. A general observation about Horsley's descriptions of fort-sites is that he never fails to mention the ruins of the *vicus*, and to say exactly where they lie. As a rule he does not describe these ruins in detail; but one remarkable exception is his account of Coventina's temple at Carrawburgh. "The buildings without this fort have been chiefly on the west side, where about a year ago they discovered a well. It is a good spring, and the receptacle for the water is about seven foot square within, and built on all sides with hewn stone; the depth could not be known when I saw it, because it was almost filled up with rubbish. There had also been a wall about it, or an house built over it, and some of the great stones belonging to it were yet lying there" (pp. 145-6).

⁹ *C. & W. Trans.*, n.s., xxxvi, p. 170.

We must next consider his account of the Vallum. He describes its profile and measurements with admirable precision; but with regard to its standard profile he has been misled into thinking the Marginal Mound a constant feature and hence interpreting it as the main rampart of the complex, or Vallum *par excellence*. What deceived him was, I suppose, the prominence of the Marginal Mound in just those places where the ditch (having in fact been cleaned out, an operation of which that mound is a by-product) is in particularly good condition. It was natural to regard such places as showing with peculiar clearness the original features of the work.

Horsley's genius for field-work nowhere shines more brightly than in his perambulation of the Vallum. The first notable feature is his decision that the Vallum never existed between Wallsend and Newcastle, but began at Newcastle itself (p. 137); contrast Gordon's foolish suggestion that the Wall had been built on its line and had obliterated it, or Stukeley's hallucination as to its existence between the Sally Port and Red Barns. Horsley first notes it opposite the milecastle at Quarry House (5) and thereafter keeps it constantly in view.

At Rudchester he made a discovery for which, I think, he has never been given the credit. Everyone knows that the Vallum has a trick of bending to the southwards when it approaches a fort, so as to embrace the fort within a re-entrant. When such a deviation of the Vallum was first discovered in modern times, by Haverfield at Birdoswald in 1896, he described such behaviour as "a new fact . . . which had been entirely unsuspected before."¹⁰ Next year he went to Rudchester to look for a similar deviation, and was surprised to find it visible on the surface of the ground. This seemed, he said, "not to have been noticed before." He was wrong. Horsley noticed it, and it is quite plainly drawn on his map. Moreover, it is drawn in such a manner as to suggest that Haverfield's plan of it, showing its shape

¹⁰ *C. & W. Trans.*, o.s., xiv, p. 413.

as that of a very shallow V, is incorrect, and that in reality it has the same shape as the now familiar deviations at Benwell, Halton, and Birdoswald.¹¹ Haverfield here as elsewhere followed the lead of the nineteenth-century antiquaries in neglecting Horsley. But even Bosanquet, writing on this sector of the frontier for the *Northumberland County History* in 1926,¹² failed to notice that this feature is entered on Horsley's map.

This is the only place in which Horsley discovered a deviation of the Vallum. Elsewhere he merely observes that it fades out on approaching forts, and assumes that in general it skirted them in contact with their southern defences.

Although in this respect Horsley's anticipation of modern discovery has gone unrecognized, in another respect he has received his due. When the crossings of the Vallum were noticed by Mr. William Hepple, and systematically studied by Mr. Simpson and Dr. Shaw,¹³ it was explicitly stated that, in part, this was a rediscovery, and Horsley's observations were quoted in full (*loc. cit.*, p. 353). Between Rudchester and Harlow Hill, he says, "the breaks in the north *agger*, which are remarkable both here and in other places, deserve to be considered. They look like gaps made for carriages; but whether they are really for this purpose, or whether stones have been wrought out of it for paving *Severus's* military way, I shall not pretend to determine. They are oft very numerous, continued for a good space, and within thirty or forty yards (sometimes more, sometimes less) one of another" (p. 140). He mentions them again a little to the west of Wallhouses: "the north *agger* is high by intervals, but has great breaks in it" (p. 141).

¹¹ See his map, sheet 2 (Benwell-Rudchester). On sheet 3 the same feature is drawn more as Haverfield shows it in *C. & W. Trans.*, O.S., xv, facing p. 178. But everyone familiar with Horsley's map knows that where a sheet of it professes to portray the works from A to B, the portions at the two ends of the sheet, east of A and west of B, are sketched in without pretence to accuracy.

¹² Vol. XII, pp. 26 ff.

¹³ *C. & W. Trans.*, N.S., XXII.

And again on the slope beyond the North Tyne: "there are also in this part of the north *agger* several breaks, as if they had been made for the passage of carriages, which I also observed in other places" (p. 145). These are the only occasions on which he mentions them, though his words imply that he saw them often enough to suggest that they might well be "a system extending throughout the length of the Vallum."¹⁴ He notices that where the Military Way runs along the North Mound they are absent (p. 145); but he nowhere mentions the corresponding breaks in the South Mound, or the causeways in the ditch. The causeways, it is true, are nowhere very prominent, and exist at all only in a few places; but I do not know why he failed to see the gaps in the South Mound, which are quite as conspicuous as those in the North; unless, having seen that there are none in the Marginal Mound, he assumed that there would be none in the South Mound, and never looked for them.

Where did the Vallum end on the west? From Horsley's time down to 1934, the orthodox view¹⁵ was that the Vallum did not extend beyond Dykesfield. Horsley saw it near Grinsdale and again, "very visible," between Wormanby and Burgh; but after the west end of Burgh he says it "appears no more with plainness and certainty" (p. 155). He says later that he is "pretty confident that it was not carried on so far as" Bowness-on-Solway, and that from Easton Marsh onwards "there is no appearance" of it (p. 156). Yet on the very next page he calls attention to a ditch south of Drumburgh Castle which he suspects may be the Vallum; and later writes: "I am rather of opinion that it has gone beyond *Drumbrugh*, and down to the *Solway Firth*" (p. 157). In 1934 this second opinion was proved correct; the Vallum was found, plainly visible on

¹⁴ *C. & W. Trans.*, N.S., XXII, p. 354.

