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M. PHIL.

~~PH. D.~~ THESIS BY N.J. REED

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On the recommendation of the Examining Committee and of the Council of the Faculty of Arts, the Senatus Academicus agreed at its meeting on 25th June, 1980, to award the degree of M.Phil. to Mr. N.J. Reed for a thesis submitted for the degree of Ph.D. entitled "The Sources for Roman Britain".

A RECONSIDERATION OF THE LITERARY SOURCES
FOR THE HISTORY OF ROMAN BRITAIN

Thesis by N.J. Reed

ABSTRACT

The Greek historian Polyaeus, and several other historians, may preserve truer versions of aspects of Caesar's Second Invasion of Britain, than the Commentaries of Caesar himself. A hitherto unexplained coin of Caesar can be explained if Polyaeus' principal story is accepted.

The evidence for the Boudiccan Revolt - in particular, the indications given by Tacitus - suggests that the traditional date of AD 61 should be accepted, and not 60, as presumed for the last twenty years. As to the sources, a common one, Suetonius Paulinus, can be detected behind the accounts, as well as the sources preceding Tacitus and Dio.

The scanty evidence for the period between 196 and 207 AD in Britain indicates there was not just one invasion from Scotland between those years, but at least three separate invasions, each prompting a reconsideration of the country's defence system.

The province itself was divided administratively not under Caracalla but under Severus, to lessen the risk of revolt from a governor of the province.

The Severan campaigns in Scotland can be clearly divided into the four years AD 207-210. In 207 Caracalla led an expedition up to the Forth. In 208 Severus and Caracalla occupied and fortified Fife, constructing bridges across Forth and Tay. In 209 the army was divided in two, and established 63-acre camps as far as Montrose. In 210 the entire army advanced further north, and returned claiming a great victory. But late in the year the two principal tribes of Scotland rebelled again, which led Caracalla to retreat to the line of Hadrian's Wall.

In an Appendix, it is suggested that the frequent circum-navigations of Britain by the navy were an attempt to demonstrate

(REED THESIS ABSTRACT: concluded)

that even Scotland had been 'conquered'. Our sources were consistently misled by their rhetorical training into assuming the sole purpose was to demonstrate that Britain was an island.

The Antonine Itinerary was an official Imperial Document drawn up from a road map of the Empire, in order to indicate routes to be taken by the imperial Annona. It was put together, incorporating earlier material, under the Emperor Severus Alexander, who himself instituted the annona as a regular tax.

Although the province of Britain as a whole never made an overall profit for its masters in Rome, its income was quite respectable, and the budget might have balanced if either Agricola or Severus had succeeded in their ultimate objectives.

A RECONSIDERATION OF THE LITERARY SOURCES
FOR THE HISTORY OF ROMAN BRITAIN

by N.J. REED

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NOTE

With this thesis are also presented for consideration five articles published in classical journals. They complement the material of the thesis at various points, and are as follows:

- I "The Sources of Tacitus and Dio for the Boudiccan Revolt": *Latomus* 33 (1974), 926-33

This complements Section C of the thesis.

- II "The Fifth Year of Agricola's Campaigns": *Britannia* II (1971), 143-8

Relevant to Section F of the thesis, and its Appendix on the circumnavigations.

- III "Agricola 24,2": *Classical Quarterly* 26 (1976), 115. Relevant in the same way as II above.

- IV "Three Fragments of Livy concerning Britain": *Latomus* 32 (1973), 766-786.

Relevant to the Conclusion.

- V "Drusus and the Classis Britannica": *Historia* 24 (1975), 315-323.

Relevant to the Conclusion.

JULIUS CAESAR'S CROSSING OF THE THAMES

The reliability of Caesar's Commentaries is a matter for dispute. Normally, attempts have been made to impugn them by examining his account very closely, and seeing if one can detect inconsistencies or general improbabilities.¹ Where other sources are available, they stand as a useful check. But in the case of his British invasions, it is assumed that we have no such independent check because all our sources are potted versions of his own account. This is undoubtedly true for most of the surviving accounts, which add little apart from the remark that the invasions were not successful - a conclusion which few have disputed.

One person who has disputed this, however, is C.E. Stevens. In his article on Britain Between the Invasions,² he pointed out that in Caesar's account of his final departure from Britain, he uses legal language which leaves the strong impression that he had settled on an annual tribute from Britain to Rome. Such a conclusion would indeed be warranted, if our source were an ordinary historian. But we are dealing with a very partial one. Though Caesar would no doubt like to give us the impression that tribute was laid down and he was waiting for the first instalment, there is no suggestive evidence that the tribute was ever paid. This is

far more likely to be a feeble attempt at a 'cover-up', to conceal the unsuccessful conclusion to the British campaigns.

But while most of the ancient historians take their account straight from Caesar, is this necessarily true of all? Is it not likely that one or two of the more intelligent decided to delve rather deeper, and consult independent sources? At any rate, it would not seem very surprising if this were to emerge from our analysis.

Among the fragments which survive from the extensive historical works of Appian, several come from his book of 'Celtic History'. One, Fragment 19, reads as follows: εὐθὺς ἠρέθισον τοὺς Βρεττανοὺς παρορκῆσαι, ἔγκλημα ἔχοντας, ὅτι σπονδῶν σφίσι γενομένων ἔτι παρῆν τὸ στρατόπεδον.

We cannot tell from this passage whether the British did 'break their oath', but the most likely context for this action - and only Caesar can provide us with that context - is in 55 BC.

On the day they first landed in Britain, Caesar's forces fought a battle with the natives, and were victorious. (B.G. IV, 26). A treaty was at once concluded, with the natives giving Caesar some hostages, and saying they would summon others and hand them over in a few days. This is presumably Caesar's reason for not having departed soon after the treaty was agreed. But no less than four days afterwards, eighteen ships appeared

offshore. Caesar explains (IV,28) that these were the ships he had mentioned before. He is referring in fact to ships which he did not take with him when he set out. They were to carry the cavalry: 'equitesque in ulteriorem portum progredi et naves conscendere et se sequi iussit.' When Caesar himself reached Britain, 'dum reliquae naves eo convenerint, ad horam nonam in ancoris expectavit.' After giving a brief speech to his commanders, Caesar set off again, and they finally beached on the shore where the first battle is fought. And the next we hear of the cavalry is their arrival four days later, with no explanation of their four day delay. Can we really believe they took four days to embark and set out? It would be more plausible to think that once Caesar landed and fought his battle on the first day, he would have sent word that the cavalry were unnecessary. Unless he was planning a further advance, even though the truce and withdrawal had been agreed. From Caesar's own words, then, we are entitled to be suspicious.

However, according to Appian the natives found cause for complaint, in that the army was still present although the treaty was in force. This implies that under the treaty the army was supposed to have gone. And why, indeed, was Caesar still hanging around, when the natives could perfectly easily have sent their hostages

to Caesar on the Continent?

It would be much more plausible to assume that the Britons did indeed have a grievance. They waited for Caesar's troops to depart, as had been agreed in the treaty. But when, four days after, they were still there, and when, moreover, all the Roman cavalry suddenly turned up, they realised Caesar had no intention of keeping his side of the bargain. So the British leaders "started to rouse the Britons to break the oath". But the reason for this we learn not from Caesar, but from Appian. It seems that Appian had acquaintance with a source independent from Caesar, which gave a bit more of the other side's point of view. And this is important, because it shows that at least one other source was available, which the enquiring historian could use.

Who then could this source be? One possibility is Caesar again - but this time his despatches. We know that he published some of them, after placing them in book form.³ Because they could appear in book form, they must have been considerably more lengthy than the sketchy account surviving in the Commentaries. Indeed, Cicero regarded the latter merely as material for history, rather than as what the history itself should be.⁴ But the source could have been some other writer, to whose identity we may have a greater clue

after examining other instances of information surviving independently of the Commentaries.

One obvious instance represents part of the immediate aftermath to the Gallic campaigns. Frontinus, the writer of a book of Military Stratagems, recounts (II,13,11) that Commius, leader of the Atrebates, was fleeing from Gaul to Britain when his ships were stranded on an ebb tide. He nonetheless ordered all sails to be spread, so that Caesar, watching from the shore, assumed he had got away safely. No-one has disputed the veracity of this anecdote, and in itself it indicates a source independent of Caesar.

Another chance piece of information comes from a writer who names his source, and it prompts speculation. Athenaeus is our direct source: 'Julius Caesar, who was the first of all men to cross over to the British Isles, along with a thousand ships brought with him three servants in all. Cotta, his second-in-command at that time, tells us this in his work on the Roman Constitution, which he wrote in Latin.'⁵ Seeing that Cotta was Caesar's subordinate, one wonders if he could have refrained from mentioning much other interesting matter on the Invasions, if he regarded it as relevant to his theme. But the likelihood is that he was discussing the power and influence of provincial governors from a

historical viewpoint, and noting with surprise the small number of retainers which Caesar took on his expedition.

Another incident supposedly recorded from this invasion, but not recorded by Caesar, is retailed by no less than three authorities, though with significant discrepancies. Our earliest source, Valerius Maximus, would on general grounds be thought the least reliable, since he is more of an anecdotist than a historian. The gist of his account is as follows (III,2,23):

While Caesar was in Britain, one of his centurions, M. Caesius Scaeva, crossed to a hostile island with four other men. After fighting valiantly, only Scaeva managed to escape - the others being killed - and he lost his shield in doing so. On finally returning to Caesar, he fell on his knees and begged his pardon for losing his shield.

Plutarch,⁶ writing nearly a century later, in about AD 100, is clearly referring to the same incident when he names the centurion as Cassius Skeuas. But Cassius is described as merely stuck in a marsh with some companions. So when he asks Caesar's pardon for losing his shield in the course of wading out, this seems only natural - and the whole point of the

anecdote is lost. Maximus' account at least has the merit of plausibility.

When we turn to Cassius Dio,⁷ writing in about AD 230, the incident is transferred both in time and place, and accompanied by much circumstantial detail. It is ascribed to the year 60 BC, when Caesar was governor in Lusitania. Dio says he had pursued the inhabitants of the Herminian Mountains to the ocean. After they had crossed over to an island, Caesar sent in pursuit some of his men in frafts. Only one of these survived in the resultant fight - Publius Scaevius - but he lost his shield in escaping. Later Caesar summoned boats from Gades, took his whole army across to the island, and conquered it. He then sailed to Brigantium, a city of Gallaecia, and subdued it also. Such is Dio's narrative.

The variety of circumstantial detail in this account can cut two ways: rather than adding to its verisimilitude, it might indicate picturesque details added by the author to fill out the narrative. However, the details here are not particularly striking. Instead, they give a detailed context to the narrative. And more strikingly, they do not recount the ^{'pardon' for} loss of the shield. So this is not an anecdote shorn of its context and with details later added. Rather, it is part of a continuous narrative in

a wider context. Taking these factors into account, we might expect it a priori to be the most reliable version, presumably deriving from an early and reliable source.

But if we are going to claim that Dio's version is closest to the truth, how did the other versions arise? Luckily, Dio's account does provide a possible answer. His narrative includes Caesar's crossing with his whole army to an island and conquering it, and immediately afterwards subduing Brigantium. In the following century it would be only too easy to place such an action in the context of his best-known invasion of an island, Britain, and its best-known tribe, the Brigantes. So this does provide an explanation for the origin of the other versions. On the other hand, if we assumed that the chronologically earlier versions of Plutarch and Valerius were closer to the truth, there would be no easy way to explain the origin of Dio's version.

So it does look very much as though Dio's version is the most reliable. And if that is so, this anecdote can be removed from those incidental pieces of information sometimes used to supplement Caesar's narrative of his British invasions.

Much the most curious of these anecdotes comes from another writer of stratagems: not Frontinus, but the Greek writer Polyænus, who wrote in the second century AD. I give a literal translation.⁸

"Caesar in Britain was trying to cross a large river. Cassivellaunus, a King of the Britons, was hindering him with a large force of cavalry and charioteers. Following in Caesar's army was an enormous elephant, an animal which had not been seen by inhabitants of Britain. Caesar armoured it with steel plates, placed a big tower on top of it, and stationed slingers and archers on top. He then gave orders for it to walk into the river. The Britons were astounded seeing such an enormous and unfamiliar beast. One need hardly mention the horses, since, even among the Greeks, horses flee if they see an elephant even without any armour. But when confronted with one carrying a tower and armour, and showering arrows and slingstones, they could not even endure the sight of it. So the British fled, along with their horses and chariots. So the Romans crossed the river without danger, having frightened away their enemy by the use of one animal."

The picture of an elephant covered with iron plates and supporting a tower in which were placed slingers and archers, is a surprising one. Yet none of these practices were unknown to the Romans.⁹ Towers on elephants were used by Hellenistic rulers such as Antiochus III as early as the third century BC. While less than ten years after this supposed incident of 54 BC, Q. Metellus Scipio was using elephants with both towers and armour when fighting on behalf of his father-in-law Pompey.¹⁰

However, seeing that Caesar makes no mention of the elephant in the Bellum Gallicum, the first and most obvious assumption would be that Polyaeus has confused this episode with some other general and/or country. There is, incidentally, no chance that he simply made it up: his work is generally scholarly and reliable, and the story, whatever its plausibility, must derive from an earlier source. Yet it is very hard to see what 'confusion' was made by Polyaeus or his source. We know of no comparable example where a Roman or even a Carthaginian general used an elephant to help in crossing a river.

We do know - or at any rate feel fairly certain - that Claudius brought elephants to Britain. The evidence is not strong, but appears in Dio LX,21. Here we are told that Aulus Plautius had been ordered to send for Claudius if he met with particularly strong opposition, καὶ παρασκευῆ γε ἐπὶ τῇ στρατείᾳ

πολλή τῶν τε ἄλλων καὶ ἐλεφάντων προσυνείλετο.
 When the message came from Plautius, Claudius entrusted home affairs to Lucius Vitellius "while he himself set out." The elephants might thus have been a useful part of the reinforcements, and we should apparently assume he took them with him. But there is no sign he used them to cross a river, nor should we expect a reduction from two (or more) elephants in the narrative, to one. In any case, it is generally agreed that Dio has exaggerated Claudius' role in the action, and that Plautius only called for him once the main fighting was over, so he could take part in the spectacles in celebration of the Conquest. If this is correct, Claudius would hardly have been summoned while there was still a strongly contested river-crossing to be made, and thus the elephants for any such crossing would not yet have arrived. We may conclude that the obvious hypotheses to explain away Polyaeus' story, carry little conviction.

Let us turn for a moment to the practical feasibility of bringing an elephant. Otto Seel¹¹ tries to laugh off the whole of Polyaeus' account on the grounds of physical impossibility: explaining at great length how the elephant would have to be embarked, then landed, then taken across southern England and the Downs to Brentford. He seriously underestimates the capabilities of the Roman army if he thinks any of this is impossible. It would

certainly have been quite possible to take it across the Channel, given that Claudius transported at least two, a century later. Presumably tying it down on a huge raft would have been sufficient. Nor would there have been any difficulty over taking it the eighty miles to Brentford. After all, Hannibal had taken thirty-seven elephants across the river Rhone and mountain ranges. One suspects Seel's main reason for scorn is the 'idiocy' of the notion that Caesar would ever have taken an elephant in the first place.

Almost the only scholar willing to believe Polyaeus was C.E. Stevens, in an article published in 'History Today'.¹² And he did in fact produce a plausible reason why Caesar might have wanted to take an elephant to Britain. In 54 BC Gnaeus Domitius Ahenobarbus (a distant ancestor of the Emperor Nero) was standing against Caesar for the consulship, which he had failed to win the previous year. But in 54 Domitius again found himself excluded, this time by Caesar's command being extended for five years. Now, it so happened that Domitius' grandfather conquered Provence in 121 BC, and was so proud of this conquest that he afterwards rode round the province on the back of an elephant (Suet. Nero 2). What better way, then, for Caesar to be one up on his defeated rival, than for him to ride round the fabulous province of Britain on an elephant, once the Britons were subdued?

Yet, supposing Caesar had done this, why does he not mention the fact? Stevens thought it was precisely because he did not succeed in conquering Britain, and so was unable to ride the elephant through the conquered province. "If this elephant story is true, Caesar had made rather a fool of himself over it, and the last place where he would call attention to it would be the Commentaries." But when he was in Britain he did not know the final outcome, and so could feel confident about mentioning the elephant in his despatches. These, then, are reasons why Caesar might conceivably have brought an elephant to Britain, and why he might have been reluctant to mention it later. But we do need confirmatory evidence before we could think of believing Polyaeus' strange story.

There is one possible way of checking it, which did not occur to Stevens. What does Caesar himself say about his crossing of the Thames? It so happens that Caesar's own account of the crossing has baffled commentators ever since they started discussing it. Here is Caesar's account (B.G. V,18):

"Caesar ... ad flumen Tamesim in fines Cassivellauni exercitum duxit, quod flumen uno omnino loco pedibus, atque hoc aegre, transiri potest. eo cum venisset, animadvertit ad alteram fluminis ripam magnas esse copias hostium instructas. ripa autem erat acutis sudibus praefixis munita, eiusdemque generis sub aqua defixae sudes flumine tegebantur. his rebus cognitiss a captivis perfugisque Caesar praemisso equitatu confestim

subsequi iussit. Sed ea celeritate atque eo impetu milites ierunt, cum capite solo ex aqua exstarent, ut hostes impetum legionum atque equitum sustinere non possent ripasque dimitterent ac se fugae mandarent."

According to this narrative, we are supposed to imagine the cavalry charging straight into the river, blithely ignoring the stakes underwater and in the bank, being followed by the infantry, and all of them moving 'with such speed and attack' that the enemy on the other bank simply turned and fled.

This is flatly impossible, and commentators have resorted to a variety of ingenious explanations. Back in the last century, the normal view was that Caesar ordered the cavalry to cross at some other unspecified point, where there were no stakes in the river. Kraner and Dittenberger¹³ tried to maintain that the cavalry must have crossed at the same place as the infantry, because Caesar expressly states there was only one crossing-point on the river. However, Caesar actually says there was one crossing-point pedibus, and of course there is no reason why the cavalry should not have been able to swim across elsewhere.

A.G. Peskett¹⁴ maintained that praemisso, correctly translated, should mean that the cavalry went in front of the infantry in place, not simply in time. T. Rice Holmes¹⁵ repeated the obvious objection that this would be physically impossible with the stakes in the way -

and so followed the usual view of praemittere as 'to send earlier in time'. But he found greater difficulty in subsequi, which ought to mean 'to follow closely behind' and thus should imply that the infantry walked in the hoof-prints of the cavalry. He tried to justify his unusual translation of subsequi as 'to follow afterwards in time' by reference to another passage in Caesar: B.G. IV,25.

This is where Caesar describes the splendid example of the eagle-bearer of Legio X, who leapt down from his ship onto the British shore, while the rest were hesitating. 'Tum nostri cohortati inter se, ne tantum dedecus admitteretur, universi ex navi desiluerunt, hos item ex proximis primis navibus cum conspexissent, subsequuti hostibus adpropinquaverunt.' Holmes imagined that the ships were drawn up in lines parallel to the shore, and that in consequence the soldiers in any second or subsequent line could not have 'followed directly in the rear' of their comrades, or they would have been drowned. This view was accompanied by the deletion of either proximis or primis in the text, following earlier editors. In 1957 W.K. Lacey demolished the scenario envisaged by Holmes.¹⁶ In his opinion, the manuscript proximis primis makes perfect sense: the ships would actually have been drawn up in a number of columns, each of them several ships deep, and at right angles to the shoreline. Thus, the soldiers in the ships nearest the shore (primae naves) and nearest the standard-bearer (proximae naves) followed his example. However, though Holmes envisaged the mili-

tary situation wrongly, this passage does appear to provide the parallel he was seeking since, even with Lacey's interpretation, the soldiers on the nearby ships would not have 'followed directly in the rear' of the standard-bearer.

But how does this help in understanding the crossing of the Thames? Suppose the cavalry did cross at some other point. We are then obliged to believe that the infantry alone charged through the stakes - ripping them up or sawing them off while only their heads were above water - and doing all this with such speed and attack that the opposing forces on the riverbank could not withstand them. Even if the cavalry were attacking the enemy flanks at the same time, the scene is still unbelievable.

Caesar does use the verb subsequi in two other places in the BG: II,11 and II,19; on both occasions it has its normal meaning of 'following directly in the rear'. Indeed, II,19 describes a very similar river-crossing, again with praemisso equitatu, but in this case there is no difficulty over interpretation, since there were no stakes in the way.

It is interesting to note that in the later edition of the Commentary by Kraner and Dittenberger,¹⁷ the new editor Meusel was far more cautious than any of his predecessors: 'Wie man sich den Vorgang zu denken hat, ist bei der Kürze von Caesars Darstellung zweifelhaft. Am wahrscheinlichsten dürfte noch die Annahme sein,

dass der grösste Teil der Reiterei an einer etwas tieferen, aber nicht mehr durch Pfähle verbarricadierten Stelle ... über den Fluss gesetzt ist... Jedenfalls ist es nach Caesars Darstellung wenig wahrscheinlich, dass die Legionare, während die Reiterei den Kampf begann, die eingetriebenen Pfähle zu entfernen gesucht haben.' Very true: so now one has to imagine the legionaries charging through two rows of stakes without even trying to remove them!