¹⁵ There were dissentients. In 1877 Canon J. T. Fowler actually saw the Vallum at Glasson and Port Carlisle. Nor did he stand alone. *Durham Univ. Journal*, XXIX, 26-31; *C. & W. Trans.*, N.S., XXXV, pp. 216-7.

the surface, and tested by trenching, all the way to Bowness.¹⁶

Horsley does not often contradict himself. I have already shown that in one such instance, concerning turrets, the explanation is that his work was unfinished. This second case obviously demands the same explanation. It is very noticeable to an attentive reader that Horsley's field-work is at its best in the eastern half of the Wall, and that west of Carlisle he describes the remains in the style of a man who has visited them once,¹⁷ but has not gone back to them again and again to settle points over which he was doubtful. I believe that the whole of chapter 9 represents a document, summarizing the result of his observations in the field, to which after every new journey he added a sentence here and there. The final revision of the whole had to wait until his field-work was as complete as he desired to make it. Now, I feel sure that with regard to the westernmost sector this never happened. Proof of this is the fact that he never saw for himself the signal-stations on the Cumberland coast which had been noted by Gordon; all that he has to say of them is dependent on Gordon's work, and this debt is duly acknowledged (p. 134). Gordon's account of them is very poor; he does not say how many there are, or (with any explicitness) how they are disposed or how far they extend.¹⁸ This is not the kind of thing that could satisfy Horsley. I can only suppose that his visit to the Solway coast took place before the publication of Gordon's book in 1726, and that he was never able to go again. That was the journey on which he formed the

¹⁶ *C. & W. Trans.*, n.s., xxxv, pp. 213-20.

¹⁷ In describing Bowness he speaks of "the evening I was there" (p. 158).

¹⁸ "At regular Distances, some a Mile, some two, from one another, along the whole Coast of the South Side of the *Solway* Firth, reaching from the End of the Wall at *Bulness*, to the most westerly Promontory of that *Aestuarium*." What is this promontory? Herd Hill? If so, their extent as known to Gordon was only $3\frac{1}{2}$ miles, which is incompatible with the statement about their spacing. Or the northern extremity of Moricambe Bay? or St. Bees Head?

opinion that the Vallum went to Drumburgh and beyond; therefore the passage in which he says that it does not do so is presumably based on a hasty and partial inspection, not reaching so far as Bowness, at a still earlier date. On the face of it, this is probable; Horsley was already famous as the authority on the Wall by 1725.¹⁹

I have by no means exhausted the subject of Horsley's field-work. But I have said enough to show its extraordinarily high quality. His description of the visible remains is still the best in existence; and although it is not perfect, in the sense that he did not see everything that there was to see, it is very nearly perfect in the sense that it includes practically no mistakes. I have only noticed one blunder in it: namely the assertion that the road to Bewcastle joins the Wall at Carvoran instead of Birdoswald. Other mistakes have been alleged. For example, Bruce²⁰ accuses him of saying that at Wallsend "it is the western, and not the eastern, rampart that is continued down to the edge of the river," and that Gordon had already stated the facts correctly. The truth is quite otherwise. What Gordon claimed to have seen at Wallsend includes nothing at all resembling a fort. It consisted, as he describes it, of "a faint track of the Wall" 162 paces long, running north $12\frac{1}{2}$ degrees west, and then bending through a right angle and pointing south-westerly, and so going 135 paces to Cousins' House.²¹ Horsley, commenting on this very obscure passage, explains it (rightly, no doubt) as indicating that what Gordon saw was the west rampart of the fort and its junction with the Wall; and that this was all he saw of the fort. Even here, Horsley points out, he made one positive blunder. Had the west rampart really extended 162 paces from the junction with the Wall, it would almost

¹⁹ *Arch. Ael.* 4 x, p. 16.

²⁰ *Wall*³ 1867, p. 8.

²¹ *It. Sept.*, p. 70. Any interpretation of the passage (as so often with Gordon) must be conjectural. Actually, Cousins' House was about 335 yards from the junction of Wall and fort. But the spelling of place-names in Gordon's book suggests that his printer could not read his writing and that Gordon did not correct proofs.

have reached the river; but this, says Horsley, it does not do (it extends in fact only some 60 yards). Neither Gordon nor Horsley saw the spur-wall which runs from the fort's south-east corner to the river; but, apart from that, Horsley's description of the site is as good as Gordon's is bad. As for the point on which Bruce praises Gordon for observing the facts correctly, and blames Horsley ("who is usually so candid and accurate") for mis-correcting him, here are Horsley's words: "nor does this [the western] rampart reach to the river" (as it would, if Gordon's measurements were correct); and here Bruce's: "Horsley . . . maintains . . . that it is the western . . . rampart that is continued down to the edge of the river." Through such reckless misrepresentation was Horsley's name blackened by a generation incapable of appreciating his work.²²

II. THE NAMES OF THE FORTS.

Horsley's seventh chapter consists of a topographical commentary on the *item per lineam valli* section of the *Notitia Dignitatum*. This is introduced by a page of preface, and between the preface and the body of the chapter is inserted a digression on the Agricolan military way, identified with the North Mound of the Vallum. The substance of the chapter is early; Horsley's interpretation of the *Notitia* section was complete before 1726, and I suspect that it was written out at an early date in the form in which it stands printed; in any case it is pretty clear that the digression (as he calls it) on Agricola's road is a late insertion into a pre-existing text.

One of the many services rendered to Horsley's memory by Sir George Macdonald is his proof that the interpretation of the *Notitia* passage on the Wall which Gordon

²² To this generalization a partial exception must be made in favour of MacLauchlan. But even he might have learned much more from Horsley than he did.

printed in 1726 and Horsley in 1732 was not Gordon's work but Horsley's. Gordon must have heard it from Horsley's lips in 1725; he printed it in a garbled form as his own, without a word of acknowledgment; and Horsley's calm and complete exposition of it six years later, unaccompanied by any breath of complaint against Gordon, is one of the noblest existing models of how learned controversy should be conducted.

Before Horsley took up the problem, ideas about Romano-British place-names stood pretty much where they had been left by Camden. That father of English history is a man whose work stands far above either my praise or my censure. In this matter of nomenclature, as in all the others that he handled, he laid down once for all the lines on which posterity was to work. Of the four main sources—Ptolemy, the *Antonine Itinerary*, the *Notitia*, and the Ravenna cosmography—he showed how to use the first two and established in principle the conditions under which the third could be used, namely through inscriptions which, found at a given place and naming a given unit of garrison troops, might enable the historian to fix at that place the ancient name associated with the same unit in the document. This method, I say, Camden established in principle, for he actually used it in the case of the *Itinerary*. It is the more curious that he omitted to apply it in the case of the *Notitia*. The clue for doing so was actually in his hand. He knew that inscriptions had been found of Coh. i. Aelia Dacorum at Birdoswald and of the Ala Sabiniana at Halton; but it does not seem to have struck him that these enabled him to fix the sites of Amboglanna and Hunnum and that the other *Notitia* stations might then be looked for in order. Instead of proceeding on this method, he got himself into a blind alley by looking for phonetic similarities between ancient names and modern.

The clue which Camden neglected was first followed up by Horsley. He recognized the force of the Ala Sabiniana inscription at Halton (p. 105) and of those mentioning the

Dacian cohort at Birdoswald (p. 107); to these he was able to add an inscription of Coh. i. Batavorum at Carrawburgh, several of Coh. i. Tungrorum at Housesteads, and one of Coh. iii. Gallorum at Chesterholm (p. 106). This gave him five fixed points in the first twelve names of the *Notitia* list, and seven forts left over to claim the other seven. The argument, for those twelve forts, was now complete; and the subsequent discovery, before his book was published, of a slab naming the garrison of Benwell, gratifying though it was, did no more than corroborate it.

But the *Notitia* list contains eleven more stations after Birdoswald. Horsley's own field-work made it impossible to place all these (as the title of the section seems to require) on the Wall itself. West of Birdoswald he found six forts: Castlesteads, Watchcross, Stanwix, Burgh, Drumburgh, Bowness. Extrapolating his series, he identified these as Petriana, Aballaba, Congavata, Axelodunum, Gabrosentum, and Tunnocelum. In the absence of confirmatory inscriptions, it was a risk; but he tried to diminish it by emending the last to Itunocelum, with a reference to Ptolemy's name for the Solway. His detailed discussion of these six names is full of sound learning and acute argument, as for instance where he notes that Aballaba is singular in being garrisoned by a *numerus*, which would occupy a small fort, and that Watchcross is the smallest fort of the series; or where he observes that the cohorts he would locate at Burgh and Drumburgh are both already known in Cumberland; but Horsley must have seen, as the reader of to-day (wise after the event) can see, that these six identifications are much inferior in certainty to the preceding twelve.

He is now left with five names unidentified, Glanibanta, Alione, Brementenracum, Olenacum, and Virosidum. What was to be done with them? What he actually did was determined by Camden's identification of Alione with Whitley Castle. An inscription had been found there mentioning a cohort of Nervii. Camden published it as

Coh. iii Nerviorum and accordingly located Alione at the place where it was found; and it was only in 1911 that the truth came out. The inscription had been found and copied by Reginald Bainbrigg of Appleby; the copy which he sent to Camden gave the cohort not as Coh. iii Nerviorum but as Coh. ii; and Camden himself altered the numeral from ii to iii.²³ Horsley, relying on Camden's text (the original was in his time already lost, owing to the habit, not yet quite extinct, of carting inscribed stones about the country to adorn gentlemen's residences), was obliged to deal with these five names on the assumption that the second of them had been satisfactorily fixed at Whitley Castle.

The previous eighteen had given him an orderly series from east to west; these five must also therefore give a series in order, most likely in the same direction. One of them is fixed at a point eleven miles south of the Wall; therefore the series must run through that point, and is likeliest to run parallel to the Wall. This, then, is the hypothesis which Horsley proceeds to work out.

Glannibanta and Alione, he notes, reappear in the *Itinerary* (as Clanoventa and Alone) thirty miles apart, and with a place called Galava between them. Thirty miles east of Whitley Castle, if you go by way of Oldtown in Allendale, is Lanchester; and Oldtown, where Horsley found "some ruins and remains of antiquity" (p. 453) would be right for Galava. But the whole road from Oldtown to Lanchester is, as he confesses (*ibid.*), doubtful; and he seems hardly more certain of that between Whitley Castle and Oldtown. The equation of Glannibanta or Clanoventa with Lanchester is weak, being supported by nothing stronger than the syllable *lan*.

Bremetenracum he places at Old Penrith (p. 111). He finds nothing to support this, and has to admit that if it is

²³ Haverfield in *C. & W. Trans.*, n.s., xi, p. 359 and plate. Horsley discusses the inscription, pp. 250-1. He is not wholly satisfied with the text as published, but has no reason to doubt the numeral of the cohort.

right the fort changed its name, for in the *Itinerary* it is Voreda (p. 112). Olenacum and Virosidum must then be Old Carlisle and Maryport, again with no confirmatory evidence. And the implication, that these five places together made up a line of supporting stations to the Wall, is plausible only when one thinks of them as severally connected with it by north-south roads, not at all when one tries to conceive them as strung out (like the forts of the Wall proper) on an east-and-west road of their own, for there is no such road. Nor is it easy, when we have identified Glannibanta with Clanovenia, to dissociate Bre-metenracum from the *Itinerary's* Bre-metonacae, which Horsley places at Overburrow.

In short, Horsley's scheme for the last five forts in the *Notitia* list is wholly unconvincing. I confess that in my opinion that is to his credit. What he was doing was to build up an interpretation of the passage round Camden's version of the Whitley Castle inscription. That version, we now know, was falsified. Any theory logically based upon it must therefore be false. The better the logical structure, the more inevitable the falsity. The only way from false premisses to a true conclusion is through sophistical arguments. Horsley's arguments are never sophistical; there has never been a writer on these subjects whose thought has been firmer or sounder in its logical structure.