But having made this point, Meusel continues, 'Geringe Wahrscheinlichkeit hat auch der Bericht Polyäens, Caesar habe durch einen Elefanten, auf dessen Rücken sich Schleuderer und Bogenschützen befunden hätten, die Britannier zur Flucht veranlasst. Schleuderer und Bogenschützen hat allem Anschein nach Caesar bei diesem Feldzug überhaupt nicht gehabt (c. 16,1).' By contrast, Rice-Holmes, who does not even consider the elephant, specifically postulates such slingers and archers as the only protection the legionaries had while negotiating the crossing. At 16,1, the words which must seem crucial to Meusel are 'intellectum est nostris, propter gravitatem armorum, quod neque insequi cedentes possent neque ab signis discedere audent ...' But why need we assume that nostris here means 'every single one of our men'? In any case, the whole sentence is qualified by 'toto hoc in genere pugnae'. And lastly, we should expect the slingers and archers to have been kept in reserve anyway. Whatever reason we might have for disbelieving

Polyaenus, this one will not do.

It would certainly not have been difficult for Caesar to obtain an elephant in 54 BC. Towards the end of 55, Pompey gave Rome its first stone theatre, and commemorated its opening by games in which twenty African elephants fought against African hunters.¹⁸ He could easily have supplied Caesar with an elephant for the start of his next year's campaign.

Indeed, just nine years later (46 BC) we come across an incident which may not be wholly coincidental: 'Pugnare et Caesari dictatori, tertio consulatu eius, viginti elephantum contra pedes D, iterumque totidem turriti cum sexagenis propugnatoribus eodem quo priore numero peditum et pari equitum ex adverso dimicante.'¹⁹ This much from Pliny: other sources, Dio and Appian, disagree over the numbers involved, but in view of Pliny's greater detail, his turriti elephantum cannot be doubted.²⁰ So this incident shows that Caesar was using elephants with towers only eight years after his British campaigns.

To conclude. The testimony of Polyaenus would not convince by itself. The clinching evidence is the impossibility of Caesar's own account. Given that, I shall assume henceforth that Polyaenus' account of the Thames crossing is true in all essentials.

What then can we deduce about Polyaenus' source? We might expect the anecdote to derive from Caesar's despatches and reach Polyaenus through an intermediate source. Alternatively, it could come from an independent

witness to Caesar's Second Invasion. For an intermediate source, Stevens suggested Livy, but in that case the anecdote would surely have been repeated by other writers and become well-known. While if it was well-known, it is just the sort of anecdote Suetonius would have included - but he does not.

The most exhaustive survey of Polyaeus' sources was written by J. Melber.²¹ He reaches no firm conclusions on Polyaeus' authorities for Book VIII, but does suggest that the 'elephant' incident is taken from a historian of Greek nationality. This is based on the words "even among the Greeks" in the passage: why Greeks rather than Romans? But the answer to this need have nothing to do with the source: it was the Macedonian army which often used elephants, while the Romans had not. The writer may well have known that when elephants were used by the Macedonian army, the opposing Greek horses were terrified. Seel in fact suggests that the 'Greek source' is Polyaeus himself who, he thinks, 'brazenly invented' the stratagem. Seel certainly makes a valid point in noting that earlier in his work (IV,21) Polyaeus refers to a Greek ruse to stop their horses being frightened of elephants (by using wooden models of them, and artificial trumpeting!); this could explain our author's remark about Greek horses in the later stratagem - if the remark is not repeated from his source.

Melber's more general conclusion, drawn from the work as a whole, is that Polyaeus may only have used Greek sources, rather than Latin ones. If so, that

should lower the field by half. But beyond that we shall probably not be able to go without further evidence.

We concluded above that Caesar brought an elephant to Britain, and was afterwards embarrassed about it. That could be a matter of passing and perhaps whimsical interest. But two more important conclusions follow from our analysis above.

First, Caesar wrote about the passage of the Thames in a grossly misleading manner, in order to cover up a political embarrassment. This can be added to other examples of his half-truths which all form part of 'l'art de la déformation dans les Commentaires'.

Of greater moment is the conclusion that this incident proves that Caesar meant to conquer Britain, and privately knew that he had failed in his task. There would have been no point in his bringing the elephant, except for the political oneupmanship envisaged by Stevens - which involved a plan of conquest. Of course, by Caesar's skilful writing the purpose of the campaigns is obscured, and some writers have concluded that his expeditions were a success.²² It may well be that his invasions taught him much, and he could have built on his previous work if ever he came back to finish the job. But he did not. The job remained unfinished, and Britain was left to be conquered by others.

FOOTNOTES

1. See M. Rambaud, L'Art de la Déformation dans les Commentaires de César (Paris 1953), and cf. H. Gesche, Caesar (Darmstadt 1976), 71-8 for bibliography.
2. In Aspects of Archaeology: Essays presented to O.G.S. Crawford, ed. W.F. Grimes (London 1951), and cf. Antiquity 21 (1947), 3-9.
3. Suet. Vita Caesaris 56.
4. Cicero Brutus 75,262. Each of Caesar's despatches must have been several pages in length (or the equivalent in scrolls), and he should have written every few days. Indeed, Cicero mentions uber^rim^ae litterae sent to himself from Caesar in Britain; the commander would presumably keep the Senate as fully informed..
5. Athenaeus, Deipnosophistae VI,273b.
6. Vita Caesaris 16,5. For the date, cf. C.P. Jones, JRS 56 (1966), 66-9.
7. Dio 37,53. Approximate date: F. Millar, A Study of Cassius Dio (Oxford 1964), 38-9.
8. Polyaeus, Stratagemis VIII,23,5.
9. See H.H. Scullard, The Elephant in the Greek and Roman World (Cambridge 1974) on towers: 242-5; on defensive armour: 196 and 239.
10. 'Elephantis turritis armatisque': [Caesar], Bell. Afr. 41.
11. Rhein. Mus. 103 (1960), 253-6.

12. History Today, September 1959, 626-7.
13. Caesaris Commentarii, ed. F. Kraner and W. Dittenberger (Berlin 1879), n. on V,19. 8/
14. A.G. Peskett in his Commentary (1887), n. on V,19.
15. Ancient Britain and the Invasions of Julius Caesar (London 1907), 698-9.
16. CQ 7 (1957), 118 f.
17. Berlin 1960, n. on V,19
18. Refs in Scullard o.c. (n.9), 283 n. 164.
19. Pliny, NH VIII,7,22.
20. Cf. Scullard ibid. n. 165.
21. Jahrb. für Kl. Phil., Supp. Bd. XIV, 1885, 417-683. O. Seel, Rhein. Mus., 103 (1960), 230-71 re-examines aspects of Melber's work on Polyaeus, in considering P.'s use of Trogus, Caesar and Livy.
22. Cf. e.g. S.G. Brady, CJ 47 (1952), 305-16.
23. This examination of the 'elephant' theory is a revision of part of my Presidential Lecture to the Oxford University Archaeological Society given in 1968. The validity of Polyaeus' version has been accepted since by one authority, Prof. C.F.C. Hawkes, who I am happy to say was present at my lecture. (See Hawkes, 'Britain and Julius Caesar', P.B.A. 63 (1977), 125-192.) He, however, wishes to place the elephant incident at the Medway, curiously ignoring the point I made about the impossibility of Caesar's account of his Thames crossing.

CAESAR'S ELEPHANT COIN

We have tried to show elsewhere that C.E. Stevens was correct in his theory that Julius Caesar brought an elephant to Britain. As additional evidence for his theory, Stevens adduced a very common coin of Caesar, which depicts an elephant trampling on a serpent, with the legend CAESAR underneath.¹ He points out that this is the only time Caesar uses an elephant to symbolise himself: in fact, the elephant was the well-known symbol of another distinguished Roman family, the Metelli.² Stevens suggested, then, that this is the elephant which Caesar took to Britain, trampling on a dragon which, he said, was the well-known symbol of Oceanus, i.e. the English Channel. And he points out that, according to Sydenham's dating, the coin could go back as far as 54 BC - which fits nicely with Caesar's second expedition.

It is unfortunate that of the five or six writers who have subsequently considered the coin, hardly any have been aware of all the evidence - or indeed, of the other writers considering the evidence since 1959.³ But this has one advantage: where two experts reach the same conclusion independently. Both Dayet in 1960, and Alföldi in 1968, examined the coin closely, and showed that the animal being trampled underfoot was not so much a serpent as a dragon (Latin draco), shown raising its head back to strike the elephant, and displaying scales on its neck. Unlike Alföldi,

Dayet had read Stevens' article, but he pointed out that Oceanus is nowhere represented as a dragon: when he is represented, it is as an old man with a long beard.⁴ But since Dayet was willing to accept that Caesar did bring an elephant to Britain, he suggested that the trampled animal is a sea-serpent, and thus by extension symbolises the Channel.⁵ This is surely an amendment to his theory that Stevens himself would have accepted.

However, if we were to accept this view of the coin, we should be left with a major problem. This coin of Caesar's is twice as common as any other coin issued by him, as is shown by the number of its obverse dies. Why, then, does Caesar widely advertise the elephant on the coin, if he hushes it up in his Commentary? Dayet seems to realise this difficulty, and suggests that it would have been difficult for Caesar to represent the river-crossing as a glorious success for his troops, if he had described the British army fleeing at the mere sight of an elephant. Yet this is surely not a strong argument: there are plenty of other occasions when Caesar is well able to demonstrate the courage of his men.⁶ Rather, the use of the elephant is something he could have been rather proud of: it is no coincidence that Polyaeus includes it in a book of Stratagems.

There is, however, another possible explanation for the issue of the coin, even if the transport of the elephant had proved embarrassing. These coins

were minted by Caesar to pay his troops in Gaul, and it is in Gaul that they are concentrated. They would ~~this~~ circulate primarily among his soldiers, and secondarily among the native Gauls. Neither of these groups could be expected to know why he had originally taken the elephant, and how his plans had gone awry. Rather, the natives would have been impressed, nay, overawed, by its appearance in their country, and the soldiers impressed with its transport across the Channel and use at the river crossing (though the latter would surely have been an inspired piece of impromptu planning by Caesar). Back in Rome, the literate public at whom the Commentaries were aimed would know of the reasons for its transport, and it would be desirable not to remind them of it in print. But they were hardly likely to see coins circulating in Gaul, and by the time they might, years later, the risks of embarrassment for Caesar would be small.⁷ So even if we accept Stevens' view of the coin's message, we can still find an explanation for the elephant's absence from the Commentaries.

What, then, is the traditional explanation of the coin, for those who wrote before the Stevens theory or did not accept it? It is that the word 'caesar' was the Punic word for elephant, so that the elephant, with CAESAR written underneath, represented Caesar trampling on his enemies. Grueber took the symbolism a stage further by suggesting that the serpent was in fact a carnyx or Gallic warhorn (symbolising the Celtic tribes

Caesar conquered). The representations of it in the coins he cites do indeed look similar to the later issues of the elephant coin. But the earliest and clearest representations on the coin show a serpent with a long curving neck, turning round to face the elephant, and this does not resemble any carnyx.⁸ What it does strongly remind one of is a passage of Pliny:⁹

'Elephantos fert Africa ... bellantesque cum his perpetua discordia dracones tantae multitudinis ... idem (dracones) obvii deprehensi in adversos (elephantos) erigunt se oculosque maxime petunt.'

It certainly seems likely that this motif of a perpetua discordia explains why the elephant is trampling a serpent - however we are to interpret its reference to Caesar.

What then of the Punic 'caesar' meaning elephant? This explanation for the legend on the coin only arose because this 'Punic pun' was one of the four explanations advanced by the ancients to explain the etymology of the name Caesar.¹⁰ This particular explanation is now generally thought erroneous: the name was probably derived from caedo. But in any case, it is inconceivable that Caesar should choose to circulate a coin based on a Punic pun in a country, Gaul, where Punic was totally unknown. Two further factors confirm us in dismissing the idea. First, we should have to suppose the name was given to an unknown ancestor of Caesar's for killing an elephant - though to name a man after an animal he has killed was foreign to Roman

custom. Secondly, it is doubtful whether there actually was a Punic word caesar meaning elephant: at least, no-one has produced evidence for one.¹¹

We must turn to the question of dating the coin. The most convincing terminus ante quem was provided by M. Crawford, in his book on Roman Republican coin hoards.¹² Here he decisively showed that the coin cannot have been issued any later than 49 BC. That left later interpreters in rather an impasse. Jocelyn Toynbee in 1973, and H. Scullard in 1974, presumably following her lead, put forward a new interpretation.¹³ At Thapsus in 46 BC, Caesar inflicted a decisive defeat on Pompey. Both sides used elephants, and Caesar captured sixty-four, armoured and turreted, which had belonged to the Pompeians.¹⁴ Toynbee said it was "surely in commemoration of the event" that Caesar issued his denarius. But both writers added that, if the hoards do show an earlier date for the coin, one would have to fall back on the 'Punic pun' idea.

In his later and more detailed discussion of the coin, Crawford rightly dismisses all such views, but admits that the true interpretation of the coin is obscure. He finally concludes, "I believe that it was intended to symbolise victory over evil." The numerous passages he cites do indeed show that elephants were associated with victory in iconographic terms. He also notes the issue of coins by a Greek confederacy in 395 BC, which showed Heracles strangling snakes -

following the myth of him in his cradle.¹⁵ We may take it that this is not just a case of all these cities adopting Heracles as their hero, the snakes simply being used to identify him. In Greek times the snake could sometimes symbolise the forces of evil,¹⁶ and this association would have been present in the Greek mind when they looked at coins of the confederacy.

In Roman times, on the other hand, the association of the snake with evil does not seem to have existed until the late Empire, under Christianity. According to Toynbee, 'Snakes, at any rate the non-poisonous varieties, were in the main of good repute in the pagan Graeco-Roman world, in contrast to the evil character ascribed to them in general in Jewish and Christian literature and thought.'¹⁷ The two examples she quotes of it as a force for evil are from sixth century and Byzantine times.¹⁸ The earliest clear example is equally clearly Christian: a coin of Constantius II, depicting him on his horse, trampling a snake, with the legend debellator hostium.¹⁹ So even though pictures of Heracles strangling the snakes were common in the early Roman Empire,²⁰ this does not imply that the Romans thought of these snakes as evil - except insofar as they might have bitten Heracles.

We noted earlier the passage of Pliny which describes snakes as the 'natural enemy' of the elephant - a theme which recurs in Aelian, Solinus and others.²¹ One might perhaps conclude that the coin was thought of as an elephant symbolising victory (and

with 'CAESAR' telling us whose was the victory) easily crushing its natural opponent, the snake. But in fact, the scaly beast shown is rather more than a snake. As Alföldi shows, it is a reptile with scales, horns and a beard. It thus depicts a monster - a fearsome draco - which would naturally be assumed to be evil. So if we accept Crawford's date of 49 as the earliest possible, any connection with Britain can be ruled out, and the coin satisfactorily explained as 'victory over evil'.

A more complicated idea would be to regard Caesar's elephant as symbolising light, crushing a serpent of darkness. But such symbolism concerning light and dark is more likely in the later Empire, when ideas derived from Mithraeism and the eastern mystery religions were more pervasive.²² The idea of victory over evil does then make sense, and Crawford is to be congratulated on this plausible idea. But does his dating rule out any other explanation?

These are his reasons for the date of 49 BC:

'The status of no. 443 as the first military issue of Caesar is established beyond all possible doubt by its occurrence as the only military issue of Caesar not only in the Cadriano and San Cesario hoards, but also in the Carbonara and San Giuliano hoards, and by the greater degree of wear which it displays in later hoards, compared with other military issues of Caesar. The absence of no. 443 from hoards of the 50s makes it clear that

the issue was only struck after Caesar moved into open rebellion.'

The point at issue is whether Crawford's evidence would allow us to date the coin any earlier than 49, or even as far back as 54, where Sydenham placed it. If it could be backdated to the 50s, then of course the connection with Britain once again becomes possible - with the draco being taken to be a monstrum marinum, symbolising the Channel.

We may accept Crawford's arguments for this being Caesar's first military issue. But this only shows its appearance at least by 49. It might be earlier. Everything then hinges on its 'absence from hoards of the 50s.' The point may be examined by reference to Tables XIII and XIV in Crawford, where all the relevant hoards and fixed dates are listed. The dating of all such hoards depends in the first instance on those coins within them, which can be assigned to a particular year: the latest such coins forming the earliest date of deposition for the hoard. In our case, it rapidly emerges that there is unfortunately no known chronological fixed point for dating hoards between 55 and 49 BC. So the only evidence available at the moment for dating our coin is hoards which come down as far as 49, but do not contain the elephant coin.

There is in fact only one hoard, from Brandosa, which comes as late as 49, yet does not contain the

coin. Of its total of 421 coins, only nine date to 49 BC. It would therefore not be surprising for it not to contain our coin, even if this had appeared before 49. And in addition, the findspot of the hoard was in Italy, whereas our coin we know was first issued in Gaul, and so would be unlikely to appear in Italy until some time later.

It is possible that Crawford had a subsidiary consideration in his dating, in the thought that Caesar would not have issued such a coin until he had indeed 'moved into open rebellion'. But there was an earlier Roman general who did so: Sulla.²³ He did not put his head on the coins; no more did Caesar on this occasion when, after all, the coins were being minted specifically for his soldiers in Gaul. Thus the earlier date for the coin remains possible, and so does the British connection - in that the coin may date from before 49, when Caesar rebelled.

Of one thing we may be reasonably sure. Even if the coin started off with its specific reference to Britain, its extreme commonness among Caesar's coinage indicates that it must have attained a much wider significance: that of victory crushing evil. This view receives slight support from the rapid degeneration of the draco motif into a simple snake, looking nothing like the original fearsome sea-monster.

Yet if Caesar originally meant the coin to have this general symbolism, one should ask whether he would have chosen an elephant to symbolise himself,

given that an elephant was, outstandingly, the coin type of the Metelli. Emperors or other commanders may have occasionally used elephants for shows commemorating victories, and sometimes had them depicted, usually in the plural, on coins.

But the Caesar^{coin} is rather different, and would only be satisfactorily explained if there were some individual incident from 49 BC or earlier which would associate Caesar with an elephant. That incident can only be its transport to Britain across the Channel in 54. That, then, was the original significance of the coin.

FOOTNOTES

1. H.A. Grueber, Coins of the Roman Republic in the British Museum, (London 1910), 390-1;
E.A. Sydenham, The Coinage of the Roman Republic (London 1952), M.H. Crawford, Roman Republican Coinage (Cambridge 1974), no. 443, pp. 461 and 735.
2. Crawford 287.
3. M. Dayet, Carnet de Numismatique Celtique VII: Rev. Arch. de l'Est 1960, 42-7; A. Alföldi, Bonner Historia-Augusta-Colloquium 1966/7 (Bonn 1968), 9-18, and Plates I-IV;
J.M.C. Toynbee, Animals in Roman Life and Art (1973), 38; H.H. Scullard, The Elephant in the Greek and Roman World (1974), 194.
4. Cf. Daremberg and Saglio, Dictionnaire des Antiquités, s.v. 'Oceanus'.
5. For such serpents, cf. id. s.v. draco.
6. Compare e.g. B.G. III,14,8; V,35,6; and the bravery of the standard-bearer of the tenth legion (IV,25), discussed ^{above} below.
7. Evidence on the speed of circulation of coins around the Empire is still scarce. But it is unlikely that any great number of this issue would reach Rome quickly.
8. See Plates I-II in Alföldi.
9. Pliny, NH VIII,11,32 - 12,33.
10. Alföldi, o.c.

11. Alföldi, o.c. 12-13. Despite these objections, Alföldi still argued a more complex version of this theory, involving a hypothetical relation of Caesar's having taken the nickname 'Caesar' after killing an elephant. Inspired by his visit to Africa in 46 BC, Caesar issued his coin to commemorate this earlier event. Apart from the intrinsic implausibility of this theory, the coin is definitely earlier than 46, as we shall see.
12. M. Crawford, Roman Republican Coin Hoards (London 1969)
13. See note 3 above.
14. Scullard o.c. 197 and n. 139.
15. Cf. B.V. Head, The Coins of Ephesus (London 1880) 25-6.
16. Cf. E. Mitropoulou, Deities and Heroes in the Form of Snakes (Athens 1977), 19-23.
17. Toynbee, o.c. 223.
18. Id. 145.
19. See note 5 above.
20. Cf. Pliny, NH xxxv, 63
21. Aelian I,38; II,21; VI,21; Solinus 25,14 and cf. Scullard 217.
22. In any case, this interpretation is ruled out by the fact that "in Mithraeism, the snake was held to be beneficent" (Toynbee o.c. 234).
23. Cf. Crawford, Rom. Rep. Coinage 730-1.

THE DATE OF THE BOUDICCAN REVOLT

Just a century ago J. Asbach tried to demonstrate that the Boudiccan Revolt took place not in AD 61, where Tacitus places it, but in 60.¹ Asbach's view found few adherents in the following eighty years,² but in 1958 Sir Ronald Syme gave his support to the view, since when it has been almost universally accepted.³ So much so that the death of Boudicca's husband Prasutagus has been put back to AD 59, though there is nothing to indicate that more than a month elapsed between his death and the revolt.

The issue is not a minor one. If Asbach was right, Tacitus was guilty of a major error. In addition, since the revolt is one of the few landmarks in British archaeology, the new dating has led to material evidence being marginally redated, and to some confusion over the identification of the consuls in 61. The question will therefore bear re-examination.

Asbach gave three main reasons for the date of 60. One of these was invalid and has never been revived. He said it was 'unlikely' the governor Petronius Turpilianus stayed for a single year.⁴ It is equally 'unlikely' he stayed for two, but that is the only alternative.

The second rested on two premises. 1) After the revolt Suetonius Paulinus handed over his command

'Petronio Turpiliano, qui iam consulatu abierat,':
 (Ann. XIV,39). This phrase (it was said) must imply that Turpilianus came to Britain immediately after vacating his consulship. 2) Tacitus tells us that the consules ordinarii of 61 were Caesennius Paetus and Petronius Turpilianus. An inscription (CIL VI,597) dated to 1st March of an unknown year, gives L. Caesennius Paetus and P. Calvisius Ruso as consuls. Asbach suggested assigning it to 61, and by coupling it with point 1 deduced that Turpilianus had already left for Britain by March of 61. Since he took over after the rebellion was crushed, it would have to have started in 60.

There are two objections to this. First, since Tacitus had mentioned Turpilianus as consul slightly earlier, it was natural for him to explain that 'he had now left his consulship.' It is quite gratuitous to deduce anything else from this phrase. Second, Asbach admitted that the inscription could refer to the ordinary consuls of AD 79. Recently P.A. Gallivan following M. Hammond, has restated the arguments for consulships normally lasting six months under Nero (cf. Suet. Nero 15,2), and for identifying the two ordinary and two suffect consuls of 61.⁵ It follows from his arguments that this inscription must be dated to 79 and is thus irrelevant.⁶ It also follows that the earliest we could expect Turpilianus in Britain would be July of 61, and then only if we gratuitously

assume he came immediately after his consulship.

We turn then to the remaining argument given for redating the revolt: that the events described in Tacitus must have taken more than a year to complete. For Asbach the reasoning was simple. He said it was certain ('certo certius constat') that the decisive battle against Boudicca took place at the end of summer or beginning of autumn. Listing the events which followed, he postulated that the journeys alone would have required at least six months. On this basis they should have continued into the following year. Unfortunately, no-one else thinks it so certain the battle was in late summer, and if it was no later than mid-June (as I shall try to show), six months from there does not take us into the following year.

Henderson and Syme, however, take the different and superficially more plausible view that the events as a whole take more than a year: in Syme's words, 'One must observe the total of events that ostensibly belong to the one year.' After listing these he concludes, 'It is clear that Tacitus has been guilty of an inadvertence in dating. The revolt must have begun in 60.' Thus, if we are to acquit Tacitus of this inadvertence in dating, it must be shown that the events described could all have been completed by the end of December 61. The following analysis does not claim to give a definitive date for the events concerned. For our purposes, all that is necessary

is a timetable which is internally plausible, and which agrees with such evidence as we have.

The first action ascribed to Suetonius Paulinus in his final year of campaigning is an attack on Anglesey. It does not seem to have been noticed that we are provided with a significant clue to the time of year. Some time after the decisive battle with Boudicca, the natives were suffering from famine, as they had been '*serendis frugibus incuriosi, et omni aetate ad bellum versa, dum nostros commeatus sibi destinant.*' Sowing of crops normally takes place in March or April in East Anglia - at any rate, no later - so we must deduce that the revolt was already under way by the end of April.

The only person to ~~actually~~ realise the chronological significance of serendis frugibus was A.R. Burn. But, strangely enough, he was so certain that the revolt broke out in the late summer (his reason being, "Suetonius had had time to reach and conquer Anglesey before the outbreak"), that he was driven to suggest that "Tacitus' serendis frugibus is merely careless. What the Iceni had really neglected was not the previous season's ploughing and sowing, but the harvest." But it is simply no good rewriting our authority to make him conform to our preconceived ideas.

In contrast to previous views, I would suggest that the invasion of Anglesey may have been the first action of the new campaigning season in March/April

of the year in question. Let us look first at the evidence of Tacitus.

Paulinus, we are told (Agr. 14,3) 'biennio prosperas res habuit, subactis nationibus firmatisque praesidiis.' 'Subactis nationibus' clearly implies that certain ^{tribes} which had not been subdued were subdued. But on 'firmatis praesidiis' Ogilvie says,⁷ "firmare praesidia is regularly used to mean 'to strengthen garrisons', and this must be the meaning here too. There is no parallel for translating firmatis praesidiis as 'establishing strong forts'." And on this basis, Ogilvie affirms that, "Didius had established some advanced forts; Paulinus consolidated them," - which seems to imply that Paulinus did not cover any new ground, but simply gained a tighter hold on territory Didius or his predecessors had overrun.

The use of firmare in its pregnant sense, as 'to make a firm x', is most common in the expression aciem firmare, which is found in at least three passages of Tacitus,⁸ and just as often in Livy.⁹ For example, in chapter 35,2 of the Agricola, we find the general stationing his troops before the battle of Mons Graupius: 'Ita disposuit, ut peditum auxilia, quae octo milium erant, mediam aciem firmarent, equitum tria milia cornibus adfunderentur.' Here, as Ogilvie rightly says, firmarent means 'should make a strong centre', and in this context there can be no question that this is its meaning. In such passages it is

clear there are no troops in the 'battleline' before the soldiers referred to are placed there. At the same time one could argue that the acies already exists as an abstract entity, and is then 'strengthened' with these troops. This must be how Ogilvie takes the expression.

One should note that similar to the usage in Agr. 14,3 is that in Ann. XIII,41: 'nec id nobis virium erat quod firmando praesidio et capessendo bello divideretur', where this makes better sense if it refers to 'planting firm garrisons'. More importantly, though there seem to be no provable pregnant uses of firmare in the literal sense in Tacitus, there are two quite clear metaphorical ones: Ann. VI,30 'firmarent velut foedus', and Ann. XII,47 'Dis testibus pax firmaretur'. There is an equally clear pregnant use in the literal sense in Livy VI,23,12 'subsidiis Camillus firmat': where subsidiis would be necessary, if the acies were already conceived as existing.

These parallels do seem sufficient to suggest that 'firmatis praesidiis' in the Agricola is much more likely to mean 'establishing firm garrisons' - and, by implication, new ones - which would be in the new territory overrun and subdued by Paulinus. Those tribes could then have included the Ordovices, so that Anglesey would have been left for the final year of campaigning, and the attack on it launched at the start of the season.

Is this compatible with the evidence from archaeology? Archaeological evidence for the early campaigns in Wales is slowly accumulating, though it is still very patchy. Professor St. Joseph, for example, notes the occurrence of an apparent sequence of camps of about 40 acres, at Wall, Burlington, Wroxeter, Whittington and Penrhos.¹⁰ The size could fit one legion with an almost equal number of auxiliaries - or even, conceivably, a two-legion force. While these camps point to an invasion of Wales via the Severn and Dee valleys, there is little evidence of their date. They could belong to forces led by Ostorius Scapula, or Suetonius Paulinus, or even Agricola.

In 1976 another such camp, of about 48 acres, was discovered by Professor G.D.B. Jones at Rhyn Park, half a mile south-east of Chirk.¹¹ Here the provisional dating from excavation suggests Ostorius Scapula - and it is interesting to note that the camp was turned into a fortress and maintained for a short time, since permanent buildings were installed. This is most suggestive of it being used as a campaign base for assaults on Wales, at a time when Chester was not yet being used.

The archaeological evidence shows that Chester, as a permanent foundation, was founded about AD 74-5.¹² So far, no underlying marching-camp has been identified, but that is hardly surprising, given the size of the fortress. There can be no doubt that a camp was estab-

lished on the site at some previous date. However, the permanence of the fortress at Rhyn Park suggests that any campaign on west Wales based on this fortress, should have been at a date when Chester was not yet occupied, even as a camp.

The discovery of a marching-camp at Prestatyn¹⁴ on the north Welsh coastline, indicates that, as one would expect, one campaign was conducted along this coast, and must have been based on Chester. This must surely have been the line taken for the attack on Anglesey, and in that case we may assume that Rhyn Park was not used as the base for the attack on Anglesey. This deduction fits with the provisional dating to Scapula, since he is the only one of the three possible generals, whom we do not know to have attacked Anglesey - and indeed it is unlikely that he did. Evidence for the preliminaries to the invasion of Anglesey is more likely to emerge with a date for the camp at Prestatyn, and the discovery of further camps along the route. The best evidence, of course, would be the praesidium established on Anglesey by Paulinus - but it is highly probable that Agricola also placed one there.

In the meantime, the archaeological evidence can be ignored, so far as our particular problem of chronology is concerned. It would only become relevant if we were able to show, for example, that in the year of the invasion of the island, Paulinus' forces were

based on Chester, and thus needed to be brought across north Wales before the assault could be launched.

But even if we adopt this more usual view, that Paulinus had some campaigning to do at the beginning of the year before invading Anglesey, we must still conclude, for reasons given earlier, that the revolt had started by the end of April.

Let us resume our hypothetical chronological narrative. News of the revolt at Colchester was sent to Paulinus as soon as it broke out. The distance from Colchester to Anglesey is well under 300 miles. Even via London the distance would only be 325 miles. The imperial post normally covered fifty miles per day, but for an urgent message like this would be twice as fast. It could only have taken four days at most to reach Paulinus, so he must have heard by the first week of May. What was he doing when the news reached him? After the Roman army gained control of Anglesey, *'praesidium posthac impositum victis excisi- que luci saevis superstitionibus sacri ... haec agenti Suetonio repentina defectio provinciae nuntiatur.'* In other words he had already conquered and installed a garrison on the island by the beginning of May. Nor is this implausible. After two years already in the province, Paulinus must have chosen Chester - or somewhere even nearer - as base for his operations against Anglesey. Suppose the campaigning season began in April. A few weeks would suffice for the advance and

consolidation up to the Menai Straits, some sixty miles away (if this had not occurred earlier), and conquest of the island need not take more than two days.

Paulinus moved to London as soon as he heard the news, and the cavalry group halled probably took about four days to arrive. Even if he delayed for the expected rendezvous with Legio II, a week is the maximum we can imagine. By the middle of May, then, Paulinus was in London reviewing the situation and collecting reinforcements.

Boudicca's forces took two days to subdue those soldiers who had taken refuge in a temple. They then went out to meet Cerialis, wiped out the legion with him, and some of them pursued him to his camp. All this is unlikely to have taken more than two weeks, and they then turned towards London, which is no more than five days march/undignified scramble from Colchester. They could have arrived in London soon after mid-May, and shortly after Paulinus left it. He left probably in order to gain more time for reinforcements: we are told that when he finally faced Boudicca he had decided to omittere cunctationem (Ann. XIV,34).¹⁵ But we do not know where the battle was, and he may have kept delaying, by moving away, for over a week. Similarly the enemy may have taken several days in sacking London, moving to St. Albans and sacking it too. However, neither from Paulinus' nor Boudicca's point of view is there any reason why the time between

the rebels' leaving London and their battle should have been more than two weeks. Thus it is quite possible that the battle took place at the end of May. (And it would be hard to place it later than mid-June.)

We now turn to the events which followed the battle. The first set of events is described thus by Syme. 'Paulinus, after defeating Boudicca, proceeds to stamp out the insurrection, keeping the army in the field; reinforcements come from the Rhine; and the auxiliary troops are disposed in new camps for the winter.' Within days of the battle Paulinus would have requested reinforcements. Messages from Britain sent by the governor should not have taken more than two weeks to reach Rome; this request would be no slower. The Emperor could have received it by the middle of June. After a decision on which troops to send, word would go out to Germany in the third week of June. Allowing two weeks for the message to reach, say, Cologne (about 900 miles from Rome), the troops would then have about 140 miles to travel to Boulogne. This would take a week.¹⁶ Allowing them a week to prepare for their transfer and another week for crossing the Channel, we need not imagine more than six weeks elapsing between the message starting from Rome and the troops' arrival in Britain; they should have arrived by the end of July. During the weeks since the battle, 'omnis exercitus sub pellibus habitus est ad reliqua belli perpetranda,' and once the reinforcements

arrived, 'cohortes alaeque novis hibernaculis locatae.' But this is not evidence that winter was drawing on: our author continues, 'Quodque nationum ambiguum aut adversum fuerat igni atque ferro vastatum,' and there was time enough for that! 'Hibernacula' therefore means new quarters which they continued to occupy during the following winter. Also, it was after the reinforcements arrived, i.e. after July, that the natives started to suffer from hunger. This is just the timing we should expect, since the harvest is normally reaped in August.

We turn to the remaining events, again in Syme's summary. 'The procurator Julius Classicianus (successor to Decianus Catus) quarrelled with the legate and sent an unfavourable report to Rome. Thereupon the government dispatched a person of confidence, the freedman Polyclitus. His report was temperate, and Paulinus was kept in charge of operations. He was later superseded because of a mishap to the fleet.'

Since Catus fled almost as soon as the revolt broke out, we should expect a new procurator to be appointed once Rome had news of the final outcome, if not sooner. If Classicianus had been appointed immediately, he could have reached Britain by the middle of July. By the end of that month he may well have seen enough of Paulinus' conduct to send a critical letter to Rome. The letter would arrive in

mid-August; Polyclitus could have arrived by mid-September. After a fortnight's tour he could send his report, which would arrive in mid-October. The letter retaining Paulinus would arrive by the end of October. 'Later', say in mid-November, he loses some ships (only too likely at this time of year); this news would reach Rome at the end of November. The government are waiting for an excuse to get rid of Paulinus: this mishap provides them with one. Nero at once sends out Turpilianus, who arrives before the end of the year. Such is a possible timetable for 61, which demonstrates that the events described by Tacitus could all have taken place before the end of the year.¹⁷

This timetable does not imply an admission that Tacitus' narrative is incompatible with Turpilianus having arrived after December 61. As Overbeck has pointed out, even if we were to accept that Tacitus telescoped events of more than one year for the sake of continuity, 'it seems easier to leave the outbreak of the rebellion in 61, where Tacitus firmly fixes it, and suppose that Petronius came out early in 62. Thus, when Tacitus introduces his next topic with the words eodem anno he means ... the same year as that most recently specified, which is AD 61.'¹⁸ On this view the only events of 62 would be Paulinus' loss of the ships and handing over of his command - both of which are preceded by postea in the narrative.

But the purpose of this article has been to show that there is no conclusive reason to assume that Tacitus has telescoped events, and thus no reason to redate the revolt to 60.

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FOOTNOTES

1. Analecta Historica et Epigraphica Latina II (Bonn 1878), 8-16. The revolt is described in detail in Tac. Ann. XIV, 29-39. All further Tacitus quotations come from these chapters.
2. Two were Henze in P-W RE s.v. Boudicca, and B.W. Henderson, Life and Principate of the Emperor Nero (1903), 477 f.
3. R. Syme, Tacitus (1958), 765. The only published disagreement comes from J.C. Overbeck, AJPh 90 (1969), 143 f., and A.R. Burn in Tacitus, Studies in Latin Lit. and its Influence ed. T.A. Dorey (1969), 60 n.6.
4. He was curator aquarum in AD 63.
5. P. Gallivan, CQ 24 (1974), 290 f.
6. It was given this date without argument by A. Degrassi, I Fasti Consolari (Rome 1952), 23.
7. Ogilvie, note on Agr. 14,3.
8. Agr. 35; Hist. IV,33,7; Ann. I,51.
9. Livy 22,46,3; 23,29,4; etc.
10. JRS 63 (1973), 242-4.
11. G.D.B. Jones, Rhyn Park Roman Fortress: Excavations 1977 (Border Counties Archaeological Group 1977).
12. Cf. D. Petch in The Roman Frontier in Wales (2nd edition ed. M. Jarrett, 1969), 35.
13. Petch, ibid.
14. Jones, ibid.

15. A detail which does not emerge from the account in the Agricola. For an analysis of the sources behind the various accounts, see Latomus 33 (1974), 926 f.
16. Twenty miles a day was common for troops marching through friendly territory.
17. For a similar timetable, differing in some particulars, compare the plausible reconstruction in the novel 'Imperial Governor', by George Shipway (Manchester 1968).
18. Overbeck: see n.3 above.

TWELVE YEARS OF MYSTERY: AD 196-207

Events in Britain between 196 and 207 have recently become a matter for dispute. To paraphrase Mark Twain, "Scholars have already shed much darkness on the subject, and at the present rate we shall soon know nothing at all."

Yet until a few years ago there was general agreement. It was said that when the British governor Clodius Albinus crossed to Gaul in 196 to fight against Septimius Severus, he took with him the troops from Hadrian's Wall, which was left undefended. The Maeatae, who lived north of the Wall, swept over it, causing extensive damage both to the Wall and further south. Finally the governor Virius Lupus was compelled to buy peace from them for a large sum of money. Inscriptions from the Wall imply that it was rebuilt between 205 and 207. One was then left with a single problem - the idea that some eight years elapsed between its destruction and restoration.

However, in two articles with Dr John Mann as a co-author (hereafter referred to as 'Mann')¹ stress was laid on the statement of Herodian (3,14,1) that in 207 barbarians from the north had invaded the province and caused destruction, so the governor called for reinforcements or the emperor himself to come. Mann therefore discounted the theory of an invasion in 197, and said it was a major invasion of 207 which caused the emperor's arrival. In addition, he postulated that the Antonine Wall was occupied until 207. A.R. Birley

replied to Mann's views in an article in Archaeologia Aeliana for 1974.²

One might hope that archaeology could help in sorting out the problems, for example, in supplying us with a terminal date for the occupation of the Antonine Wall. The recent book by D.J. Breeze and B. Dobson³ concludes, 'For the date of the abandonment of the Antonine Wall, we must fall back on the ceramic evidence which, with all its uncertainties, appears to demonstrate a date in the 160s.' Yet they admit earlier that 'none of this evidence is self-dating, and depends on a relatively small number of dated deposits interpreted by even fewer highly specialised scholars.' When considering the numismatic evidence, they say it attests the occupation of the Antonine Wall down to 154, 'and the unstratified coins of later date hint at some continuing Roman presence in the Antonine Wall area.' These later coins include a Commodan hoard from Rumbling Bridge deposited not earlier than 186. And they go on to point out that some late Antonine samian and an inscription from Castlecary on the Antonine Wall both seem to imply its occupation in the 180s. Finally, they agree that the simplest interpretation of the literary evidence would suggest that the Antonine Wall was occupied until the end of the second century.

It is clear that the archaeological evidence is not yet sufficient to give us any firm date in this period of the Wall's history. A re-examination of the literary evidence might take us further. But before

entering into detail on the events of 196-207, we must first deal with the location of the Maeatae, a tribe prominently involved in these events.

The general agreement that the Maeatae were located in Fife is now disputed by only one prominent scholar, Professor S.S. Frere.⁴ He agrees that placename evidence (e.g. Myot Hill, Dumyat) 'shows that this confederacy included land north of the Forth.' But referring to Dio's statement (76,12,1), 'The Maeatae live next to the wall which divides the island in two, and the Caledonians live beyond them,' Frere says, 'It is sometimes argued that the Wall referred to was the Antonine Wall; but this does not suit the context in Dio's narrative who, writing of events in 208, must mean Hadrian's Wall, recently reconditioned by Severus.' From this he deduces that part of the confederacy of the Maeatae was south of the Antonine Wall, and presumes that, 'the Selgovae had joined the Maeatae.'

Dio refers elsewhere to one or other of the Walls. Under AD 184 he mentions (77,8,2), 'the wall dividing the British and Roman armies,' which conveys the distinct impression that Dio himself did not know which wall was involved on this occasion. But his remark about 'the wall which divides the island in two' reminds us strongly of Tacitus' description of the Forth-Clyde Isthmus (Agricola 23). Tacitus writes of the narrow stretch of land separating the estuaries of Clyde and Forth, and the hostile British forces being cut off 'velut in aliam insulam' by a precursor of the Antonine Wall.

It is striking that the three other sources which refer to Hadrian's Wall or its reconstruction under Severus say that it separated the barbarians from the Romans, not that it divided the island in two. Thus, Dio's phrase should imply the Antonine Wall, and indeed, would be a correct description of it even if it was not operational. But if Dio's ^{epitome} present tense gives the impression that it was still operational, this could be explained by its context. His description of the two tribes is part of an ethnographical digression which clearly derives from earlier sources,⁵ and there is no difficulty in supposing that when his ethnographic source was writing the Antonine Wall was still in operation.

There is therefore no evidence that the confederacy of the Maeatae spread south of the Antonine Wall, or that it included the Selgovae. We may locate the Maeatae firmly in Fife.

Let us now return to the events of 197. Against the theory of destruction in 196-7, Mann argued, 'If Britain was suffering from barbarian invasion in 197, it is difficult to understand why Severus did not visit the province himself, or at least send sufficient troops to deal with the crisis.' But this begs the question. Herodian (III, 8, 1-2) says that in 197 Severus 'set affairs in Britain in order, and divided the government of the island into two.' Unless the second half of the sentence merely

amplifies the first half, it would seem that he did make arrangements in Britain - which might have included sending troops there (he might, for example, have sent back the auxiliaries who had guarded the Wall) and indeed he may himself have come to Britain. This first argument of Mann's, then, will not do.

Elsewhere he argues, 'There is no evidence that the Parthian situation was so pressing that it had to be dealt with immediately after the defeat of Albinus' (in 197). But Birley rightly points out that the Parthian situation was in fact pressing, and would have needed Severus' presence, whatever the situation in Britain. That, then, could explain a lack of Roman military activity in Britain until 199, even if there had been an invasion in 197.

Mann continues, 'No pressing problem, military or otherwise, presented itself to Severus' (sc. between 199 and 207). 'It is difficult to imagine this energetic man allowing his governors to struggle for so long to restore the situation in northern Britain.' (Rather, Mann assumes the situation did not become dangerous until 207.)