But logic is not only concerned with "linear inference" from premisses to conclusions. It is also concerned to build up a coherent whole of thought which, irrespective of its relation to the premisses on which it rests, carries its credentials on its face, as a convincing or acceptable theory. Where, in spite of sound reasoning, this does not happen, we may be sure that there is something wrong in the premisses from which we started. The unconvincingness of Horsley's theory about the last five forts in the *Notitia* list throws us back on the re-examination of his premisses. Somewhere in them there must be a falsehood. Now that

we know where the falsehood lies, we can scrap the theory without regret, and give Horsley nothing but praise for the way in which he constructed it.

This, too, must be said in Horsley's praise. He never attached undue importance to the results of such theorizing. He was a good enough scholar to know that the forms in which the ancient names have come down to us are often corrupt, and he had none of that pedantry which takes pleasure in using an ancient name for a site which has a modern one. In this he deserves imitation. We know by now that the ancient name of Birdoswald begins not Ambo-, but Cambo-, though we do not yet know how it ends. We know that the ancient name of Chesterholm is not Vindolana but Vindolanda. But it is a curious irony that the two Romano-Celtic names that are oftenest used in Northumberland to-day are certainly wrong. The Roman town at Corbridge was never called Corstopitum; the eighth fort of the Wall was never called Borcovicium, still less Borcovicus.²⁴ Everyone knows this, but Corbridge and Housesteads are not good enough for us. Let us by all means follow Horsley's lead in studying Romano-British place-names; but let us also follow him in distinguishing fact from theory, and calling places by the names by which they are called.

III. HORSLEY'S THEORY OF THE WALL.

A theory, in this connexion, means a way of combining two sets of data so that they fit into each other. We have, first, the remains themselves as we have become acquainted with them through archæological study; and secondly, the ancient historical texts referring to them. These are the two sets of data: The business of a theory is to bring them into relation, so that what is mentioned in the texts can be

²⁴ Corstopitum is not Celtic (*N.C.H.* x, p. 474); the reading of the *Itinerary* is corrupt, but no satisfying emendation has been offered. For Borcovicium (again not Celtic), Ravennas has Velurtion, and *E.E.* vii. 1041, implies a name beginning *Ver* (cf. Verlucio, *Iter* 14).

identified on the ground, and *vice versa*. But this way of putting it somewhat over-simplifies the facts. You would not expect your texts to mention every detail of the remains; consequently you must be allowed to dot the i's and cross the t's of the ancient writers, to supplement what they tell us by reading into their words implications which are not expressed there. Conversely, you may be sure that some things mentioned in the texts will have left no visible trace in the remains as they now exist before your eyes; consequently you must be allowed to reconstruct these remains in your head, in the hope that as so reconstructed they will tally with the statements and implications of the texts. This is as much as to say that the two sets of data which have to be fitted together are not rigid data, like the pieces of a jigsaw puzzle, but flexible data, like a foot and a shoe, which accommodate themselves to each other if you handle them skilfully. And the consequence is that the difference between a good historical theory and a bad one is not like the difference between assembling a machine rightly and assembling it wrongly, so that in the one case it will work and in the other case it will not, but like the difference between a good fit and a bad fit in shoes. You can force your foot into an ill-fitting shoe, but the two things irk each other; the foot blisters, the shoe goes out of shape. In the same way, bad historical theories can seldom be logically refuted; but we can feel and locate the strains which they impose on the delicate organism of archaeological thought on the one hand or the scholarly interpretation of texts on the other.

Horsley's study of the texts leads him to distinguish four periods to which wholesale constructive work on the British frontier might be ascribed: that of Agricola, that of Hadrian, that of Antoninus Pius, and that of Severus. According to Tacitus, Agricola built great numbers of forts in northern Britain, and on the Forth-Clyde line he built a regular string of them serving as a temporary frontier. If he treated the Forth-Clyde isthmus in this way, he might

have done the same with the Tyne-Solway isthmus; for his fort-building scheme was certainly systematic, and the instance of the Forth-Clyde line shows how his mind worked in planning the system. In spite of Tacitus's silence, therefore, we have a probability that Agricola built a line of forts across the southern isthmus. If he did, he must have connected them by means of a road; and accordingly we must look at the remains to see if they include a road and a line of forts which can be ascribed to Agricola.

Hadrian, according to the *Historia Augusta*, built a wall eighty miles long across Britain; and was the first to do so. It was a turf wall, for we are told in the same compilation that Antoninus Pius built a second turf wall; and the inscriptions found on the Forth-Clyde isthmus enable us to identify this with the Scottish barrier. Hadrian's must therefore be sought elsewhere.

Severus, too, is said to have built a wall across Britain; and "the testimonies, whereby it is proved that *Severus* built such a wall, are more numerous and strong than those which prove the same thing concerning *Hadrian*, or *Antoninus Pius*. And yet nobody questions the truth of this fact in *Hadrian's* case, and the inscriptions in *Scotland* will by no means suffer us to doubt of the other; why then should we make any question concerning *Severus's*?" (Horsley, pp. 117-8.)

The texts, in brief, tell of three walls. One, of turf, is the Antonine Wall in Scotland; Hadrian's, which by implication is said likewise to have been of turf, must therefore be the Vallum; the third, the Tyne-Solway stone Wall, is accordingly the Wall of Severus.

The outline of the theory is now constructed. Let us proceed to details. The stone Wall of Severus includes the forts as parts of itself, but these must be earlier than the Wall that connects them; for they yield inscriptions of the second century (p. 99), and the course of the Wall is so arranged as to accommodate itself to their positions, "making some turns with no other view, but to come up

to and fetch in a station" (p. 98); hence the forts belong either to the Hadrianic scheme or the postulated Agricolan one. But the milecastles are part and parcel of the stone Wall, and thus belong to Severus. And so does the Military Way; for, as Horsley himself ascertained, it is laid out so as to provide communication between the milecastles (p. 118). The ditch in front of the Wall obviously belongs to the same complex.