Birley replies, 'This is surely to carry insularity to extremes. What of the Emperor's tour of Egypt in 199 and 200? What of the decennalia, the tour of his native Africa (including military operations on the borders of Tripolitania), the overthrow of Plautianus, and the consequent political upheaval? Can none of these items

be dignified with the label of "pressing"? The frank answer is 'No'. Severus would not have embarked on a leisurely two-year tour of Egypt, visiting antiquities, undertaking some of the governor's duties and making reforms in local government: all this would be incomprehensible had there been chaos and rebellion in Britain, requiring the Emperor's presence - he would have gone as soon as the Parthian campaign was over. Nor are the other items any more pressing; indeed, the dismissal of Plautianus could have taken place wherever the Emperor happened to be. If there was an invasion in 197, we cannot possibly blame 'pressing business elsewhere' for Severus' failure to arrive until 207.

So far, we may conclude only that by the year 199 the situation in North Britain was sufficiently quiet no longer to need the Emperor's presence. Do we then need to assume there was trouble earlier, in 197? Cassius Dio says, in a passage unfortunately devoid of its context, 'Since the Caledonians were not abiding by their promises and were preparing to assist the Maeatae, and because Severus was devoting his energies to the neighbouring war (with Albinus), Lupus was forced to buy peace from the Maeatae for a large sum, receiving in exchange a few captives' (Dio 76,5,4). Here we are dealing with either an internal rebellion in Fife, as Mann supposes, or an invasion into the Roman province, as Birley supposes. We cannot use the remark about captives as evidence of invasion: they

might have been soldiers from the Wall, but could equally have been traders or Roman citizens north of the Wall.

However, we are also told the Caledonians were 'preparing to assist' the Maeatae. Now, they might have assisted them by arresting or molesting Roman traders. But how could they prepare to assist them in this manner? The only possible 'assistance' seems to be by preparation of weapons and amassing of men for reinforcements for hostile military action. Which must be action being taken by the Maeatae at that moment, if they had already 'broken their promises'. And it is equally hard to see what that hostile action was, unless it were an attack on the Wall - whichever Wall it was. This correlates with the fact that it was partly because Severus was engaged on a war elsewhere that Lupus was compelled to buy peace: had Severus been able to come, peace would not have had to be bought. We may conclude that there was an attack on the Wall in 197, and from the fact that the Maeatae had to be bought off, the attack must have been successful.

This conclusion leaves us with the problem of why it is only in 205 that rebuilding starts on Hadrian's Wall. Birley offers three possible reasons:

- 1) The Romans had decided 'to resume the policy of Julius Agricola, and dispense with the need for a linear frontier at all.'
- 2) Evidence may yet appear of rebuilding on the Wall between 197 and 205.

3) The army may have been instructed to reoccupy the Antonine Wall after 197 - indeed, if one were faced with constant danger on Hadrian's Wall one way to try to improve matters would be to reoccupy the more northerly barrier, after subduing the tribes between the Walls.

The first idea may be dismissed: if such a decision had been taken, generals would have been campaigning in Scotland for some years before 205, and the historical record would surely reflect this. ?

The second idea is little more plausible. As Mann points out, it would imply that it took eight years to restore Hadrian's Wall - about as long as it took to build in the first place. On the other hand, if they spent some five years restoring forts south of the Wall before restoring the Wall itself, they would surely have reoccupied the Antonine Wall in the meantime, to stop further incursions from the north. However long the mysterious and uncertain Third Occupation of the Antonine Wall was, it cannot represent eight years occupation.

That only leaves us with the third view - that the Antonine Wall was occupied from 197 at least until 205. This only differs from Mann in that he disputes the invasion of 197. But if we follow him in his assumption that the Romans were still trying to maintain the Antonine frontier until 207, why is there a substantial rebuilding of Hadrian's Wall in

205-7? Mann adduces the fact that the name of the procurator Oclatinus Adventus appears on several of the inscriptions alongside that of the governor Senecio. He suggests one might 'see Adventus operating alongside Alfenus Senecio, as the personal representative of the Emperor, in the counter-measures to the growing hostility to the north of the frontier and in the preparations for the campaigns of the Emperor himself.' But neither of these would explain why Hadrian's Wall was being rebuilt in 205-7. Counter-measures might include a strengthening of the Antonine frontier perhaps, but not a retreat to the Hadrianic one. There can only be one explanation: yet another invasion, in 204-5, which persuaded those in charge that the Antonine frontier was no longer practicable, and a strategic retreat to the Hadrianic one was required.

Although this invasion is not attested directly in our sources, there are pointers. Dio (77,10,6) refers to Severus 'winning battles through others in Britain' sometime between 205 and 207. But a stronger pointer emerges from the character of Senecio. He was previously governor of Syria, and as Dr Graham points out,⁶ 'For Syria, long the greatest of imperial provincial commands, to precede Britain in a public career was only justified by some special crisis. Thus Pertinax went to Britain after his Syrian governorship in order to quell a revolt.' A similar reason may have led to Senecio's appointment in 205.

To turn to the supposed invasion of 207. Birley points out that Herodian's statement should be regarded with suspicion, and that the invasion he refers to might have taken place ten years beforehand. (If my suggestion of an invasion in 205 is credible, the gap would be only two years.) But as we have seen, it is impossible to maintain with Birley that right up to the year 207, "Severus might have been risking the future of his reign and his dynasty by crossing the Channel." After two years rebuilding Hadrian's Wall in 205-7, some new factor must have brought the Augusti to Britain, and an invasion is the obvious conclusion. As Birley admits, "It may indeed be the case that there was damage on Hadrian's Wall - though on a lesser scale than Herodian suggests - all over the north c. AD 207."

To conclude. The evidence suggests invasions in 196, 204-5 and 207. It is hardly surprising that the third invasion was enough to decide Severus that all such artificial frontiers were impossible to hold in Britain. The only course was to remove the frontier altogether, and conquer Scotland.

FOOTNOTES

1. AA⁴ 48 (1970), 1 f., BJ 170 (1970), 178 f.
2. AA⁴ 50 (1974), 179 f.
3. Hadrian's Wall (London 1976), 120.
4. Britannia, A History of Roman Britain (1974), 138.
5. Cf. C.R. Whittaker in the Loeb Herodian (1969), note on 3,14,8.
6. JRS 56 (1966), 103.

WHEN WAS BRITANNIA DIVIDED?

Herodian, referring to the time after the Battle of Lugdunum in 197 AD, says that Septimius Severus "set affairs in Britain in order, and divided up the governorship of the province into two commands" (III,8,2). Cassius Dio (LV,23,2,3) gives us a few details which help to determine the boundaries of the two provinces in about 220, the time when he was writing. From this passage, it emerges that Britannia Superior, i.e. southern Britain, held two legions, while Inferior had one. As a result, we should expect an ex-consul to have had charge of the two legions in Britannia Superior, and to have been stationed there.

Yet under the reign of Severus, ^{we find} ~~there are~~ no less than three ~~occasions when we find~~ consular governors operating in the north of Britain, in Inferior.¹ S.N. Miller² suggested the consular governor could take over in cases of emergency, but if this happened at least three times in ten years, the division of the province becomes meaningless. In 1954 E. Birley,³ improving on some views of Ritterling,⁴ suggested that the Severan division differed from that attested by Dio, and that under Severus there was a shortlived procuratorial province in north Britain.

In 1966 Dr A.J. Graham rejected Birley's view, and the evidence of Herodian. Instead, he proposed that no division took place until the reign of Cara-

calla. Graham gave two main reasons for rejecting Herodian's evidence. First, Britain had some very distinguished governors under Severus, and this is less likely had they only been in charge of half the former province. Second, he replied to the argument from the analogy with Syria. Miller had pointed out that after Severus defeated Pescennius Niger, he divided the province Niger had governed. This would help to ensure that no Syrian governor could pose a similar threat through his control of the three legions there. It would be logical, said Miller, to assume that Britain was similarly divided, after the British governor Clodius Albinus had been defeated. In reply to this, Graham noted that the governor of Pannonia also had three legions, but this province was not similarly divided by Severus. This, said Graham, was impossible to explain, given analogies with Syria and Britain. Rather, we must assume that Herodian was incorrect, and indeed that he "may have been the first to misapply the analogy with Syria."

The following year Drs J. Mann and M. Jarrett disputed this conclusion, and attempted to reinstate Birley's view.⁵ They pointed out (in a last-minute footnote) that Herodian does not actually mention the division of Syria, and so is hardly likely to have 'misapplied the Syrian analogy'. Nor is the position of Pannonia any more persuasive: Severus had been governor there when he marched to Rome, so this is the one province where he might have expected the

troops to remain loyal. They also answered Graham's point about the high status of Alfenus Senecio and other governors, and demonstrated that Senecio's arrival in the difficult circumstances of 205 need not imply a demotion for him, even if the province was divided.

At that point the argument has rested, with no really clinching argument on either side. Later authorities have tended to adopt the Mann/Jarrett hypothesis: presumably because the division makes good sense as Severan policy in 197, and because there is little reason to doubt Herodian's evidence.⁶

But there is perhaps further evidence which may be brought into play: that of the later writer Orosius. We must of course always bear in mind the bias of our sources, and Orosius certainly had an axe to grind. It was his purpose to reply to pagan writers who claimed that the arrival of institutionalised Christianity was a major cause in the disintegration of the later Empire. He tried to show instead that it was the arrival of Christ which was accompanied by peace, and that before his arrival the Empire was much more faction-ridden.⁷ However, when it comes to describing Britain as 'Britannia' or 'Britanniae', Orosius has no axe to grind - the status of Britain was quite irrelevant to his main theme. He would therefore simply have copied, and doubtless abbreviated, the references in his source.

If he should sometimes refer to 'Britannia' in

the singular, when referring to events later than AD 220, that does not of course imply that Britain was undivided: the writer is simply referring to the island of Britain. But if he refers to it as 'Britanniae' in the plural, that should imply that he and his source regarded Britain as divided by that time. The following are Orosius' references to Britain up to the time of Carausius:

- VI,9,2 "(Caesar) in Britanniam transvehitur."
- VII,5,5 "(Caligula) circa prospectum Britanniae restitit."
- VII,6,9 "Claudius ... expeditionem in Britanniam movit."
- VII,17,6-7 "Albinus tamen apud Lugdunum oppressus et interfectus est. Severus victor in Britannias defectu paene omnium sociorum trahitur. Ubi magnis gravibusque proeliis saepe gestis receptam partem insulae ... vallo distinguendam putavit."
- VII,25,3-4 "Carausius purpuram sumpsit ac Britannias occupavit ... Carausio in Britanniiis rebellante"
- VII,25,6 "Carausius, Britannia sibi ... retenta, tandem ... interfectus est. Allectus postea ereptam Carausio insulam ... tenuit, quem Asclepiodotus ... oppressit Britanniamque ... recepit."

The inference is clear. By the time Severus invaded Britain, Orosius and his source⁸ regarded Britain as having been divided. Thus Herodian is not alone in his dating of the division, and the case for a division under Severus is greatly strengthened.

FOOTNOTES

1. For an introduction to the detailed problems, see A.J. Graham, JRS 56 (1966), 92 f.
2. CAH xii, 36 f.
3. Trans. Cumb. and Westm. Arch. and Ant. Soc. 53 (1954), 52 f.
4. RE xii, s.v. legio 1609.
5. JRS 57 (1967), 61 f.
6. Cf. e.g. A.R. Birley, Septimius Severus (1971), 247 n. 1, and S.S. Frere, Britannia, a History of Roman Britain (1974), 205.
7. This bias leads him to misdate two key events in Augustus' reign: cf. Latomus 32 (1973), 779-85.
8. The identity of Orosius' source for this passage is unknown. For a brief discussion of his sources in general, cf. B. Lacroix, Orose et ses Idées (1965), 60.

THE SCOTTISH CAMPAIGNS OF SEPTIMIUS SEVERUS

In the years 208-10 the emperor Septimius Severus and his son Caracalla were campaigning in Scotland. Agreement on even this point has only been reached in the last few years, and virtually everything else about the campaigns is still matter for dispute. But archaeological evidence has been rapidly accruing in the last ten years, and is apparently now sufficient, when taken in conjunction with our other evidence, for us to erect a framework for the campaigns and to draw some wider conclusions.

Our evidence for the campaigns is four-fold: literary, numismatic, epigraphic and archaeological.¹ Of these, the epigraphic is very slight, and the ancient historians are infuriatingly imprecise. We may start by considering the implications of the coins.

Perhaps the most intriguing are two coins which depict bridges, and which are dated to 208, though that of Caracalla may belong to 209.² Both bridges can be compared with those appearing on coins of previous emperors. That of Caracalla, which shows a bridge of boats with the legend *TRAIECTVS* underneath, is similar to a bridge on coins of Marcus Aurelius of 172, showing his crossing of the Danube.³ The other coin, of Severus, shows a permanent bridge, which is identical to that issued by Trajan to commemorate his crossing of the Danube.⁴

It is normally suggested that these coins commemorate bridges over the Forth of Tay, or both, and there is no

reason to dispute this. Where then were they built?

Discussion has focussed round the boat-bridge, and in 1931 Sir Charles Oman argued that it was built across the Forth. This suggestion has since been discounted,⁵ and there are certainly topographical reasons for doubting a crossing at Alloa, where Oman placed it. He admits the position would involve crossing a broad expanse of soft tidal mud. Moreover, no Severan - and hardly any Roman - finds have been made at Alloa, and the forth is in any case not wide enough for an impressive bridge.

More recently the suggestion has been made that the boat-bridge was situated at Carpow on the Tay.⁶ We can be virtually certain there was some sort of bridge here, because air-photography has detected a bridgehead fort on the northern bank of the river, facing the fortress on the south. A bridge of boats here is certainly possible, and it would follow that Severus' permanent bridge was on the Forth - at, say, Oman's site of Alloa.

But there are just as strong objections to a permanent bridge as to a boat-bridge at Alloa. In any case, Stirling would be the natural crossing-point. However, we cannot place the permanent bridge at Stirling either. This follows from the discovery of a 63-acre (and therefore Severan) marching-camp at Craigarnhall, which "lies just to the north of the ford at Old Keir, one of the lowest fords on the Teith, and the importance of the discovery lies in the clue it affords to the Roman crossing of the rivers

Forth and Teith and of the marshes between them."⁷ This camp clearly belongs to the double set of 63-acre camps north of the Forth, which must be dated between AD 208 and 210. Its existence implies one of two things: either they forded Forth and Teith near this point, or they built small field bridges to cross these rivers. Whichever they did they would not have built a permanent bridge worthy of commemoration on a coin. So if Craigarnhall belongs to 208 we could not explain the coin. But if it belongs to 209 or 210 (as I shall try to show) they cannot have rebuilt the permanent Antonine bridge at Stirling in 208, or they would have reused it in 209 or 210, and would not have crossed near Craigarnhall. Stirling, then, may also be ruled out. Nor could we imagine a permanent bridge east of Alloa in 208: Kincardine can be ruled out for the same reasons as Alloa, and below that point the river is far too deep for a Roman permanent bridge.

The hypothesis of a boat-bridge at Carpow, then, while unobjectionable in itself, obliges us to suppose a permanent bridge somewhere across the Forth in order to explain the other coin. But that leads into an impasse, since there seems to be nowhere along the Forth where we might even hypothetically assume a permanent bridge in 208 worthy of such commemoration. We must therefore go back one stage, and examine the alternative view, which is to suppose a permanent bridge across the Tay.

Given the virtual certainty of a bridge at Carpow,

this might be the site of a permanent one. As St. Joseph says, "The distance between firm ground rising on either bank from the edge of reed-beds flooded at high tide is now some 1,100 yards, of which only half is exposed open water." It is across here, in a straight line between the fortress and its bridgehead fort, that the boat-bridge has been conjecturally placed. Yet two factors suggest that the bridge was to the west of this point. The road which fronts the principia in the fortress, and the NE/SW axis of the bridgehead fort at St. Madoes both point to almost exactly the same point of the river, 200 yards to the west of the conjectural line. This point is just before the river starts to widen to embrace Mugdrum Island -- but in any case, if we are now thinking of a permanent bridge, we should expect it to be sited where there is half the width of clear water to cross, compared with the conjectural straight line. The width of clear water just before the river widens is only 300 yards, leaving 400 yards of marsh and reeds on either side to be negotiated before firm ground is reached. Erosion of the southern half of St. Madoes fort shows that the distance would have been even less in Roman times. Thus, the siting of the permanent fortress indicates that when this was built any bridge would have crossed at this western point, and this is the obvious position for a bridge in 208 also. The making of a roadway through 800 yards of boggy ground might well call forth comment in our sources: certainly, it ought to indicate some of the preparations

which we are told were involved. It might also explain Herodian's references to γέφυρα. These pontes are not ordinary bridges over water, because the description he gives is clearly of the technique used to cross marshes by log-causeways.⁸ Such causeways must have been used at Carpow to approach the bridge across the river. There seems no reason to doubt the practicability of a 300-yard long permanent wooden bridge at Carpow. The river is never more than twenty feet deep at this point, and in fact compared with the 1100-yard length of Trajan's bridge (and Severus' coin invites the comparison) the bridge itself was perhaps not all that impressive. On the other hand, the causeways through the marshes would indeed have been an impressive technical feat, and to the natives the complete structure should have been just as frightening and over-awing as that built by Julius Caesar across the Rhine to reach the Sugambri. Here the natives were terrified in looking upon "Rhenum suum sic ponte quasi iugo captum."⁹ A 'captured Tay' must have roused similar feelings in the Scots. At any rate, the existence of this bridge is a hypothesis we can test: excavation could well reveal traces either of the causeways buried among the reeds, or remains of the piles of the bridge in the riverbed.

A permanent bridge on the Tay, then, is certainly a possibility, and this hypothesis requires a boat-bridge on the Forth in 208. Alloa and Stirling may be ruled out, for reasons already given. The only advantage in a boat-

bridge to cross the Forth would have been to cross it much further downstream in 208. For example, it could have given them a substantial advantage in cutting straight across into Fife, instead of taking the traditional roundabout route. Is there then a possible site for a boat-bridge much further downstream?

The idea that Caracalla could have had a boat-bridge built where the Forth Railway Bridge now stands, between North and South Queensferry, did indeed occur to Oman. But he dismissed it for various reasons, and concluded that if such a feat had been carried out, "It would indeed have been a TRAIECTUS worthy of commemoration in a first brass or a medallion, not merely a modest dupondius." It is ironic, therefore, that he continues, "It seems hardly possible to doubt that since this coin was struck for Caracalla, there must have been a similar one for his father Severus. But if only one specimen of Caracalla's issue survives, there is every reason why the chance of fortune should have obliterated his father's corresponding coinage altogether." As we have seen, the corresponding coin has survived -- it merely came to light after Oman wrote -- but this drives home the fact that we cannot argue from the absence of a coin (more particularly, a medallion) that it did not exist. There may very well have been first brasses or medallions commemorating the TRAIECTUS, but in the nature of things, the more valuable a coin, the more likely it was to be melted down and the metal reused later.

Oman's other objections have rather greater force.

"The width of the Forth, though so much smaller than at any other point till one reaches Alloa going up stream, is 2,765 yards -- a mile and two-thirds -- ... and I think that such a width makes impossible any idea of a bridge of boats, when one considers the size of Roman vessels, and the sway of the tides on such an enormous breadth of water, not to speak of the danger from storms." I shall deal with these objections in turn. But first, it may be as well to draw a brief conspectus of ancient boat-bridges and their potentialities.

The best known boat-bridge of antiquity is that of Xerxes, who 'yoked' the Hellespont to cross into Europe. His bridge was seven stades -- that is, about 1400 yards -- long, and the boats supporting it were anchored against a steady five-knot current. It was partly to outdo him that Caligula had his own bridge built: the longest known in antiquity. Built in AD 39, it stretched from Puteoli to Bauli, a distance of over three miles. Recently, what seems to be a mooring-block for the bridge was found near Bauli, and it is fully consonant in size.¹⁰ Now in deep water, it is a square block of Roman concrete ten metres high and thirty metres square at the base, tapering to twenty metres square at the top, which would originally have been level with the surface. The bridge itself was a double line of ships joined together, on top of which a mound of earth was heaped and fashioned to resemble the Appian Way,

complete with resting-places and lodging-houses along it. It only lasted two days before Caligula grew tired of it, but there were more permanent boat-bridges in the Empire: one at Zeugma in Syria,¹¹ and another at Arles, which is apparently that depicted on a mosaic at Ostia.¹² Temporary boat-bridges to attain a military objective were much more commonplace. The equivalent of the modern military pontoon bridge, their advantage was and is that they enable a large body of troops to cross much more quickly and safely than by ferry, and they also enable heavy equipment to be transported across water without having to be dismantled first. Dio says, "Rivers are bridged by the Romans with the greatest of ease, since the soldiers are always practising bridge-building, which is carried on like any other warlike exercise, on the ~~later~~, Rhine and Euphrates." He goes on to describe in detail one method of constructing such boat-bridges.¹³

Now, Oman's figures of 2765 yards is a gross overestimate: the actual width is about 1800 yards, not that much wider than the Hellespont.¹⁴ He also fails to mention the equally important fact that the estuary is neatly divided at this point by the small rocky island of Inch Garvie -- the railway bridge in fact has one of its feet planted on it -- so two bridges of much shorter length would have been required. From North Queensferry to the tip of Inch Garvie is some 550 yards, while from Inch Garvie to the nearest point of land to the south, Long Craig Gate, is just under

1000 yards -- 400 yards less than the length of Xerxes' bridge. The length in itself, then, would be a small problem.

A more substantial difficulty would be the depth of the water. Thus, between North Queensferry and Inch Garvie it can be as much as 180 feet, and averages about 150. Between Inch Garvie and Long Craig Gate, the southern 600 yards is only nine feet or less in depth, but the other 400 yards can be as much as 200 feet. In one way, of course, this depth is an advantage: it means that the current is much less strong than one might otherwise expect. Actually, it does not seem difficult to believe that ships could be anchored even in 200 feet of water -- ancient cables could be far longer than this. Thus, the bridge might have been constructed in the usual way, by sending the boats down one by one, and anchoring them in turn. But at the same time, Caligula's mooring-block indicates that where water was exceptionally deep, one big mooring-block at the end could suffice for an immense boat-bridge. Across the Forth, such a block would not have been necessary: there is plenty of room on the shore for placing the winches or bollards necessary; in Italy, the sheer cliffs may have prevented this at the southern end. And if this technique of fastening the bridge was adopted here, they may have followed the practice attested by the Anonymus Byzantinus, of building the bridge to the required length alongside the riverbank, and then towing it into its correct position. They could then have let down one or two large anchors (or the baskets of rocks described by Dio) into the deep water, for extra

stability.

At any rate, from the technical point of view, a bridge across the Forth was quite feasible for Roman engineering. The major danger would be from storms, which can be severe in this area. But storms, of course, can destroy a bridge wherever it may be built -- indeed, one did destroy the first of Xerxes' bridges, though that did not deter the Persians from building a second, and this time successful, bridge. All one can do is to wait for a day with perfect weather, and hope it will stay reasonable for 48 hours. After all, they should not have needed more than this to transport the troops and supplies across.

I would therefore suggest that a 1 1/4 mile bridge of boats, divided near the middle by Inch Garvie, was constructed under the guidance of Caracalla where the Forth Railway Bridge now stands. This is less than three miles from the important Severan harbour and stores-base at Cramond, so that it would have been a simple matter for supplies to be brought by sea to Cramond and then carried across the bridge. This was the TRAJECTUS Caracalla celebrates in 208, showing both Severus and himself marching across the bridge. Unfortunately, there is little hope of finding archaeological evidence for it now.

However, if it were true that the two points where the army crossed in 208 were Queensferry and Carpow, we might hope to find some traces of the route it took between those points -- and such traces could add support to the case for a crossing at Queensferry. The route taken in 208 has in

fact been a major source of puzzlement. Dio lays stress on road-building generally: "Severus encountered numerous difficulties in cutting down the forests, levelling the heights, filling up the swamps and bridging rivers." As Richmond says, "Much preparation of roads was undertaken in marshy country, a description suitable to Angus and the Mearns or to Fife, but hardly to the long-established land-route between Forth and Tay."¹⁵ If we have correctly located the two crossing-points concerned, the most natural assumption would be a new road in Fife, running in a reasonably direct line between Queensferry and Carpow. And indeed, if Severus was already planning to campaign in Scotland, it would be sensible to build a permanent roadway at least as far as Carpow.

There are several finds which support this hypothetical route. First is the recent discovery of a 63-acre (and thus Severan) marching-camp at Auchtermuchty, five miles SSE of Carpow. Second, there is a report of a "rectangular and rectilinear inclosure, within which was found a silver coin of Pertinax." This was seen before 1812 and (if we can trust the report) must surely have been another Severan marching-camp, situated about two miles east of Lochore, near Auchterderran.¹⁶ Lastly, a hoard of more than 600 coins ranging from Galba to Severus was discovered at Portmoak on the east side of Loch Leven in 1851. Most naturally interpreted as hidden by a soldier on Severus' campaigns, these coins should be on or near to the line of march.¹⁷ It may be no coincidence that "The field where

they were found was formerly a deep bog marsh which, by draining and burning, has now been brought into cultivation." It was through just such bogs that our sources imply the road was built.¹⁸ The line of the Severan road of 208, then, may well lie from North Queensferry via Auchterderran, then east of Loch Leven, and via Auchtermuchty to Carpow. If the road was here we could also explain why it has not so far been detected. Many stretches would lie through marshy ground, so that instead of the normal stone-paved or gravelled agger, one would only expect to find the log-causeway by excavation: either in ground which has remained marshy, or by detecting the tell-tale change in soil colour which would attest decayed wood.¹⁹

At the beginning of the campaigns, Herodian stresses the preparations for war, including the building of log-roads. He continues, "Once the preparations for war seemed to be satisfactorily completed, Severus summoned Geta, and left him to ... carry on the civil administration of the empire.²⁰ Antoninus he took with him, and advanced against the barbarians." The impression given is of a substantial time spent in such preparations before an advance against the enemy. And these preparations were apparently those which we have deduced were taking place in Fife. This suggests that in 208 Fife was not hostile.

At this point we can turn to the dating of Severus' campaigns, which are generally accepted to have taken place between 208 and 210. Since our sources suggest there were

only two campaigns, the second has been dated to 210, and the first to '208-9'. Now, recent writers almost all agree that the Maeatae were in Fife.²¹ So if we suppose that 208 was concerned only with a march through their peaceful territory, and preparations for the advance north, we can then neatly slot the two fighting campaigns into the years 209 and 210.

So the most convenient assumption would be that the Maeatae did not resist in 208. Perhaps a bargain was struck, whereby the Romans agreed not to take revenge on them for their hostility in some previous year, on condition that the Maeatae allowed them to build their roads and bridges without continual sabotage.

We may now consider the campaigns of 209-10, for which our most important evidence is the marching-camps. There are two major sets of camps which have now been fairly securely dated to the Severan period.²² One set, 63 acres in size, has a few examples south of the Forth, but the main series runs in two roughly parallel lines, the northwestern as far as Keithock, the other up to Kinnell, but almost certainly aiming for the Montrose Basin. The second series is of 165 acres south of the Forth, but then drops to 130 acres and continues up at least as far as Kair House, twelve miles north of Montrose. We should most naturally expect the more northerly (130-acre) set to belong to the final campaign of 210, and the other set to 209; archaeology has offered support for this by showing the 63-acre set to be earlier.²³ St. Joseph suggests that the 63-acre camps, in

two lines, mark a 'round trip' by one force. However, 63 acres is almost exactly half the size of 130 acres, and it is most unlikely that half the army was not being used during the first advance into hostile territory. It is much more likely that in 209 the army was divided into two, and led, no doubt, by Severus and Caracalla.

We may be able to go further, if we consider the question of numbers. It is normally assumed that Severus had three legions for these campaigns - the three stationed in Britain - and that when brought together "with a substantial number of auxiliaries or other troops" they occupied the 130-acre camps.²⁴ But R.E. Smith has shown that Severus also took at least part of the praetorian guard with him.²⁵ Its strength was now 10,000, which is virtually the equivalent of two legions. We can also be almost certain that Severus brought the new legion II Parthica with him as well.²⁶ Thus, Severus probably had the equivalent of six, not ~~three~~, legions campaigning in Scotland.

Bearing this in mind, it is interesting to consider the size of the marching-camps. Normally it is assumed that a 20-acre camp would have accommodated one legion. Thus Frere, building on an idea of Richmond, refers to the 20-acre camp at Rey Cross as holding one legion, and then 61-acre camp near Neath, he says, "would theoretically hold three legions."²⁷ This is also the most plausible interpretation of the Severan 63-acre camps - but it follows that 130 acres should represent a force equivalent to six legions,

which fits nicely with the force detailed above. Now, it might be objected that by the Severan period it was a common practice for detachments of legions to be taken on campaign, rather than whole ones: The reason presumably being to avoid any frontier being excessively weakened if whole legions were taken from it. But this could not apply in Britain, where temporary removal of the legions from Caerleon, Chester and York could not conceivably endanger the frontier.²⁸

Let us return to the suggestion that the army was divided in two, and led by the two Augusti, who advanced in parallel, planting 63-acre camps as they went. Severus would presumably take with him his Praetorian Guard and II Parthica, while Caracalla would lead the three legions stationed in Britain. He was thus in charge of legions who were used to fighting in Britain, and he was also quite fit, unlike his father, who suffered from gout. Thus Caracalla will have taken the potentially more hostile western route, following Agricola's old route as far as Keithock. Severus will have taken the eastern route, crossing the bridge at Carpow. And by the end of the year, matters had gone sufficiently well for them to claim the title BRITANNICVS.

We turn now to the function of the navy in these campaigns. Its activities are attested in various ways. The most evident is the establishment of vast stores-bases at South Shields, Cramond and Carpow. All these were clearly turned into such depots because of their proximity to good harbours. Coins of Neptune and Oceanus dated to 209 must also attest fleet activity. That other fleets

were involved as well is shown by an inscription set up to a Prefect of the Fleets of Britain, Germany, Moesia and Pannonia, all apparently combined for the expedition.²⁹ Presumably they were taking part in a joint campaign with the army, something of which we have a splendid description in chapter 25 of the 'Agricola'.

In this year (82) Agricola used his fleet to explore harbours 'quia infesta hostili exercitu itinera'. Severus, wise to the same considerations, followed a safe route through Fife in 208. But in 210 we have observed the drop from 165 to 130 acres once the Forth is crossed. We could link this with an inscription from Corbridge (RIB 1143) mentioning someone in charge of the granaries during Severus' expeditions. This must imply that Corbridge held supplies, which were presumably to be sent up Dere Street -- and Dere Street was used in 210, as the 165-acre camps show. Thus, the extra 35 acres are likely to designate an enormous baggage train, and indicate that much of the grain went by ship north from the Forth.³⁰ This is also suggested by the many coins of ANNONA in 209-210.

Agricola succeeded in bringing the natives to battle at Mons Graupius, and the resultant massacre must have crippled them for years. Severus presumably intended a similar result. What he did not anticipate was the natives' unwillingness to provide him with a second Mons Graupius. By this time they had learned the lesson taken to heart by modern guerillas: that guerilla tactics are the only, but highly effective, defence of primitive peoples against forces

vastly superior in equipment and organisation. As Dio says, 'Severus fought no battle and beheld no enemy in battle array ... A full 50,000 Romans died.'³¹ Nevertheless when he finally returned from the north, 'he had forced the Britons to come to terms, on condition that they abandon a large part of their territory.' The reason for their surrender and concessions is obscure; possibly they surrendered because of lack of supplies. At any rate, Severus regarded this surrender as sufficient to claim a Victoria, and the SHA, referring to his return, shows that he regarded the Scottish problem as finally settled: 'Post murum apud Luguwallum visum in Britannia, cum ad proximum mansionem rediret non solum victor sed etiam in aeternum pace fundata ...'³²

However, whatever the details of this settlement, it must have been completely invalidated by later events, since it was also in 210 that the Maeatae, followed by the Caledonians, revolted. This revolt of the Maeatae occurred after Severus' return from the north, probably in the second half of 210. The reason for their revolt has often been put down to the severity of the terms imposed on them earlier. That is possible, though the terms mentioned above were apparently imposed only on the Caledonians, so we should have to imagine harsher terms suddenly imposed on the Maeatae as well, in spite of their quiescence from 208.³³ But the reason may alternatively be that Severus' very failure to inflict a military defeat on the Caledonians persuaded the Maeatae that they could lose little, and might gain much, by adopting similar guerilla tactics. Severus retaliated by sending

the soldiers to carry out a policy of wholesale extermination. Given that the settlement with Caledonia seemed to have settled the Scottish problem once and for all, we can well imagine the exasperation which must have prompted Severus to this policy.

Dio continues, "When this had been done, and the Caledonians had joined the revolt of the Maeatae, he started preparations to make war on them in person. But while he was thus engaged, his sickness carried him off on the fourth of February (211)." In other words, the Caledonians only joined the revolt once Severus had embarked on his brutal policy towards the Maeatae. There can be little doubt that that was the reason why they revolted as well: they were not going to stand idly by while their fellow Scots were being systematically massacred. We can also deduce that the revolt of the Maeatae started pretty late in the year. The Caledonians must have joined in fairly soon after the massacre started, but Dio implies that by that time it was too late in the year for Severus to deal with them also. If we also take a rescript of the Emperors datelined York, 5th May 210 as evidence that they were still in York at that time,³⁴ we may reasonably conjecture that the major campaign, now led by Caracalla alone (as Herodian III, 15, 1 implies), started in June, was over by September at latest, and the Maeatae revolted in September or October.

We must now leave these matters for the moment, and turn back to the events preceding the occupation of Fife

in 208. The archaeological evidence consists in the series of 63-acre camps south of the Forth, of which only three have been discovered so far: Kirkpatrick, seven miles north of Burgh-by-Sands on Hadrian's Wall, and probable examples at Castlecraig and Eskbank near the Forth. Their size should indicate a force equivalent to three legions advancing from the Wall, and if assigned to 208 or 209 we should have to suppose that a more easterly set, to contain the other half of Severus' army, has not yet been detected. This is possible, though unlikely. 210 may be ruled out, as the army in this year is accounted for by the 165-acre camps. This campaign in the territory of the Selgovae, then, might rather belong to 207, and may have been undertaken by the three British legions, not by the forces brought by Severus in 208. If there were no further evidence we should most naturally assign this campaign to the governor Senecio.

But there is other evidence. It is still sometimes assumed that coins of 208 with PROF AUGG indicate the departure of the Emperors from Rome in that year.³⁵ This will not do: the coins are repeated in 209, when they can only indicate departure from their British base to the Scottish front, and the coins of 208 may indicate the same.³⁶ But coins clearly indicate that Caracalla was active in 207 in some province. There are several Caracallan issues of 207 showing Mars, others with Virtus, i.e. his military prowess, and most significant, some showing him with captives and a river-god. These just might refer to some mysterious and badly-documented events on the Continent.

They are more commonly thought to refer to Britain.³⁷ To add to them there is another coin, or medallion, on which stress can be laid.³⁸ Dated to 207 and inscribed ADVENT(us) AUG(usti) GALL(iae), it shows Severus alone on horseback arriving at the walls of a city. There is no reason to dispute its genuineness,³⁹ and the singular AUG and depiction of Severus alone strongly suggests that he was not accompanied by Caracalla on his arrival in Gaul, whatever impression the historians give to the contrary. Given the prominence which Caracalla has on the British coinage in 209-210 it is inconceivable that he arrived after Severus, and much the simplest view is that he was already in Britain in 207. In that case, it was probably he who led the British legions through the territory of the Selgovae in 207; the captives on his coin will be from that tribe, and the river may well be the Solway Firth, which he could have crossed (via a ford) to reach Kirkpatrick.

If Caracalla was already in Britain in 207, and Severus in Gaul, we may then turn to the rescripts from 208. The first two, of the 12th and 18th of February, were issued by Caracalla alone. That of the 10th of March, and four of the other five rescripts of this year, were issued by the joint Augusti.⁴⁰ The natural conclusion is that Severus joined his son in Britain between 18th February and 10th March. We may now offer an explanation for a puzzling coin of 208. This is a rare type depicting Severus seated as a magistrate on a curule chair, being crowned by Victory and resting his elbow on a kneeling figure. The legend is

VICTORIAE AUGG. Since both Emperors were in Britain in 208, this is not likely to refer to Africa.⁴¹ Rather, it might commemorate the resumption of Roman administrative power over Fife. The non-military character and attitude of the figure of Severus suit the diplomatic agreement we have deduced above, and the kneeling figure in that case would be a member of the Maeatae.

Summary of the campaigns

Since the train of argument has obliged me to treat these campaigns out of chronological order, I now give a summary in the correct order.

207

Caracalla comes to Britain. Conducts campaign with the 3 British legions from Hadrian's Wall up to the Forth, through territory of Selgovae. Severus arrives in Britain late in year.

208

Uncontested advance of both Augusti through Fife (Maeatae), establishing bridges at Queensferry and Carpow with a road between.

209

Caracalla advances with 3 British legions against Caledonians, close to old Agricola's route up to Keithock. Severus, with Praetorian Guard and II Parthica advances via Carpow to Kinnell (and probably Montrose); both leave 63-acre camps. Fleet, drawn from 4 navies, brings supplies north from South Shields.

210

Advance of all forces, with supplies, from Corbridge to Inveresk, planting 165-acre camps. Army, under Caracalla, continues near old Agricola route at least as far as Kair House, planting 150-acre camps. Fleet sails north from Cramond, taking supplies. Extensive casualties; no big battle; concessions made by Caledonians. Fleet circumnavigates Britain and Victory claimed. Late in year, Maeatae revolt. Severus' attempted genocide provokes Caledonians to revolt as well.

The Purpose of Severus' Campaigns

In the case of Agricola, no historian tells us his purpose in invading Scotland, but no-one doubts that he did intend to occupy south-east Scotland, if not more. Our evidence, normally accepted as conclusive, is the building of the permanent fortress, intended for the 20th Legion, at Inchtuthil. If a force was going to be stationed here permanently, they must have intended to keep a hold on the land around. But in that case the establishment of the permanent fort at Carpow indicates a similar intention on Severus' part to hold the surrounding land, and its demolition soon after no more negates this intention than the similar demolition of Inchtuthil. One argument has been given against this view. "It is evident from Severus' neglect to garrison the lowlands that he was not intending a permanent occupation of Scotland."⁴² But if

Severus felt confident of victory in the far north he may have thought garrisons unnecessary in the lowlands.

What was so special about Fife, sufficient to warrant Roman occupation? The question was asked by Professor E. Birley some forty years ago,⁴³ and still no answer has been offered. Yet Fife was under some form of Roman control before Severan times, and we can obtain some idea of the date of this control from the same article by Birley. In it, he suggests that a woman condemned to the salt-mines inside a province, but captured by bandits from an extera gens across the frontier, was in fact serving her sentence in Fife. She was brought back by the centurion Cocceius Firmus, whom Birley identifies with a veteran who appears on an altar dated to 169 found at Histria, on the Black Sea. Since he was already a veteran in 169, the incident referred to, and thus the subordinate status of Fife, presumably as some form of protectorate, will predate 169. The extera gens would of course be the Caledonii, not the Macatae, and the protectorate status of Fife would most naturally be associated with the building of the Antonine Wall.

As to the value of Fife to Rome, Birley dismisses the possibility that coal there should have been sufficiently important to make Fife valuable; nor should we expect salt by itself to be any more important: there were plenty of salt-pans elsewhere in Britain. On the other hand, Fife cannot have been so marshy that corn-growing and farming were impossible, and we might link this up with the very high

frequency of coins depicting ANNONA in 209 and especially 210. These may simply be a reference to the importance of the fleet in bringing supplies north. But we might also see in them a reference to the fact (if it was one) that the Maeatae were now paying tribute in corn, like any other state in Britain or the Empire. Indeed, tribes through whose territory the Romans were marching normally had to contribute more to the annona than in time of peace. Also, their subordinate or neutral position towards Rome must have produced a useful buffer state against the Caledonians, and when they were peaceful, the risk of attacks on the eastern half of the Antonine Wall must have been greatly reduced.

Any one of these factors in isolation was perhaps insufficient to make their subjection seem worthwhile. Nevertheless, the combination of buffer-state, and its resources in coal, salt and especially grain, would probably have appeared enough to pay for the extra troops installed in it. When not occupied, their friendly disposition would itself be useful in maintaining security along the Antonine Wall.

The Severan plan, then, involved an occupation at least of Fife. Did he intend anything further? According to Dio (lxxvi,13,1), he intended to conquer the whole of the island. And it has sometimes been suggested that Agricola's plan was to occupy and garrison the whole of the Scottish Highlands. Analogies are drawn with the mountainous areas of Wales and Spain, which were both occupied. However, there are two

vital differences between these countries and Scotland. Both the Welsh and Spanish mountains contained goldmines, and such mines ranked very high on the list of desirable attributes of prospective provinces. They might offer a return on the investment of troops in their locality, and provide a considerable profit as well. Mineral resources were always an important consideration in Roman aggrandisement, as shown, for example, by the extraction of lead from the Mendips as early as AD 49.⁴⁴ But Scotland had no such resources, as far as they knew. Secondly, unlike Scotland, neither Wales nor NW Spain could be easily cut off from the pacified areas by a limes - indeed, a limes cutting off Wales would have to run for more than 100 miles. Scotland was far greater than Wales in area, but it could be, and for some time was, cut off from the province by a limes only forty miles long. We may conclude that the government in Rome is very unlikely seriously to have considered occupation and garrisoning of the Highlands.