We ascribed the Vallum to Hadrian. Now the Vallum, as we actually have it, consists of the following works, all strictly parallel to one another, though not to the Wall: (1) the North Mound, on the top of which, as Horsley discovered, the "Severan" Military Way is here and there situated; (2) the ditch; (3) the Marginal Mound; (4) the South Mound. Previous antiquaries had regarded this complex as a single work, the *limes* of Hadrian. But Horsley rejected this view. He does not explicitly tell us why; but study of his text shows that he had two reasons: (1) the ditch, instead of being its northernmost element, comes south of one mound; (2) its choice of ground, neglecting northward command, is inconsistent with the theory. Horsley therefore analyses it into two distinct works. The Marginal Mound he regards as Hadrian's principal wall of earth or turf; it was only many generations after his death that archæologists learned to distinguish earth-work from turf-work by excavation. In front of this should lie its ditch; and so it does, immediately to the north. Behind it lies the South Mound, explained as a secondary earth or turf wall. This leaves the North Mound unexplained; but here comes in one of Horsley's most ingenious ideas. The North Mound, he supposed, is not a rampart but a road. Not Hadrian's road, for that would have been built behind the Vallum, not in front of it; not Severus's road, for that has already been identified; therefore a road belonging to another period, which must be the period of Agricola.

∴ The postulated Agricolan frontier is now beginning to

take shape. It consists of this road together with the sites it is designed to connect. What are these? Obviously the forts of the Wall. The forts are therefore assigned to Agricola; and the Agricolan system in its entirety consists of the forts as we know them *plus* the North Mound of the Vallum, serving as a Military Way to connect them.

Hadrian's frontier is a modification of Agricola's. The same forts continue in use. The Military Way connecting them continues in use, except perhaps from Chesters to Carvoran, where the Stanegate (assigned to Hadrian) provides a more direct route and makes it intelligible that the Agricolan road should have been allowed to fall into disrepair and even to be blocked, as it is at Limestone Corner, with great stones wrought out of the Vallum ditch. Hadrian made his Vallum strictly parallel with Agricola's road, and immediately to the south of it. Why to the south? Because the Agricolan road had often chosen sheltered ground at the foot of a south slope, "so if the ramparts and ditch had been north of it, they must have gone along the very side or declivity of the hill, leaving the upper part of it on the north. They might therefore rather chuse to make the vallum stronger, though this military way should be more exposed to the enemy" (p. 100). It would be easy for troops marching along this road to cross the Vallum and take up a position of defence, if attacked.

This hypothesis accounts in the most brilliant way for the Vallum's choice of ground. If we take, for example, the famous *crux* of its southward turn passing round Down Hill, all is easy if we assume that originally the North Mound was laid out by itself as a road, which would not unnaturally skirt such an eminence, and that the rest of the Vallum followed its lead.²⁵

²⁵ Bruce's observations on the Down Hill problem (*Wall*, ed. 1, p. 157; ed. 2, p. 124; ed. 3, p. 132) are typical of his relation to Horsley. In the first two editions he quotes him, but with contempt. In the third, he no longer even does him the honour of quotation, but conveys the impression that no one has ever thought about Down Hill before.

But in order that an hypothesis should be scientifically valid, it must not only explain difficulties; it must also be inherently probable. Horsley was therefore under an obligation to show (a) that the North Mound really was, to all appearances, a road; (b) that it was older than the rest of the Vallum.

(a) As to its being a road, he has various arguments. It is stony, and "in many places much broader than *Hadrian's vallum*" (i.e. the Marginal Mound) "seems to have been" (p. 99). Its choice of ground is everywhere suitable for a road; "for it either keeps ground that is a little higher than the adjacent, or else the skirt of a hill without climbing it" (p. 100). Lastly, "in some places, I remember, there is a small trench on each side, where the way lies lower, no doubt for draining the water off" (p. 100). The first two points are well taken; but I do not know to what he is referring in the third. It ought to be investigated.²⁶ His summing-up, that "it has as much the appearance of a military way as most other elevated ridges that are universally allowed to be *Roman* military ways" (p. 100); is on the whole not unjust. No doubt, it differs in appearance from the Military Way of Severus, being more raised above the surrounding ground and less well paved; but he explains this by suggesting that the engineers of the third century had different ideas about road-building from those of the second. "Where the country afforded gravel and sand, the more antient military ways were high raised, with these materials, and a mixture of stone, and well trenched on each side to keep them dry (as the military way from the station at *South Shields* is), but not so constantly

²⁶ The only case in which I know of such a ditch flanking the North Mound is at Brunstock, where the Cumberland Excavation Committee found a small ditch, 18 inches wide, running along the north edge of that mound. It is described by Haverfield in *C. & W. Trans.*, o.s., XIII, pp. 458-9, and by Mrs. Hodgson (more correctly), *ibid.*, XIV, pp. 390-1. Mrs. Hodgson's section of the main trench, showing this ditch at its north end, faces *ibid.*, XIII, p. 457; her original drawing shows that this little ditch had been filled with turf débris presumably slipped from the north kerb of the Mound.

and regularly paved; whereas the later ways were better paved, but not so much raised" (p. 121).

I suspect, however, that these were afterthoughts; and that what first led him to conceive the North Mound as a road was the relation between it and "Severus's Military Way." This is suggested by p. 99 where he argues that there must have been an Agricolan road; that the North Mound would fill the bill; and that the fact of Severus's road being so often built on the top of it creates a presumption that it was a road already.

(b) He had also to show that it was older than the rest of the Vallum. On this head he has two arguments to urge: (1) the assumption of its priority explains the course taken by the Vallum at Down Hill and elsewhere; (2) as compared with the other Vallum mounds the North Mound is particularly denuded, or, as he puts it, ruinous. This statement rests, of course, on his discovery of the now familiar gaps in the North Mound. Observing that they are absent from the Marginal Mound he argued quite correctly that the Marginal Mound (whose direct relation to the Vallum ditch led him to identify it as Hadrian's "principal *agger*" or Vallum *par excellence*) was later than the North Mound. Had he noticed that the gaps penetrate the South Mound as well, the whole theory would have collapsed: for this fact shows that the two breached mounds, the North and South, must be taken together and dissociated from the unbreached Marginal Mound, whereas Horsley's theory demands that the South Mound and Marginal Mound should be taken together and dissociated from the North. Another fact which Horsley might have noticed and which if noticed would have been fatal to his theory, is the intermittent nature of the Marginal Mound.