The Aftermath

We turn lastly to the results of the campaigns. Whatever concessions may have been extracted from either Scottish tribe by Severus in 210, the terms of the agreements cannot have lasted, since in 211 both tribes were in revolt, and Severus was planning to lead a further expedition, when he died in February. Thus he was now back to square one. But actually the situation was worse than that: at no point previously during the campaigns had both tribes been actively

hostile. From 208 to the summer of 210 the Maeatae stayed quiescent; the Caledonians seemed to have been conquered before the Maeatae revolted later in 210. Such was the Emperor's exasperation at this setback when everything seemed settled that he lashed out in genocide. And it was this which brought the Caledonians into revolt as well. Naturally, after all this it would be an intolerable loss of face for Severus to withdraw, even though three years of campaigning had now in practice merely worsened the situation. However, it is hardly surprising that once his father was dead, Caracalla gave up all the grandiose schemes of conquest, and decided to withdraw all major forces to Hadrian's Wall, however costly that line was to maintain. (Supposed evidence for a campaign in 211 has been refuted, and the dating of the Carpow building-inscription to 212 is quite unconvincing.⁴⁵) His new frontier policy involved patrolling north of the Wall and apparently a check on meetings of the Scottish tribes. But in the end, Severus' attempt to solve the Scottish problem once and for all was forgotten.⁴⁶ The northernmost frontier of Empire stayed at Hadrian's Wall until the end of the Roman occupation.⁴⁷

FOOTNOTES

1. The principal references may be found in chapter xvi of A.R. Birley, Septimius Severus (1971), the most recent detailed discussion of the campaigns. I shall largely confine my refs. to those not already in Birley. The other major recent discussion is in S.S. Frere, Britannia, A History of Roman Britain (1974), 194 ff.
2. H. Mattingly, BMC v (1950), pp. 269, 353 and cf. clxxiv.
3. BMC iv, p. 624; cf. C. Oman, Num. Chron. 5 xi (1931), 137 ff. The bridge is also depicted on the Column of Marcus: C. Caprino et al., La Colonna di Marco Aurelio (Rome 1955), Figs 9 and 10.
4. BMC iii, pp. ci, 178-9. Detailed description in P.L. Strack, Untersuchungen zur Röm. Reichspr. des Zweiten Jahrhunderts I (Stuttgart 1931), 127 ff.; cf. C. Cichorius, Die Reliefs der Traianssäule (1896-1900), Taf. lxxii, Bild xcix.
5. e.g. by C.R. Whittaker, Herodian Loeb Vo. I (1969), 359, though one's confidence in his judgement on this is somewhat shaken by his later remark (p. 361) that "Severus perhaps crossed into Fife by sea, and from here advanced up the West (sic!) coast to the Tay base."
6. J.K. St. Joseph, JRS 59 (1969), 118; Birley o.c. 258.
7. St. Joseph, JRS 63 (1973), 218.

8. Herodian iii,14,5. For this interpretation, see my detailed arguments in "Drusus and the Classis Britannica": Historia 24 (1975), 315 f., and for a survey of excavated examples, R.v. Uslar, Gymnasium 78 (1971), 213 ff.
9. Caesar BG vi,9; quotation from Florus i,45,15.
10. R.F. Paget, Vergilius 17 (1971), 33, 37; cf. Dio lix,17; Suet. Calig. 19.
11. Pliny, NH v,86.
12. Cf. A. Grenier, Manuel d'Archéologie Gallo-Romaine (Paris 1934), ii,2, 495 ff. and G. Becatti, Scavi di Ostia (1961), Pl. 184. The mosaic is partly restored: cf. Calza, Not. Scav. xi (1914), 286.
13. Dio lxxi, from Suidas s.v. ἑσπύρα. Another account, perhaps from firsthand experience, in Arrian Anab. v,7. The Anonymus Byzantinus, περὶ στρατηγικῆς c. 19 gives a general account of such bridges; cf. also Tac. Hist. ii,34. For discussion of their representation on Trajan's Column, cf. Richmond, PBSR xiii (1935), 5-6. But I cannot credit his 'pontoons lying between each pair of boats' -- these are surely small cabins on the boats, visible on single boats elsewhere on the Column.
14. Oman's figure is the total length of the railway bridge and its approaches.
15. I.A. Richmond, Roman Britain (1955), 58.
16. For Auchtermuchty, cf. St. Joseph, JRS lv (1965), 82. Auchterderran: G. Macdonald, Archaeologia 68 (1917), 169 f. and O.G.S. Crawford, Roman Scotland (1949), 146.

17. For this interpretation, see the evidence of A. Robertson, PSAS ciii (1970), 134 n.10, which militates against the traditional view (repeated in her text) that these hoards were hidden by natives. One can note that two of the other three such hoards were also found near the known line of march. Portmoak: cf. G. Macdonald, PSAS lli (1917), 264 f.
18. Severus had already been constructing roads through marshes elsewhere, in this case close to the sea: cf. P-W Sudd. xiii (1973), 1487, and especially CIL x, 6811.
19. However, parts would be of normal construction. I suggest that a well-preserved stretch, later used in medieval times, survives running from Easter Lumbennie (NO 238163) past Macduff's Cross straight to Carpow.
20. While the Augusti were in the north, Geta Caesar was issuing coins depicting two types of Minerva (BMC v, clxi). Conceivably he spent some time at Bath.
21. Cf. Birley 247 and his references in n.1. In the last ten years, the only dissentient has been Professor Frere, o.c. (note 1), 188.
22. St. Joseph, JRS lix (1969), 105 ff. and JRS 63 (1973), 216 ff. - the latter with important additions and alternative interpretations.
23. Note also that the western line of 63-acre camps (close to the line of the 130-acre ones) is more closely spaced, as is natural if it indicates the first campaign through hostile territory.

24. cf. Richmond in Richmond-Ogilvie, De Vita Agricola (1967), 62.
25. cf. R.E. Smith, Historia 21 (1972), 488 and n. 41.
26. cf. Smith ibid. Unknown to him, there is direct archaeological evidence of this legion's presence: a standard depicting a centaur found at Spennithorne in Yorkshire (cf. M.V. Taylor, Antiq. Jnl. 24 (1944), 24-5). Its findspot suggests it was in the ditch of a marching-camp.
27. o.c. 121, from an idea of Richmond, CW xxxiv (1934), 50 ff.
28. Only one legionary detachment from abroad is specifically attested for this campaign: CIL xiii, 3496. There may have been a few others, or there may not. The only other evidence for the size of the expeditionary force is Dio's statement (lxxvi,13,2) that Severus lost a full 50,000 men in Britain. The reliability of this total can be judged on reflecting that it would be the equivalent of more than eight legions. For similar wild overestimates of army or casualty totals in Dio, cf. Townend, Hermes 92 (1964), 479 f.
29. CIL vi, 1643. The wording seems to imply -- and I see no reason to doubt -- that virtually the entire fleets of these four countries were being used. No difficulty in obtaining enough ships for the boat-bridge, either.

30. Another possibility is that it represents a body of auxiliaries who were sent to guard the Antonine Wall (cf. St. Joseph, JRS lxxiii (1973), 231). This is less likely, in particular because they would surely have been left to guard the Wall during the previous winter, and thus would not have formed part of the force sent out in the spring. But it is remarkable that not one auxiliary detachment is specifically attested as taking part in these campaigns -- though several have been suggested as possibilities.
31. Dio lxxvi, 13, 2. He also makes it clear that Roman army losses were alarmingly high.
32. Vita Severi 22, 4. Note that depriving rebels of supplies -- normally by adopting a scorched earth policy -- is almost the only means of attempting to enforce surrender on guerilla forces. (It was tried most recently by the Americans against the Vietcong.)
33. It may not be wholly coincidental that such a reason has normally been advanced by Scottish prehistorians. The alternative I suggest is, admittedly, the view of an English Romanist.
34. Cod. Just. 3, 32, 1: cf. Birley 267.
35. Cf. Jarrett and Mann, BJ 170 (1970), 199.
36. One may compare the ADVENTVS AVGVSTI coin of Severus of 210, which can only refer to his return to the base at York (where he died): BMC v, clxxiv.
37. BMC v ibid. and cf. Richmond o.c. (n15) 57 ff.

38. H. Cohen, Médailles Impériales (Leipzig 1880-92) iv,3;
F. Gneecchi, I Medaglioni Romani (Milan 1912) ii,73.
39. The only reason ever given for suspecting it is that of Cohen: while admitting that Severus would have come to Britain via Gaul, he said that this journey "ne dû être qu'en 963 (AD210) ou au plus tôt en 962." On the contrary, we now know that he was in Britain by 208 at latest, and thus may well have been in Gaul in 207. Gneecchi merely followed Cohen in his doubts, and Birley, Gneecchi; neither had independent reasons for doubt.
40. Cod. Just. 8,25,2; 2,11,9; 3,28,4. For other rescripts from 208-210, see Birley's list, p. 267. All rescripts from 209 and 210 were issued by the joint Augusti, except one from 13th January 209. That one was a judgement delivered by Severus in persona, but Caracalla may well have been present.
41. As tentatively suggested by Mattingly ibid.
42. Frere o.c. 201.
43. E. Birley, PSAS lxx (1936), 363 ff.; reprinted in Roman Britain and the Roman Army (1953), 87 ff.
44. Cf. CIL vii, 1201.
45. cf. Jarrett and Mann, JRS 57 (1967), 64, followed by Birley 270. Admittedly, the former do not answer Wright's argument that there is no room for two Emperors' names on his reconstruction of the Carpow inscription; Wright repeats this view in Britannia

v (1974), 289 ff. However, his own reconstruction of Caracalla's name takes less than 1½ of the four lines, and it is quite possible to restore all three Emperors' names on the inscription, the first line and a bit reading, "IMP E T D N L SEPT SEVERVS / PIVS E T ...". His remaining arguments merely show that the archaeological evidence is compatible with a brief occupation after 211. It is equally compatible with no occupation after 211.

46. Later centuries remembered two facts about his activities in Britain: a) that he 'built' (i.e. rebuilt) Hadrian's Wall, and b) that he campaigned in Scotland. It was natural (though incorrect) for them to assume that b) came before a) and was a preparation for it. Gillam and Mann (Arch. Ael. 48 (1970), 44) find needless difficulties over this matter.
47. I am grateful to several people, in particular Professor S.S. Frere, for criticism of an earlier version of this Section, which has now been published in shortened form in PSAS CVII (1978), 92-102. The coin of Caracalla which I refer to (p.68) as belonging to either 208 or 209, has now been firmly dated to 209: cf. A.S. Robertson, 'The Bridges on Severan Coins' in Roman Frontier Studies 1979 (ed. W.S. Hanson and L.J.F. Keppie), 131-9.

APPENDIX: ROUND AND ROUND THE ISLAND

We mentioned above that at the end of his campaigns, the Emperor Septimius Severus ordered the fleet to sail round Britain. According to Cassius Dio, (39,50), he thus demonstrated that Britain was an island.

More than a century earlier, after fighting the battle of Mons Graupius, General Agricola ordered the fleet to sail round Britain. According to Tacitus, 'tunc primum Romana classis circumvecta insulam esse Britanniam adfirmavit.' (Agr. 10)

I have attempted to show elsewhere (Latomus 32 (1973), 771-2) that Livy, writing in about 5 BC, stated that 'at one time' no-one had sailed round Britain, but that Julius Caesar opened up the country for exploration, and since then it has become better known. The implication of the words of Jordanes, who records these remarks of Livy, must be that Livy implied that at some time between the invasions of Caesar, and the time when Livy wrote, someone had sailed round Britain. It was most likely on a trading expedition.

In 1951 Mr C.E. Stevens (C.R. 1 (1951), 7-9) drew attention to the suspicious coincidence between the mention of a supposed 'conquest' of the Orkneys by Claudius, and detailed information about those islands in Pomponius Mela (III,54), writing just before Claudius' British triumph of 44. Stevens suggests that

some 'adventurous sea-captain' from Orkney put into Colchester in AD 43 and made submission to Rome in the name of his people. It would be far more plausible for Claudius to have ordered the circumnavigation of Britain by his fleet, and for this to have brought back the sort of information retailed by Mela, as well as the official submission from the chieftains encountered there.

What, then, was the purpose of all these circumnavigations? It might be said that no-one believed their predecessor's word on the matter, and kept on checking to see if they really were correct in claiming the insularity of Britain. But that cannot be said of Septimius Severus. He undoubtedly had a detailed account of Agricola's campaigns when he was campaigning in Scotland - it would have been invaluable in plotting strategy in Scotland, even a century later. And not only would Agricola have said that his fleet sailed round from the east, there was also the incident of the Usipi who sailed round from the west (Agr. 28).

Severus' circumnavigation, at least, must have had some other purpose. I suggest that all these circumnavigations, including that of Claudius, were a symbolic gesture to indicate that the rest of the island had been conquered. In Roman religion, the action of moving round something, whether on foot or not, was a way of indicating that one had some power over the thing surrounded. Sailing round Britain

could have indicated that the Roman state now had overall control of the island. In one respect this might be literally true. If one's fleet had control of the waters surrounding a country, one had, theoretically, the power to affect or stop trade with countries across the water - and thus a stranglehold over its supplies. This would not have been literally true in Britain's case, since it could no doubt be self-sufficient at least in food. But the symbolic gesture of circumnavigation must still have held significance.

Why, then, do later writers assume that the purpose of all these circumnavigations was to find out whether Britain was an island? The answer seems to lie in the fact that the question 'an Britannia insula' was a common set topic for debate in suasoriae in the oratorical schools (cf. G.W. Clarke, Cl. Ph. 63 (1968), 145-6). The establishment of proven facts would have had little effect on such set themes. After all, it was the rhetorical skill of the speaker in a suasoria which was being assessed, not the cogency of his arguments, let alone the conclusion reached.

The historians we have mentioned are bound to have tackled suasoriae as part of the normal oratorical education, and they may well have tackled this subject. If so, it is probable they assumed that Britain's insularity was still doubtful, so whatever circumnavigation they were later writing about

would have been, they thought, to ascertain the truth.

PATTERN AND PURPOSE IN THE
ANTONINE ITINERARY

The Antonine Itinerary (A.I.) is one of a group of twelve documents surviving from late antiquity, another of which is the Notitia Dignitatum.¹ The text from which our editions are derived is preserved in at least five primary manuscripts. These are generally agreed all to derive from the 'Codex Spirensis', which was copied between 1427 and 1551, and then disappeared.² Its fate was revealed when a bi-folium of the Codex (containing part of the A.I.) was discovered in the binding of a sixteenth century book from Speyer. As suspected, once the Spirensis had been copied, the original was regarded as a useful source of scrap paper, and more of it is only likely to be recovered if further sixteenth century manuscripts are dismantled.

It has recently been discovered that a 15th century manuscript of the Notitia corpus was apparently brought to England after 1715. Several pages of the Notitia were discovered in the 19th century hanging in cottages in the village of Walsingham in Norfolk, and now survive through meticulous paintings made by their discoverer. They came from a manuscript which may have been in the library of Holkham Hall nearby.³ Five other pages, apparently from the same manuscript, survive in the original in the Fitzwilliam Museum, Cambridge. Maier suggests that this manuscript was copied from a different codex at Speyer, which was not the Codex Spirensis. While that is possible,

it is most implausible that two quite independent manuscripts of the Notitia corpus should both happen to survive at Speyer when no others survived elsewhere. It is more likely that one was copied from the other, and in that case the discovery is not important for our reconstruction of the text.

As to the date of the A.I., it has for long been accepted that it was put together some time in the third century. However, several of the routes are earlier, and the earliest is the maritime route from Rome to Arles (497,9 - 508,2), which was compiled before AD 107.⁴ Kubitschek, following Mommsen, dated the final compilation of the A.I. to the eighties of the third century.⁵ This is supported by the inclusion of names which did not come into existence until 286. They are the following:

330,6	Dioclitianopolis	Not before 286
330,3	Heraclea	Perinthus renamed Heraclea sometime after 275. Not attested before 286.
225,2	Legio I Iovia	} These legions given these titles not before 285.
226,1	Legio II Herculea	

On the other hand, if the A.I. were appreciably later than 286, we should expect to find other changes as follows:

Legio II Flavia Constantia	} These were founded in 296, but do not occur in the A.I.
Legio III Diocletiana	
Cirta (occurs 7 times)	Renamed Constantina after 310
Byzantium (occurs 6 times)	Renamed Constantinopolis in 324
Antaradus	Renamed Constantia after 346

One should add that both Maximianopolis (321,5) and Constantinopolis (323,8) do occur once in an itinerary. However, these names are only found in the late manuscripts 3LB, and must be glosses.

Unfortunately, the absence of the legions created in 296 does not give a firm terminus ante quem. These two were stationed in Cusas and Andro respectively, but these places only occur once in the A.I. (155,3; 157,5) in a route of accepted Caracallan date (see below). So the legions' absence elsewhere in the A.I. might be due to chance. Our latest possible date for the compilation, then, is 310, but since nothing points to a date later than 286, we should do best to assume a date soon after this.

* * *

'The purpose, origins and date of the Antonine Itinerary have been much debated, and it is easiest to begin by saying what it is not.' Thus Professor A.L.F. Rivet in his recent important re-examination of the British section of the A.I., which he prefaces with a useful survey of earlier interpretations of the Itinerary as a whole.⁶ After discarding a former view that it may represent the routes of the Cursus Publicus, i.e. those routes to be taken by the holder of a diploma, he supports the view of Dr. Denis van Berchem, that the A.I. is a collection of journeys made by or planned for emperors or their armies.⁷ This theory was initiated by van Berchem's convincing identification of the most prominent route (123,8-162,4), which leads from Rome via Milan to Egypt, as that

followed for the most part by Caracalla on his journey to the east in 214-5.⁸ Where he failed to follow this route between Viminacium (in Moesia) and Nicomedia, van Berchem suggested that he followed another route per ripam a Viminacio Nicomediam also found in the A.I. (217,5-231,3).⁹ Guided by these identifications, van Berchem went on to suggest two other routes as those followed by individual emperors (Severus Alexander and Valerian) on particular occasions. But apart from one or two other observations made by van Berchem in the same article of 1937, studies of the A.I. as a whole have not really advanced since that date. For example, Rivet's brilliant analysis of the individual British itineraria is very useful in identifying archaeological sites. But as far as the A.I. as a whole is concerned, it merely confirms that some journeys are of a different date from others, and appears to confirm that their arrangement is as chaotic as is normally supposed.¹⁰

There are, however, two dangers in leaving the matter at this point, depending on our approach. We may adopt the cautious view of Rivet, that we should hesitate to apply even a terminus post or ante quem to a particular journey unless we have firm archaeological evidence of the date of occupation of a site on the route.¹¹ But for this to be useful we should need firm evidence of non-occupation at particular times in addition -- and proving a negative archaeologically is virtually impossible. (There may always be an undiscovered site with the same ancient name somewhere

nearby.¹²⁾ And even if it proved possible to give one route a firm date from archaeology, this would be of little use in dating others. To wait for further archaeological evidence, then, is virtually a policy of despair.

The more optimistic, but at the same time more dangerous view is to take our cue from the title of the work:

'Itinerarium Provinciarum Antonini Augusti'. Van Berchem took the Antoninus Augustus as Caracalla, and conjectured that when it was necessary for the whole collection of routes to be given a title, 'on a repris celui de l'édit de Caracalla, le plus ancien et le plus fameux, et l'on a baptisé le tout: Itineraire de l'empereur Antonin.' Thus, if anything, we should expect the majority of routes to be later than Caracalla. More recently however, Professor N.G.L. Hammond, taking his cue from the title, seems to assume without argument that the routes in the A.I. may be taken as Caracallan unless there is decisive evidence to the contrary. Thus, when considering the two versions given of the Via Egnatia, one of which (329,1-332,9) can be dated to shortly after 286,¹³ he assumes that the other (317,3-323,8), clearly earlier, should be dated to 'between 211 and 217'.¹⁴ The danger in this approach is evident when we realise that this is precisely the route identified by van Berchem as followed by, and thus planned for, Severus Alexander instead, and so not earlier than 222.¹⁵

So far no detailed analysis of the A.I. has been tried, in the way that it has for the Peutinger Map.¹⁶ A credible

dating of the routes in the A.I. would be useful in itself, but a credible interpretation might have wider implications, since it is one of a group of documents surviving from late antiquity, another of which is the *Notitia Dignitatum*. If we were able to draw some conclusions about the present or former state or status of the A.I. this could have implications for our views about the *Notitia*, on which much ink has been spilt.

The analysis below attempts to isolate features peculiar to certain routes, and patterns in groups of routes. The interpretation offered of these features and patterns is bound to be controversial and is offered more as a stimulus to thought than with a claim to probability. The analysis is divided into six parts:

- 1) An identification of further imperial routes.
- 2) An alternative method for dating routes.
- 3) An exposition of patterns of routes in certain provinces.
- 4) An interpretation of these patterns.
- 5) A possible date.
- 6) Conclusion.

1. Some Imperial Routes and their dates

The route from Rome to Egypt is unusual not just in its length, but in that at the beginning it lists major stopping-places en route. Van Berchem identifies these as stativae: places at which the army would stay longer than one day. If this is so, we may then look elsewhere in the A.I. to find other routes also headed by a list of the principal stations: which we may assume are also stativae. Such routes are the following:

- 2,2-22,5 Mercurii - Carthage
- 24,6-31,5 Carthage - Caesarea
- 57,7-70,1 Carthage - Alexandria
- 123,8-162,4 Rome - Hiera Sicaminos
- 231,8-240,5 Sirmium - Treveri
- 241,1-256,1 Taurinum - Vetera
- 356,1-363,2 Mediolanum - Gesoriacum
- 396,1-402,5 Arelate - Castulo

Given that one route is that followed by Caracalla, we might expect that these other routes were also planned for him, complete with stativae. That does not mean that he actually took these routes: as we have seen, on his eastern route he diverted from it for part of its length, and indeed, he certainly never made a journey from Mercurii in Mauretania to Carthage as Emperor. But there is apparent support for the view that the journeys were planned as possible imperial routes. It comes in the maritime itineraries, which follow

the land itinerary section and are also attributed to the Emperor Antoninus Augustus. After listing various places near Africa to which 'you' - presumably the emperor - could sail, the last being Carthage, the author adds, 'si autem non Carthagine sed superius ad Libiam versus volueris adplicare, debes venire de Sicilia ab insula Maritima in promunturium Mercuri' (493,4-7): a clear indication that this is an alternative offered. Another indication that they are being offered to the Emperor is the remarkable statement in the middle of a route from Emona to Sirmium (259,11-261,3), 'sed mansio Augusti in praetorio est'. This suggests an exceptional situation, and presumably the emperor was normally accommodated in a specially commandeered house or a mansio.¹⁷ At the same time the statement does indicate that this too was a journey planned for an emperor.