Why did Horsley not see these things? In the case of most field-workers the question would not arise; we are well accustomed to their missing even what is obvious. But Horsley's standard of field-work, as I hope I have convinced you, is not like other people's. And I think we are entitled

to say this at least: that if he did not see these features he did not look for them; in other words, he did not make a special perambulation of the Vallum thinking away the North Mound and studying the rest as forming by itself Hadrian's line of frontier-works. And this again suggests that his new theory of the Vallum was framed at a very late stage in the course of his work, like his scheme of turret-distribution, and that his untimely death prevented him from checking it on the ground.

If this is so, the fact will have left its traces in his text. I conceive his chapter 9, as I have already explained, to be a document that had existed in manuscript for many years, altered and expanded from time to time as new observations were made in the course of field-work. Chapter 8 was probably written all at once, when chapter 9 had reached a form which Horsley hoped would be more or less final; but even this might have been altered after it was written. The bulk of chapter 7 forms a single whole which might have been written either early or late. It was either written or thought out early enough to be used by Gordon. The work on the Wall which consists of these three chapters was well advanced when Gordon met Horsley in 1725. Even the engravings for it were then in existence, or at least the drawings for them; otherwise they could not have been so freely copied by Gordon in his *Itinerarium Septentrionale* of 1726. It was about 1727 that this work of Horsley's began expanding into a comprehensive work on Roman Britain.²⁷ It is likely that when parts II and III were relatively complete Horsley would go back and revise part I. That is to say, he would take up the study of the Wall again in the last year or two of his life. This revision was cut short by his death. The result would naturally be a distinction in the text between inserted passages expressing views formed during this revision, and a general body of statements expressing the views which he held previously.

²⁷ *Arch. Ael.*⁴ x, p. 17.

If this new theory of the Vallum had been tested by a complete perambulation of the remains, the results of this perambulation would have been worked into chapter 9 and the whole text of the chapter revised to suit it. This has not been done. In a general way, and apart from a very few passages to be discussed below, the text of that chapter is written from the point of view of the traditional unitary theory. This can easily be proved. According to the traditional theory the North Mound is an integral part of the Vallum complex, the northernmost member of Hadrian's earthwork. According to the new theory it is not Hadrian's but Agricola's, not a part of the Vallum but an independent work prior to the Vallum, not an *agger* but a road. It would not be like Horsley's clear thinking and precise wording to call it "Hadrian's north *agger*" when he had decided that it was really "Agricola's military way." The new theory of it had already been fully stated and discussed in chapters 7 and 8; why confuse a reader by reverting to a terminology implying a quite different theory in chapter 9?

But in that chapter the North Mound is mentioned thirty-eight times, and is never simply and without further explanation (as we should expect) called the old (or Agricola's) military way, as it often is in the two preceding chapters, but always "*Hadrian's north agger*," and always treated as an integral part of the Vallum. To this rule there are certain partial exceptions; passages where, though still called the north *agger*, it is interpreted as a road. Carefully considered, I think that these exceptions prove the rule; for all of them appear to be insertions, sometimes hasty and jarring insertions, in a pre-existing text.

(1) p. 139, middle. Near Heddon-on-the-Wall, Horsley thinks "that the military ways have here coincided, because the north *agger* is so large." This is expressed as a second thought, the first being that the Military Way is discernible close to the Wall. The insertion has been made at a late stage in the history of the text, when the explana-

tion of the North Mound as a road had won its way deeply into the author's mind; for everywhere else the phenomenon here described as the coincidence of the two military ways is described as the coincidence of Severus's military way with Hadrian's north *agger*, except in one other passage (p. 146, end) where for a whole paragraph together this same terminology is used (see below).

(2) p. 141, bottom. At Down Hill, the Vallum skirts the hill to southward, "thereby, one would think, rendering the *vallum* itself a weak defence at that part." Now comes the insertion: "If the north *agger* was the old military way, and prior to the *vallum*, there was nothing improper in carrying it on the south skirts of the hill; and then when the *vallum* came afterwards to be built (for a defence or place of retreat) they were under a kind of necessity to form it after this manner." And three lines more. Here the new theory is inserted, tentatively, as an hypothesis to explain the Down Hill anomaly.

(3) p. 144, middle. "And if the north *agger* was the antient military way from station to station, and not ruined before this" (the Stanegate) "was laid yet this is still the shorter and better, if the march was not immediately from station to station, but from the bridge" (at Chesters) "to any of the stations more westerly than *House-steeds*." Here the new theory is brought into relation with the Stanegate, which according to the theory is Hadrianic.

In the above cases the insertions are straightforward and easy to understand; we should not know them for insertions were it not for the contradiction between their terminology and that of the chapter as a whole. In the two following, the matter is more complex. The insertion in each case makes nonsense of the passage, unless we assume (what there is, I think, no other ground for assuming) that there was a stage in Horsley's thought when he conceived the North Mound not as Agricola's military way but as Hadrian's; and that these two passages were written during that stage.

(4) p. 145. "This" (the union of the Military Way with the North Mound) "is a strong proof [both] that Hadrian's work [and the north *agger*] was prior to that of Severus's" (read *Severus*) "[; and that the north *agger* was really a military way leading from station to station]." The words which I have enclosed in square brackets I take to be an insertion, designed to bring the text into line with the new theory. But they have been hastily fitted in, and make no sense. For if, as the new theory implies, the North Mound is the Agricolan military way and therefore older than the Vallum, the fact of its being overlaid by Severus's road does not prove the Vallum earlier than Severus. Cut out the bracketed words, and the sentence expresses an argument soundly based on the pre-Horsley unitary theory of the Vallum. Insert the first and third brackets, and it expresses a valid argument based on the assumption that the North Mound is Hadrian's military way. I suggest that Horsley at one time held that theory; and that he inserted the second bracket when he abandoned it in favour of his final theory (for the distinction of "Hadrian's work" from "the north *agger*" implies that the north *agger* is not Hadrian's work), not observing that in terms of the new theory the whole argument became invalid.