Van Berchem suggested that the route described between Viminacium and Nicomedia (217,5-231,3) was that taken by Caracalla in 214 when 'ad orientem profectionem parans omisso itinere in Dacia resedit' (Vita 5,4). The fact that he did this omisso itinere might itself make us wonder whether his ad hoc route would survive in our document. However, the fact that at Troesmis and Noviodunum the legions are called Iovia and Herculia shows that this route cannot be earlier than 285.¹⁸ Given this, and also that our terminus ante quem for the whole A.I. is shortly after 286, the route may be closely dated. Perhaps even closer. Ritterling

thought these legionary titles were bestowed very early in Diocletian's reign, 'wohl noch in 285, als sich Diocletian an der unteren Donau aufhielt.' Diocletian's movements in 285 are little known.¹⁹ He defeated Carinus at the battle of Margus, near Viminacium; later in the year he took the title Germanicus Maximus which implies that he fought against the Germans, and by the 20th of January 286 we find him in Nicomedia. Given the legionary titles in this A.I. route, it is plausible to suppose it is the record of the route to Nicomedia taken by Diocletian at the end of 285.²⁰

Adding this to the three other routes identified as imperial by van Berchem, we arrive at a total of eleven which can be plausibly ascribed to emperors.

2. Dating of Routes: An Alternative Method

Another method of dating routes was suggested by van Berchem. 'Si nous trouvons en Gaule les distances comptées tantôt en lieues, tantôt en milles, ces divergences peuvent tenir à la différence de date des itinéraires qui traversent ce pays.' Unfortunately we cannot make this clear distinction in the A.I., since there are no examples of routes running through Gaul which give the distances in miles only. Indeed, it would be surprising if there were any: it has now been demonstrated for two provinces that the land routes of the A.I. were drawn up directly or indirectly from the milestone survey of the province.²¹ We should expect this for other provinces, and in Gaul leagues were used consistently on 'mile'-stones from the time of Septimius Severus onwards.²² What we do have in the A.I. is a large number of routes -- indeed, all the Gallic ones from 374,2 onwards -- which state they are giving milia passuum when in fact they are giving leugae. The reason for this error can be seen where it first occurs. In 238,1 the route arrives 'Ad Fines m.p.XX.' The last part is correctly summarised at 232,3 as 'Treveros leugas, non m.p., CCXXI'. In the detailed itinerary for this part the first station (at 238,2) appears in the nonsensical form 'Vindonissa leugas m.p. XXX', and the remaining stations have the incorrect prefix m.p.. Apparently the scribe's source had a heading leugas intended to apply to the rest, but the scribe, not understanding this, merely continued

mechanically to write m.p. in front of each figure he met.²³ Now, two of the Gallic routes do correctly state that the distance given is in leagues: Durocorturo to Treveri (365,7) and Treveri to Agrippina (372,3). These routes are also different in that their intervening stations are all called vici, and the second refers to Agrippina civitas, while in four other routes it appears as a colonia. Presumably these two are of a different date from the other Gallic routes. Two others are also distinctive in listing both miles and leagues when crossing part of Gaul (241,1 and 356,1). These are two of the seven we have already associated through their listing of stativae -- and none of the other five pass through Gaul. So this does help to confirm the link drawn between the seven, and if they were imperial, no doubt the emperor wanted to know how many miles they would be travelling in Gaul as elsewhere.

3. Patterns in the Provinces

Are we then to suppose that all the routes in the A.I. represent imperial routes? According to van Berchem, 'Si l'on admet qu'au troisième siècle les transports de troupes donnaient régulièrement lieu à des édits semblables, que le même mode était employé chaque fois qu'une vexillation partait pour l'armée ou regagnait son camp, on imagine la quantité d'itinéraires qui durent être élaborés dans les bureaux militaires. L'Itinéraire Antonin fut composé avec ces matériaux-là.' Certainly, one of the instances he quotes is hardly compatible with imperial journeys alone. "Au cours de la guerre contre les Parthes, Edessa servit de base à Caracalla. L'armée y passa le dernier hiver de son règne. Or, six routes de l'Itinéraire aboutissent à Edessa.' We can hardly imagine the Emperor taking, or even choosing between, these six different routes, and later van Berchem suggests the A.I. was put together based on 'les édits sur l'annone des armées.' We must consider later whether the bulk of routes do represent troop movements or are more directly connected with the annona. For the moment, the important factor to note is that the six routes all lead to Edessa, and one can reasonably deduce that something is moving to the place, presumably when the Emperor is there.

Let us turn to another area to see whether some similar pattern may be detected. Once again, everything hinges on the direction which the routes take. The first

route in the A.I., running from Mercurii in Mauretania to Carthage, is similar to Caracalla's eastern route in containing stativae: in this case Tingi, Rusadder, Caesarea, Saldae, Rusiccade, Hippo Regius and Carthage. Likewise the third route (24,6-31,5) lists stativae and is probably another imperial route. Is it then possible that some of the following routes represent the movement of troops or supplies to the expeditionary force? Since the main route runs along the coast, we should expect the others to run from the interior to the coast. And if alternative routes are given from A to B, this might be to collect whatever it is from the intervening places lying on different roads. I therefore list the routes, and a note on whether they conform to this pattern.

23,1	Tocolosida - Tingi	Conforms
	Carthage - Caesarea	Does not conform (and contains <u>stativae</u>)
31,6	Sitifi - Saldae	Does conform
	Lambaesis - Sitifi	Both could join up with 31,6, and give alternative routes Lambaesis to Sitifi.
	Theveste - Lambaesis - Sitifi	
	Turres Caesaris - Cirta	Could join 42,4 to Hippo; cf. 40,6 and 41,3
35,2	Tamugadi - Lamasba	Lead to Sitifi; could then
	Lamasba - Sitifi	follow 31,6 to Saldae
	Calama - Rusuccurru	Conforms
	Rusuccurru - Saldae	Start at and rejoin main route (alternatives)
	Saldae - Igilgili	

40,6	Lambaesis - Cirta	Both lead to Cirta; could
41,3	Musti - Cirta	then follow 42,4 to Hippo
42,4	Cirta - Hippo Regius	Conforms
	Hippo Regius - Carthage	Both conform
"	"	(alternatives to the main route)

Thus, every one of these routes (apart from the second 'imperial' one) leads part or all of the way to the initial imperial route. Such a pattern surely cannot be just coincidental.

There follows a series of routes only a few of which fit the pattern, and that may be due to chance. But when we turn to another major route with stativae, this time from Carthage to Alexandria, we find the same pattern visible. The main route runs via Thenis and Leptis Magna to Alexandria (57,7-70,1). There follows an alternative route of the section from Ptolemais to Alexandria. Then another alternative, from Tacapae to Leptis Magna, this time with substantial detours - the most striking between Tacapae and Agma, as it travels via Turris Tamallenis, thus adding 157 miles to the main route's 25. Finally, a route from Thelepte which joins the main route at Tacapae. The routes in Africa, then, do largely fit into a pattern of major imperial routes, and minor ones which lead part or all of the way to them.

Another lengthy imperial route may perhaps be deduced from the Spanish section of the A.I. We have already noticed the route from Arelate to Castulo (396,1 - 402,5),

which is preceded by stativae. It is followed by two short routes also leading to Castulo, and thus apparently following the African pattern. Then come three routes: Castulo to Malaca, Malaca to Gades, and Gades to Corduba. This sequence suggests they are part of a longer route, continuing that from Arles. Only one route leads from Corduba: it goes to Emerita. Another five routes lead to Emerita, again on the African pattern. Three routes lead from Emerita, but all go to Caesaraugusta; likewise four other routes lead to Caesaraugusta. It seems possible, then, that we have here a fragmented imperial route leading from Arles via Castulo, Malaca, Gades, Corduba and Emerita to Caesaraugusta.

A slightly different pattern is best exemplified in the British section of the A.I.²⁴ First I give a list of these routes in summary form. Places are given their modern names to assist recognition of the supposed pattern, and the identification of places and routes follows Rivet.

- I High Rochester to York (Stretch from Malton to Brough erroneously incorporated)
- II Birrens to Richborough via York, Chester, Wroxeter, and London
- III London to Dover
- IV " " Lymne
- V " " Carlisle via Caistor-by-Norwich
- VI " " Lincoln
- VII Chichester to London via Bitterne and Winchester
- VIII York " "
- IX Caistor " "

- X Ravenglass to Whitchurch
- XI Caernarvon to Chester
- XII Carmarthen to Wroxeter
- XIII Caerleon to Silchester
- XIV " " " (alternative route)
- XV Silchester to Exeter

A striking point about these routes is the high number (twelve out of fifteen) which lead from the north or west to the south or east. I would suggest that they are to represent the movement of something from the outlying parts of Britain to the continent. Whether that commodity was animal, vegetable or mineral will be considered later: for the moment I give it the neutral term x, while attempting to substantiate this pattern in detail.

I start with Iter II rather than I, since II has been singled out as a 'Hauptlinie'. By virtue of its sinuous line it manages to include a large number of major towns in Britain. It starts at the northwesternmost outpost of empire, at Birrens (Blatobulgium: 467,1) and ends not at London but on the coast. Eminently suited, then, as the principal route which x is to take. Iter I, then, might be a kind of 'feeder' route; this time x would travel from the most northeasterly British fort to York, and link up with II to follow this down to the coast.

III and IV could represent alternative routes to be taken by x to the continent.

V and VI are two of the three routes which lead in a

direction contrary to the general pattern. However, there are two facts which tend to confirm that they are exceptional: first, they are the only British routes largely duplicated by routes which do fit into the pattern, and second, we can tell from nomenclature that Iter V, at least, is of a different date from Iter IX.²⁵ So for the moment we may leave V and VI on one side.

Routes VII, VIII and IX end at London and make substantial detours to include important places. VII in particular pointedly ignores the direct route from Chichester to London along Stane Street, but winds to include Winchester and Silchester (Venta Belgarum and Calleva Atrebatum: 478,2-3). I shall suggest later that VII is another 'Hauptlinie'.

The purpose of Iter X has remained a puzzle even now that its course has been satisfactorily identified. After all, who would want to lead an army from Ravenglass to Whitchurch (a Clanoventa Mediolano: 481,1)? But let us take it with XI and XII. All three routes lead from outlying parts of Britain and reach towns which appear on the Hauptlinie Iter II. Indeed, two of these places, Chester and Wroxeter (Deva and Uriconio: 469,2 & 6), Iter II made an especially large detour in order to include. Thus all three routes could represent the path of x across Britain from outlying parts to Iter II and then following II to London and the coast. This would explain both the purpose of these routes and the reason for their being

grouped together in the A.I.

XIII and XIV also fit into the pattern: they lead from Caerleon eastwards to Silchester (ab Isca Calleva: 484,1; 485,8), where they could link up similarly with VII to travel to London. The reason for two routes is clear: one includes Gloucester (Clevo: 485,4) and Cirencester (Durocornovio: 485,5); the other includes Caerwent (Venta Silurum: 485,9) and Bath (Aquis Sulis: 486,3). It would be quite impossible to devise a single route which would include all four of these important places.

Iter XV ought to be another feeder for VII, both because it is put with XIII and XIV and because it too has Silchester at one end (Calleva: 486,8). And a route from Exeter (Isca Dumnoniorum: 486,17) to Silchester would complete the network of routes the rest of which systematically cover all other parts of Britain. So we need a route from Exeter to Silchester here. Unfortunately Silchester is at the wrong end of XV: the start instead of the destination. However, there is something else strange about XV: it is repeated (or rather, is first found) in Iter XII, into which it has been introduced by mistake. This confusion might have been caused by the presence of Moridunum in both routes (as Rivet cautiously suggests). Further confusion between XII and XV is evidenced by XII's title 'A Muriduno Viroconiorum' -- the ending from XV's 'Dumnoniorum' -- and in XII's total CLXXXVI (15 miles in excess), which may have been influenced by XV's total CXXXVI.²⁶ But Rivet's

caution on the reasons for this confusion is well-advised: such confusion is only likely to have arisen if our scribe had been confronted with two routes both beginning with Moridunum. XII does; XV does not. We could explain the confusion if there were some reason for supposing that XV could have been reversed: in that case it too would have started with Moridunum.

A valuable clue seems to be afforded by two routes elsewhere in the A.I. At 97,7 is a maritime route from Hyccara to Drepanis, but the mileages given next to each place would only be correct if the stations were in reverse order. This mistake could not have arisen had the scribe merely repeated or omitted one of the mileage numbers, so that the rest shift up or down (as by P1L at 18,3-20,3). As Cuntz realised, at 97,7 the scribe has reversed the order of the route in front of him, but failed to adjust the mileages accordingly. The same applies to the brief route at 256,2.²⁷ Yet it is clear that neither of these reversed routes (pace Cuntz) was derived from others which survive elsewhere in the A.I. The first one contains a name for one stage, Aquae Perticianenses (97,10), which differs from that found in the route from which it might otherwise have been derived (90,6-93,1). The other reversed route (256,2-3) correctly gives its distances in leugae, while the only route from which it might have originated (368,3-372,2) attributes the incorrect designation m.p. to its distances. We must thus picture our scribe confronted with

a large number of routes, selecting some but omitting others which confronted him, and adding others in reverse order. We have further evidence for this process of selection in Route I of the British section. Rivet shows that the most plausible explanation of this Iter as now preserved is that it represents "the conflation of two itineraria, one running from High Rochester to the praetorium at York, the other from Malton to Brough.' But he does not explain how this conflation arose, and is obliged to assume that 'the corruption of Petuaris to Praetorio would have been induced only after the conflation had taken place.' Rather, we may suppose that the similarity of names caused the confusion: the scribe was confronted with two routes, ending with similar names, and his eyes slipped from one to the other, leading to its incorporation.²⁸ Once again this suggests that the scribe chose to copy a selection of the routes in front of him, and it is only by a fluke that this 'superfluous' route was incorporated. If we had the source from which our selector was copying we might indeed have the general road-map of the Empire which the A.I. has sometimes been claimed to be. Instead, these routes are selected, and the principle of selection will be considered below.

It is not impossible that the man who was intelligent enough to select or reverse certain routes was also stupid enough to write 'Vindonissa leugas m.p. XXX' (238,2). It would be far more plausible to imagine an official going

through the listed itineras, marking those which were to be copied, and which to be copied and reversed. The menial clerk would put down the heading, and then mechanically copy out all the intervening stages, reversing them when so instructed. On at least two occasions he forgot to adjust the mileages when reversing, and he also failed to realise that 'leugas non m.p.' (232,3) meant that he should omit the usual m.p. designation. This hypothesis of reversal could also explain the chaotic case-endings of placenames in the A.I., and incorrect case-endings might help to identify which of the routes have been reversed.

Let us return to Iter XII, and assume that our copyist was confronted with two itineras, one 'a Viroconio-Moriduno', the other 'ab Isca Dumnoniorum-Calleva'. His instructions are to copy the first in reverse order, and the second straight. Having read the first title and noted that the route should start with Moridunum, his eye slips to the route which does indeed start with Moridunum (XV). He completes the title wrongly -- adding the ending of 'Dumnoniorum' and its mileage, and then obeys his instructions by reversing the route. But it is Iter XV he has reversed, and realising his mistake at the end he then correctly copies and reverses Iter XII. By the time he comes to XV his concentration is going (whose would not be, after copying out the whole of the A.I.!) and he remembers simply that he must recopy what he has already copied -- and fails to notice that XV should not have been reversed. Such a

theory, though complicated, does provide an explanation of this apparent mistake which takes into account characteristics of the A.I. visible elsewhere.

Whether or not it is thought plausible, elsewhere in Britain we are left with the interesting phenomenon of a network of roads leading from all outlying parts of Britain and joining up with others to lead through London to the coast. We can hardly suppose these twelve were for twelve different emperors all moving to the Continent, and while the alternative picture of one emperor wanting twelve different ways to get out of Britain has its attractions, it is little more plausible. The two exceptional routes, however, V and VI, might quite possibly represent imperial routes, and since one of these two is of a different date from a route which fits the pattern, V and VI have presumably been added to the scheme. They are not placed at the end, but we can see why. Excepting the first two, one of which is a Hauptlinie running through London, the other thirteen (adopting my emendation of XV) are neatly grouped: the next four routes run from London, the next three to London, and the next six are to join II and VII. Clearly V and VI, leading from London, have been tacked on to the others which do. As to these thirteen routes, they seem to fit the African pattern, of x moving in one direction, with the difference that the Hauptlinien here are probably not imperial, but the principal routes to be taken by x.

Another example of the pattern may be found nearer

Italy. The situation in Sardinia (78,4) may be summarised thus: two alternative routes from Portus Tibulas (in the north) to Caralis (in the south). One goes via Ulbia. Two other alternatives, from Tibulas to Ulbia per compendium, and from Ulbia to Caralis. From Tibulas to Sulcis, and from Sulcis to Nura. Lastly, from Caralis to Nura. Thus all the routes form a pattern leading from Tibulas to Nura, and as in Britain they manage to include almost all important towns in the island. In Corsica (85,4) only one route is given, but that too leads from north to south. Again, one might suggest that x is being moved from north to south.

As a final example Gaul may be ^{c/}sited. Here, as in Britain, we find a preponderance of routes leading from the north and west to the south and east: in this case from the coastal regions inland. No less than 21 of the 24 Gallic routes conform to this pattern.

4. An Interpretation of these Patterns

How then are we to interpret these groups of routes; do they represent troop movements or are they connected ~~with the annona~~ in some other way? Van Berchem imagines each route as a troop movement, with stops listed on the way where annona can be collected. But we can hardly suppose the A.I. to be simply a random collection of such routes: we should not have the preponderance leading from out-of-the-way places to a major route, in that case. If anything we should expect a majority to do the opposite, as suggested by a passage in the Life of Severus Alexander (c. 45), 'itinerum autem dies publice proponerentur, ita ut edictum penderet ante menses duos in quo scriptum esset: illa die, illa hora ab urbe sum exiturus, et si di voluerint, in prima mansione mansurus; deinde per ordinem mansiones, deinde stativae, deinde ubi annona esset accipienda; et id quidem eo usque quamdiu ad fines barbaricos veniretur.' Here, the list would lead from the starting-point to the enemy frontiers, and we should expect this for most other military journeys also.

Yet the substantial detours we have noticed do suggest that something is being collected from each of the stopping-points, and one might perhaps envisage soldiers who were to travel via major towns where they could pick up further vexillations before joining a big expedition. And if we look again at routes II to IV in Britain, it does appear that the section of II from London to Richborough with its full

list of halts is going to pick up x from each place, while those travelling along III and IV need not stop so often because x has already been collected by those travelling on II.²⁹ But it is hard to believe that extra troops existed at every one of the halts listed.

I therefore suggest that these routes represent the paths of the annona militaris when it is being moved to a major army force. Most of the routes in the A.I. have been selected in accordance with this design. Troops might sometimes be sent out to escort it back, and when we find an alternative route both starting and ending on an expeditionary one that may be the explanation. But if we find a route starting from some god-forsaken place in the deserts of Africa it is perhaps simpler to envisage a letter from the governor informing the inhabitants of the amount of annona they must provide, and the places along the route to which they must arrange its transportation.³⁰

The other major argument for connecting the A.I. with the annona is that of van Berchem: that the date of completion of the A.I., soon after 286, is close to the date when Diocletian chose to systematise or reorganise the collection of this tax. But there are two further pointers to our conclusion. Rickman notes the importance of the fourth century horrea at Veldidena near Innsbruck, and suggests it 'must have been a suitable base to be used by primipilares for frontier supply.'³¹ Veldidena appears in four routes in the A.I.; it is the destination of three of them.

Secondly, as Rivet noticed, 'While six routes are given in Sardinia and one even in Corsica, there is none in Crete or Cyprus -- in contrast to the Peutinger Table, which includes routes in Crete and Cyprus but not in Corsica and Sardinia.' The annona theory will explain this discrepancy: both Sardinia and the east coast of Corsica were sufficiently fertile to produce plenty of corn -- indeed, the former remained an important granary throughout antiquity -- while neither Cyprus nor Crete could be similarly relied upon: they doubtless made their contribution in some other way.

An objection might be made to our conclusion. In some cases the pattern detected might seem to indicate a general direction of route contrary to that which we should expect for the annona. For example, would not corn for the troops in Britain be sent to the fortresses, or forts on Hadrian's Wall? Apparently not. We could make this assumption in the case of the ordinary frumentum emptum which had been exacted for centuries. But the annona tax was originally imposed for particular military expeditions, and would have been sent to the expeditionary force, wherever it was: if on the continent, annona would have been sent there from Britain. And once the annona militaris became a regular additional tax, annona was taken wherever it was most needed by a large military establishment. In 356, the Caesar Julian built new granaries in Germany 'ubi condi possit annona, a Britanniis sueta transferri:' sueta showing

that this occurred to provision troops on the Rhine even when no expedition was in view.³²

Nevertheless, the pattern of routes in north-west Gaul suggests that the force for which Gallic grain was intended was south-east of there too, and it seems possible that the patterns we have detected might be related to a specific military situation in the southern half of the Empire. Further evidence comes from the maritime section. This is broadly divided into three parts: a list of various routes, the long pre-107 route, and a list of islands. Almost the whole of the first section, from 487,1 to 496,2, consists of routes from various parts of the north Mediterranean, including Achaëa, Italy and Spain, almost all leading to Africa. Whatever the significance of this, it remains a reasonable conclusion that, while many of the routes in the A.I. may represent journeys made or planned for emperors, the majority indicate the path taken by supplies of the annona militaris.