(5) There is a more extensive insertion on p. 146. Here the whole of the last paragraph is written from the standpoint of the new theory, for it consistently describes the union and separation of the Military Way and the North Mound as a union and separation of two roads. But *what* two roads? In order to answer that question we must examine one part of the paragraph, namely the following. He is describing the state of things west of the Coesike. "It" (the North Mound) "is mixed with stones, and no regular pavement appears; whereas *Severus's* military way after this parting appears little raised, but regularly paved. Both of them have the manifest appearance of a military way after they are parted. The continued separation of

these two ways is owing to the great distance of the walls from one another." Like the foregoing quotation, this, in its last sentence, makes nonsense. On Horsley's theory, it is not because the Wall and Vallum diverge that the early military way diverges from the later; on the contrary, it is because the Wall (with its road) diverges from the line taken by the early Military Way that it also diverges from the Vallum, whose course that early road determined. In terms of his own theory, therefore, Horsley is here putting effect for cause and *vice versa*. Either his mind was most uncharacteristically confused when he wrote this passage, or he wrote it with another theory in his head: namely the theory that the North Mound is a military way indeed, but not Agricola's military way but Hadrian's.

On the evidence of these two passages I am disposed to think that Horsley did at one time hold such a theory; and I imagine him to be arguing himself out of it on pp. 125-6, where he shows how well the Vallum's choice of ground is explained on the assumption that the North Mound existed as a road before the Vallum proper was made.

(6) I must mention one more passage: p. 141, middle. "Hadrian's north *agger* is visible also some part of this way" (between Harlow Hill and Halton Shields) "and afterwards very conspicuous; so that I wonder Mr. Gordon should intimate that it did not appear till beyond *Walwick*, or between that and *Carrawbrugh*." The feature of which Gordon intimates this in the passage referred to (*It. Sept.*, p. 74) is the Marginal Mound: "a third *Rampart* . . . upon the south bank of the *Fossa*." Here, therefore, Horsley is calling the Marginal Mound "Hadrian's north *agger*," a name which everywhere else in his book (including the sentences immediately preceding and following this one) is reserved for the North Mound. It might be argued that, once the North Mound has been by the new theory dissociated from the Vallum, the Marginal Mound (which Horsley generally calls either "Hadrian's principal *agger*" or, *par excellence*, "Hadrian's *vallum*") might succeed to

the title of "Hadrian's north *agger*"; and this, therefore, might be taken for a sixth case in which a phrase expressing the new theory has been inserted in the text. But I cannot think this; the wholly unexplained change of terminology is too violent, and not at all like what we find in the other passages. I think that the phrase "north *agger*" is simply a slip (rather on Horsley's part than his printer's) for "principal *agger*."

Thus the detail of the ninth chapter shows that both during Horsley's examination of the remains and during his writing-up of his observations in chapter 9 he still adhered to the old theory of the Vallum; that a very few passages embodying his new theory (or rather, if I am right, his two successive new theories) were inserted after the chapter was complete; and that he never tested the new theory by a special perambulation of the Vallum, which would have resulted in a much more drastic revision of the chapter, even if it did not prove fatal to the theory.

Let us now turn to the eighth chapter. This, composed no doubt after the ninth, is as a whole written, or at least revised, conformably with the new theory. But even here we can detect a first draft written before that theory was invented. For in one sentence he explicitly states the traditional unitary theory. "What belongs to this work," he writes, "is the principal *agger* or *vallum*, on the brink of the ditch; the ditch on the north side of the *vallum*; another *agger* on the south side of the *vallum*, and about five paces distant from it, which I call the south *agger*; and a large *agger* upon the north side of the ditch, called the north *agger*" (p. 117). This is identical with what we find, for example, in Gordon. But the next sentence contradicts it by saying: "this I suppose was the military way to the ancient *praetentura* of stations" [i.e. Agricola's]. The latter sentence was evidently inserted, after the idea came into Horsley's mind, in a manuscript already existing and written from the standpoint of the traditional theory.

As for the seventh chapter, I think it all dates from

before the invention of the new theory except for the long paragraph, called by the author a digression, on pp. 99-100. The same afterthought seems visible in his plate no. 11, facing p. 158. Here the title of the third figure unambiguously describes the North Mound as Hadrian's north *agger*; but the same feature is also described in smaller script as "the old military way or north *agger* of Hadrian," as if to correspond with both the old theory and the new. This platé, or the drawing for it, must surely have been used by Gordon as the basis for his plate 49 (facing his p. 74); and in that case he evidently saw it before its letterpress contained any reference to the old military way.

Other points of interest appear from comparison of the two plates. Gordon's shows the "smaller military way" and two milecastles, but no turrets; Horsley's shows four turrets between two adjacent milecastles. Horsley's shows the Military Way running on the North Mound, except where it diverges to reach a milecastle; in Gordon's the coincidence of road and mound is not shown, nor is it mentioned in his text. I infer that Horsley's intensive study of the Military Way (though not his discovery of the "smaller military way") took place later than 1725. If I am right in thinking that this was what led him to conceive the North Mound as another military way, first Hadrian's and then Agricola's, and that this conception is among the latest features of his text, his study of the Military Way must have been undertaken in the last year or two of his life.

If Horsley had lived a few years longer, he would not only have discovered the true system of turrets, he would also have checked his theory of the Vallum on the ground and discovered that it would not work. It was his early death, carried off by a stroke at forty-six, that deprived him of these crowning triumphs and left his work in the incomplete state in which we have it. Great as it is—the one and only complete book that ever has been written or ever will be written on its subject—the *Britannia Romana* is unfinished work, the kind of thing which fools and bairns

should not see. That is why Horsley's reputation with posterity has never equalled his deserts. The true greatness of his incomparable volume only begins to appear when we study it critically and use its incompleteness as a clue for detecting the movement of his mind and the stages by which his thought developed. When we do this, we find that the theory of the Wall associated with his name is only one phase in a long process of interpretation and re-interpretation; we can see it crystallizing in his thought, and we can even see the points at which, if he had been permitted to go on thinking, it would have begun to dissolve in the same alembic.

That theory has by now been refuted at every point. But except where, as I have shown, it was of recent growth and as yet unchecked by field-work at the time of his death, the disproof of it rests on data provided by excavation and nothing else. Excavation, in Horsley's day undiscovered, has given the archæologist a weapon as powerful as the astronomer's telescope or the industrialist's steam-engine. The scientist's business is to theorize on a basis of fact, and when a revolution in technique reveals a new order of facts he has to reorganize his whole world. The old theories are superseded; the old controversies become meaningless; the old problems become unimportant. In such a landslide, it is difficult for any scientist to keep his head; impossible perhaps; were it not that revolutions of this kind (like all revolutions, when you understand their true history) happen very gradually. The temptation is to think that such a revolution changes everything; whereas in fact it changes only one thing—the materials which in his theorizing the scientist has to use. The ways in which he can use them remain unchanged. A revolution in scientific technique reveals a new order of facts; but, once revealed, they must be reflected on exactly like any others. The starting-point of the work of theorizing has changed, but the nature of that work itself is unaltered.

This tends to be overlooked by people who have recently

acquired a new scientific technique. They become engrossed in the use of their new method, and accumulate fact upon fact, forgetting that to the scientist facts are useless except so far as they become a basis for theories. Absorbed in the excitement of hunting new facts, they neglect the discipline of theoretical thinking, and in consequence an advance in scientific technique is often accompanied by a falling-off in the quality of scientific thought. Improvements in scientific technique are a mixed blessing, because they delude us into thinking that they make scientific work easier, whereas really they leave it just as hard as ever it was. Repeatedly, in the history of science, you will find that the best theoretical thinking is done just before a revolution. It is, in fact, the high quality of this thinking, the finality of its conclusions on the basis of existing evidence, that necessitates the revolution, by convincing men that nothing more can be done until we have learnt to explore a new region of facts.

Of this generalization Horsley is an instance. In the history of archæology there has never, so far as I know, been a better field-worker, and never a thinker more highly gifted or more successful in distilling the entire bulk of the data into a coherent and acceptable theory. On the subject which I am here discussing he said, so far as the evidence available in his day was concerned, the last word. It was not until the archæological revolution of the nineteenth century, which gave us the technique of excavation, that anyone could find a flaw in his theory.

And mark the consequence. When the revolution came about, when excavation brought to light the milecastle-slabs and so inaugurated the Hadrianic theory of the Wall, the logical work by which that theory was elaborated was infinitely poorer in quality than Horsley's own. The conception of the Vallum as a defence against the south, which was an integral part of the new theory, not only implied a much lower degree of reflection on its visible features than Horsley's theory of it, but had actually been considered



and refuted in advance by Horsley himself. "It must be owned," he writes (p. 125), "that the southern prospect of Hadrian's work, and the defence on that side, is generally better than on the north . . . such considerations as these have induced some to believe, that what now goes by the name of Hadrian's work, was originally designed for a fence against any sudden insurrection of the provincial Britons, and particularly of the *Brigantes*." His answer is still valid, though the nineteenth-century Hadrianists ignored it. "Sometimes the advantageous ground is left on the south, where it might easily have been otherwise ordered, if their design had been only to secure themselves against assailants from the south. . . . In one place the *vallum* runs between higher grounds on each side. Besides, if it was designed against an enemy from the south, the ditch is on the wrong side, being to the north of the ramparts" [or, on a unitary theory of the Vallum, we might add, in the middle of them] ". . . I don't see that there was any occasion to draw such a line of defence in order to prevent their making an attack upon the stations, for they are stronger on all sides than this *vallum*" (p. 126). After reading so masterly a discussion, it is painful to turn to Bruce: "A careful examination of the country over which the Wall runs, almost necessarily leads to the conclusion that . . . the Vallum was intended as a protection against sudden surprise from the south."²⁸

Of all the archæologists who have worked at Hadrian's Wall in the last two hundred years, none have surpassed Horsley in the sheer scientific quality of his thought; almost all have fallen far short of him, and I should hesitate to say that one has equalled him. Let the problem of the Vallum be our test case. Horsley is the only man who has ever propounded a solution of that problem entirely adequate to the available evidence, and forming a logical and intelli-

²⁸ *Wall*³, 1867, p. 59. In edd. 1-2 the corresponding passage is less dogmatic: the southern defence theory is ascribed to Hodgson and said to have "much probability."

gible whole; the subsequent theories of it have either dogmatized in defiance of known facts, or confined themselves to vague generalities, or presented themselves frankly as working hypotheses awaiting confirmation. Thus, if Horsley failed to solve the problem, we with our improved archæological technique have failed much more completely. Why is this? Does the solution await the discovery of still further technical methods, as yet undreamed of? I suspect that it does not; I suspect that what is at fault is not our technique but our logic; and that if our methods were used by a man equal to Horsley in the faculty of clear and coherent thinking, they would yield a solution which, even if not final, would satisfy the twentieth century as his satisfied the eighteenth.²⁹

²⁹ A friendly critic has suggested that some readers might be deceived as to my view of the posthumous history of Horsley's text, and think I ascribe all or some of the features I have analysed to the work of an editor preparing it for publication. I have seen nothing to suggest that there was such an editor. I think his text was printed exactly as he left it, and that the afterthoughts I have detected were in every case inserted by himself in a manuscript that he kept by him for many years and altered as his observations accumulated and as his thought developed. There is nothing in the typography to suggest that he inserted any of them in proof after printing had begun, as he inserted milecastle 7/8 in the plate of his map.