One important reservation should be made to this conclusion. Though many of the routes may reflect the annona, we can hardly suppose that the A.I. represents a complete empire-wide pattern of annona supplies, with the addition of some imperial routes. For example, Egypt does not figure among such routes -- it merely appears at the tail end of two imperial routes -- yet Egypt was the annona province par excellence. But it is possible that its rather special position in the imperial administration may

have led to its exclusion from the A.I.

An examination of the correlation between known annona routes and the A.I. might add support to our conclusion. Unfortunately there seem to be only two cases where we can identify specific annona routes of the third century, and one of those is ten years later than our proposed terminus ante quem. First, we have those suggested by the list of $\mu\acute{o}\nu\alpha\iota$ in the Panopolis papyri.³³ But these are in Egypt, where we have the imperial routes only. Secondly, inscriptions attest annona being sent from Side in Cilicia to Syria, under Severus Alexander and Gordian.³⁴ However, the whole of southern Asia is represented in the A.I. by only two routes, one of which is part of the major Caracallan route. The other leads from Callipolis to Laudicia (333,9 - 337,2): not a plausible route for the annona. (It might perhaps represent the route taken by Caracalla in 214, when he crossed the Hellespont and visited Ilium, Pergamum and other places in Asia Minor before returning to Nicomedia for the winter of 214-5.³⁵)

Thus, since we know at the moment of only two annona routes earlier than the fourth century, the fact that they happen not to coincide with A.I. routes cannot be regarded as statistically significant, and the attempt to find a correlation must be dismissed as inconclusive.

5. The Emperor Responsible

It has been suggested above that routes preceded by stativae are of Caracallan date, and another three date to before 222; seven other routes may be dated after 217. But we still have no date for the large number of routes which fit the annona pattern in several provinces. The only assumption we can make is that they all precede 286, this being the terminal date for the A.I. as a whole. If we had a firm date for the establishment of the annona militaris this might give us a probable date for the annona routes. Unfortunately we have no such date. In 1926 Rostovtzeff thought the regular system started with Diocletian;³⁶ in 1937 van Berchem suggested that the regular system was initiated by Septimius Severus and later reorganised by Diocletian. Several have since dissented ~~with~~ ^{from} the ascription to Severus, and have contented themselves with the view that requisitions for imperial journeys were only systematised 'at some time' in the third century.³⁷ Clearly, in view of these divergencies we cannot use the date of the annona to help date annona routes.

We have one further clue to dating. The British section contains at least twelve routes which conform to the pattern, and two 'imperial' routes (V and VI) of a different date. The difference in date between V and IX is shown by nomenclature: IX refers to Camolodunum and Venta Icenorum while V refers to the same places as Colonia and Icinus. The fact that in V the cantonal capital of the

Iceni is now known by its tribe³⁸ and Colchester is now called 'The Colony' -- a name fossilised in the modern Colchester -- shows that route V (and presumably VI) is later than IX, and apparently several decades later. Yet if none of the A.I. routes postdates 290, routes V and VI should be either Diocletianic or pre-Diocletianic. In that case route IX and the others associated with it in the annona pattern should date from substantially before the time of Diocletian. This should also imply the establishment of a regular annona tax before Diocletian. At the same time it would be implausible to ascribe the drawing-up of a pattern of annona routes to any emperor reigning after 235. Between that year and Diocletian's accession occurred fifty years of chaos, with emperors two a penny and hardly any lasting longer than a year. Had such an important system of taxation been established during this period it would inevitably been caught up and destroyed in the rush of events as emperors were made and unmade. 235, then, is our terminus ante quem for the establishment of the annona militaris.

There is general agreement that the regular annona system cannot be put back to Caracalla. Thus Brunt 'has pointed out that the ostraka from Dakka in Nubia in the reigns of Severus and Caracalla ... show not that there were supplementary payments in kind to troops as early as this, but quite the reverse, that even under Caracalla troops had deductions made from their pay for food.' Likewise Rickman points out that Dio's record of the

provincials' complaints reads oddly if there had been a regular annona militaris tax in operation throughout the empire already.³⁹ So if we accept these *termini post* and *ante quem*, the establishment of the annona militaris should be attributed to Severus Alexander.

But it is precisely to this Emperor that some kind of reorganisation of the annona is attributed by the Augustan History. The passage was quoted above in which Severus Alexander is said to have issued exact details of his journeys two months in advance, including the arrangements for their supplies. More significant is a later passage (c. 47): 'milites expeditionis tempore sic disposuit ut in mansionibus annonas acciperent, nec portarent cibaria decem et septem, ut solent, dierum nisi in barbarico.'⁴⁰ Rickman comments, 'The implication is that these were special arrangements expeditionis tempore and revealed the unusual care shown by Severus Alexander for his troops.' But what 'unusual care' was this? We are specifically told that the soldiers need no longer (except in enemy territory) carry around seventeen days' rations,⁴¹ but could rely on receiving their annona from the mansiones en route. The passage does not make it clear whether annona was now being received by all mansiones throughout the empire, or by those along an announced imperial route. But there is nothing to show that imperial expeditions are specifically meant here; on the contrary, they have already been mentioned in detail two chapters earlier, and if our author meant only that the soldiers

were supplied by the mansiones on an imperial expedition, we are entitled to ask in what respect their treatment ^{was} different from that on Caracalla's expedition. It seems certain that in some respect the use of the annona militaris increased during the reign of Severus Alexander. If we couple the Augustan's History's statement with the evidence of date in the A.I. it seems most likely that the regular system of the annona was introduced by him. It follows that if we are confronted by a route in the A.I. for which there is no internal evidence of date, it would be safest to attribute it to that Emperor.

6. Conclusion

To turn finally to the character of the Antonine Itinerary. We have suggested that part of it consists of a list of routes for the annona, a list which can surely only have been official. This part was itself collected from a larger list of routes, of which some were omitted and others reversed. Another part of the A.I. consists of more than ten routes which represent the journeys of emperors on particular occasions. Presumably the collection of imperial journeys was also official. Yet, when our document was drawn up some of the information in it (such as the pre-107 maritime route) was already out of date. Likewise, it was later revised by the inclusion of more up-to-date information, but this did not lead to the deletion of the antiquated portions. In all these respects our document is analogous to the Notitia Dignitatum. In the case of the Notitia, it has commonly been assumed that the document we have is one kept by a military historian who obtained one or several partly obsolete documents and tried, very erratically, to update them. The contradictions or muddles in it can then be attributed to the historian rather than the Roman Record Offices.

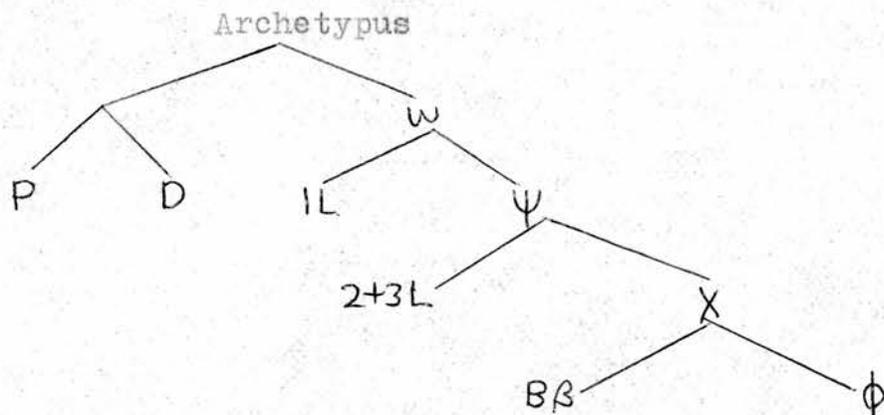
However, if we were to adopt the same view about the A.I. we should have to attribute almost the whole document to the inefficient or muddled updater, so many inconsistencies or confusions do we find. Indeed, we can find an error even in the archetypal route, that of Caracalla. In Egypt his

route runs to Alexandria (154,5) thus: 'Andro XII, Nitine XII, Hermupoli XXIIII, Chereu XXIIII, Alexandriam XX.' The route then retraces its steps in order to reach the Nile. It continues: '(Alexandriam XX) Chereu XXIIII, Hermupoli XX, Andro XXI.' We can only deduce that the compiler of this route, like the compiler of others, failed to understand or remember that mileages need to be shifted when a route is reversed. But this is an official imperial route, and we cannot blame this mistake on our supposed 'inefficient historian'; the route must be derived from an official list, and neither he nor any copyist would have introduced this mistake. Who we have to blame, in fact, is the minor copyist in the Records Department who was assigned the dogsbody's job of compiling this route. He seems to have reached the standard of accuracy and understanding which copy-typists nowadays are popularly supposed to attain.

Some years ago certain deductions were made about the Notitia, their main supporting argument being that they 'allow us to suppose that there were rational men in the Roman Record Offices'.⁴² But we should not forget that bureaucracies can often be inefficient,⁴³ and they also suffer from an understandable but not wholly pardonable unwillingness to deal effectively with inefficient members of staff, who tend to be transferred rather than sacked. In addition, it would have been not just time-consuming but incredibly tedious for a knowledgeable official to have gone

through the A.I. checking the correctness of its entries, and it is hardly surprising if such a check was not made. It may not be too paradoxical to suggest that the errors within it are themselves evidence of its official nature.

APPENDIX

The Manuscript Tradition of the A.I.

Above is the stemma of manuscripts drawn by Cuntz, following Kubitschek. It will be noted that manuscripts P, of the seventh century, and L, of the eighth, are thought to be independent. But the following 'coincidences' should be noticed.

- 18,3-20,3 Both P and LL repeat a mileage, and the following seven mileages are misplaced one line. According to Cuntz, this happened independently to P and LL 'miro quodam casu'.
- 92,5 P and LL omit an extra diverticulum.
- 95,5-96,3 Both P and LL omit descriptions (refugium or plaga) from the places listed on this maritime journey.
- 103,1 mari PLL; Thamari cett. (recte)
- 230,9 One stage omitted by PLL

The omission at 230,9 could be dismissed as coincidence. Cuntz suggests that at 95,5 f. the descriptions were included only as marginalia in the archetype, and both P and LL chose not to include them. Similar arguments could apply to 92,5. That is possible.

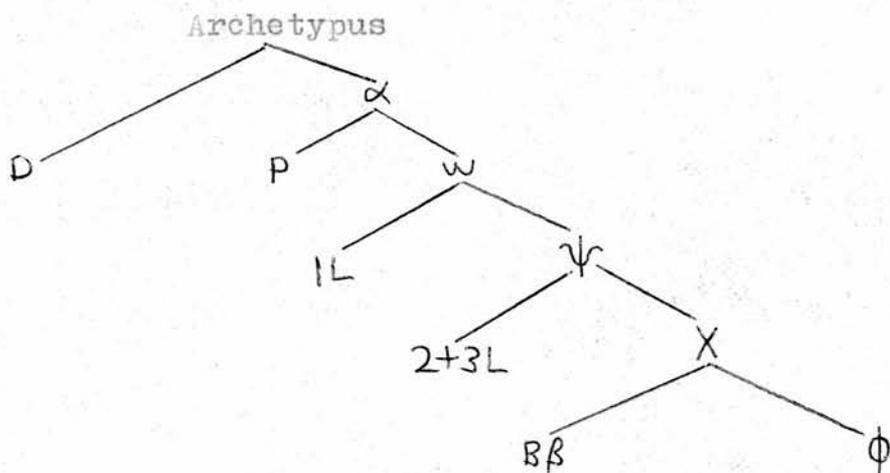
Cuntz also says that the transpositions in P and LL at 18,3-20,3 must be independent. He argues that the transposition in L does not go back further than ω , because L's reading XXV at 20,3 is the 'same' as ψ 's reading XXII (both in place of the correct reading XVIII of P). This argument is weak both because XXV and XXII are not the same, and because L's next three readings are all hopelessly and inexplicably wrong, so its reading at 20,3 may be equally inexplicable. No: one cannot believe these transpositions are independent; there must be either a common ancestor behind P and LL, or contamination from a common influence.

The same applies to 103,1, where it is impossible to believe that the scribes of P and LL independently happened to think of the sea and so both dropped Tha from the word. It is, however, equally hard to believe that a corrector would have deleted Tha from ancestors of P and LL, unless his own manuscript read simply mari. But in that case we must presuppose that mari had

early manuscript authority, which is what one would otherwise deduce on the far more likely hypothesis, that P and LL share a common ancestor not shared by D.

Readings of this common ancestor should carry an authority equal to that of D - perhaps more so, in that they predate the seventh century, while D is tenth century. It follows that the descriptions of the ports of call (refugio, plaga etc.) in 95,5 - 96,3 cannot simply be marginalia. They are more likely to be readings of the archetype, and can then be compared with the similar descriptions of ports in the maritime journey from Rome to Arles (498,1). Since, as we saw, this journey predates AD 107, 95,2 - 96,4 is hardly likely to be later than second century.

The presence of this common ancestor, which I call α , means that the stemma must be redrawn. The simplest way to redraw it is as follows, from which it emerges that if P and LL agree in a reading against D, D should be given almost equal authority to PLL combined.



FOOTNOTES

1. The most recent edition of the Itinerary is that of O. Cuntz, Itineraria Romana I (Leipzig 1929). For a useful summary description of the work, see A.L.F. Rivet, Britannia I (1970), 34 f.
2. For this Codex, cf. E.A. Thompson, A Roman Reformer and Inventor (Oxford 1952), 6 f., and I.G. Maier, Latomus 27 (1968), 96 f.
3. For further details, see J. Alexander in Aspects of the Notitia Dignitatum (British Archaeological Reports, Supp. Series 15), ed. R. Goodburn and P. Bartholemew (Oxford 1976), 11 f.
4. R. Lugand, Mélanges de l'Ecole Francaise à Rome 1926, 124-39.
5. Kubitschek, P-W s.v. Itinerarien 2337-8.
6. See n. 1 above.
7. Mem. Soc. Nat. des Antic. de France lxxx (1937), 166-81.
8. Van Berchem, ibid., and for more recent confirmatory evidence, R. Mouterde, CRAI 1952, 355, and van Berchem, CRAI 1973, 123-6.
9. However, it can be shown that this route is later than Caracalla in date: see below.
10. Studies have been made of individual routes in some other provinces, but with even less consideration of the wider problems. For a good bibliography, cf. R. Chevallier,

Roman Roads, trans. N. Field (1976), 232-3,
and for other refs, l'Année Philologique
s.v. 'Itineraria et Geographica'.

11. Thus, Rivet suggests that a date for the abandonment of Birrens could help date Iter V in Britain.
12. For example, archaeology recently suggested that Corbridge was founded twenty years too late to be Agricolan; almost immediately the Agricolan fort was found nearly a mile away: cf. Current Archaeology iv (1974), 325 f.
13. Cf. C. Edson, CP 46 (1951), 13 n. 22.
14. N.G.L. Hammond, A History of Macedonia I (1972), 19.
15. o.c. (n.7), 179.
16. A. and M. Levi, Itineraria Picta (Rome 1967).
17. Though where a palatium were available he would doubtless stay there: cf. Just. Cod. I,40,15,1.
18. For the date, cf. Ritterling in P-W s.v. legio 1351 f.
19. For the evidence, cf. P-W s.v. Valerius (Diocletianus) 2424-7.
20. The unknown places Suneata and Atubino, from which rescripts were issued on 2nd November, should thus lie close to this route.
21. For Britain, Rivet o.c. 64; for Phoenicia, R.G. Goodchild, Berytus ix (1949), 91-128.

22. Cf. A. Grenier, Manuel d'Archéologie Gallo-Romaine ii (Paris 1934), 95.
23. This should imply that his source did not include m.p. in front of each of its numbers, and most likely just gave simply figures, merely indicating at the start of a section where a route changed to leugae.
24. 463,3 - 486,17.
25. Nomenclature: Rivet o.c. 47, 65-6.
26. Cf. Cuntz, note on 483, 1-8.
27. The placing of leugas X next to Harenatio at 256,3 is most easily explained if the source iter mentioned an extra place between Carvone and Harenatio, situated ten leagues from Harenatio. Kubitschek (P-W s.v. 'Itinerarien' 2324 n. 17) did not recognise this reversal.
28. W. Rodwell (Britannia VI (1975), 84-5) somewhat misrepresents Rivet's view. He then suggests instead that Iter I includes a 'return to York' - something without any parallel in the whole of the A.I. This lack of parallel is not just "negative evidence": it is decisively against his interpretation.
29. Likewise, between High Cross (Venonae) and London, VIII omits two stages included by II, perhaps for a similar reason.
30. On the collection of the annona cf. M. Rostovtzeff, SEHRE (revised 1957), 485 and A.H.M. Jones, The Later Roman Empire

- (1964), 458 f., 626 f.
31. Cf. G. Rickman, Roman Granaries and Store Buildings (Cambridge 1971), 289.
 32. Amm. Marc. 18,2,3.
 33. T.C. Skeat, Papyri from Panopolis (Dublin 1964) = Chester Beatty Monographs 10.
 34. G.E. Bean and T.B. Mitford, Journeys in Rough Cilicia 1964-8 (Österr. Akad. der Wiss. Phil.-Hist. Kl. Denkschr. cii: Wien 1970), nos. 19-21.
 35. Cf. B. Levick in Hommages à M. Renard ii (1969), 426 f. for Caracalla's possible route in Asia Minor in this year. She does not connect the A.I. route with that which she suggests for the Emperor.
 36. SEHRE 516-7; cf. 484.
 37. For references to more recent views, cf. Rickman o.c. (n.31) 278 f.
 38. The same phenomenon explains why the capital of France is Paris and not 'Lutece'.
 39. Rickman 280, 282.
 40. We should of course always be wary in our use of the SHA, but there is no reason to distrust its information in this case.
 41. For the continuation of this practice in barbarico, cf. Amm. Marc. 17,9,2, referring to AD 358.

42. E. Birley, Trans. Cumb. and Westm. Ant. and Arch. Soc. xxxix² (1939), 210.
43. Cf. the wise note of caution sounded by M. Hassall in Aspects of the Not. Dig. (n. 3 above), 103-4.

CONCLUSION

Since we have looked in some detail at the reasons for a particular general's or Emperor's actions in our distant province, it may be as well to stand back and view from a wider perspective. What factors led an Emperor like Severus to wish to advance into Scotland? Were they the same factors which led Claudius into deciding to invade England?

But Caesar came first. Since we concluded that he intended some sort of 'conquest' of Britain, and was sufficiently unsuccessful to be embarrassed by the presence of the elephant, what precisely were his aims at the start?

Caesar's only specific reference to his aims is his remark (B.G. IV, 20) that in almost all his Gallic campaigns, Britons had been assisting the Gauls. The implication is that he invaded to try to prevent this happening in the future, if there were a rebellion in Gaul. Yet while this aim seems clear enough, it is almost the only one of his aims in which one simply cannot evaluate his success.

One episode proves that the first invasion was outstandingly successful. The Senate voted ceremonies of thanksgiving to last for 20 days. This was five days more than those given for the conquest of Gaul! Most modern authorities agree that the prime purpose

of the first invasion was reconnaissance, yet it cannot have been reconnaissance which occasioned such jubilation in Rome. The primary reason must be the crossing of the Channel - a crossing over the mighty Oceanus himself - and landing on the shores of what was still considered almost a mythical country. (For an interesting discussion of the impact of a Channel crossing, and of Oceanus as a deity, see C. Caplan and T. Newman, Arch. Ael. 5⁴, 1976, 171-6.) If that was the reason for the celebrations, it supports our earlier thesis, that the elephant coin was intended to symbolise the crushing of that mighty sea-monster, Oceanus.

To turn to the second Invasion. Caesar notably gives no advance reasons for undertaking it, nor any claim of 'mission accomplished' afterwards. Collingwood considers in some detail what Caesar's achievements were by the end of this expedition:

"From a military point of view, Caesar was entitled to regard his invasion of Britain as a success. The chief problem of strategy had been the discovery and destruction of the fortresses belonging to the British tribes against which he was fighting, and this had been done."

This is technically true, as long as the 'strategy' is defined as limited to precisely those hillforts which he chose to attack. But why should we define it as such? His overall strategy can hardly have been merely to terrorise a few natives and capture

hillforts. The main success was the capture of the major hillfort which had acted as the HQ for the British opposition. But it was still Caesar who took the initiative in offering a settlement, and this is not the action of one who had forced his opponents to submit.

Collingwood continues, "From a political point of view, the success, if less complete, was no less real." Of the tribes with which he had come into contact, every one submitted before the end of the summer, he says. But this is really just making the same point - until he goes on to say, "These submissions were precarious and were always liable to be followed by revolt, but at any rate the political consequences of his actions in Britain were as favourable as those which he had achieved in Gaul." However, there is a vital difference between the situations in Gaul and in Britain: in one, he had the forces to subdue any rebellion and to carry out any threats - indeed, it was just such a threat from his opponents which necessitated his return to the Continent. At the end of 54 BC he had no bargaining power in Britain, and if anyone on the British side failed to carry out their part of the agreement, he was in no position to make them change their minds.

It is here that the elephant incident becomes the decisive evidence against Collingwood's idea of Caesar's 'political success'. Even if we suppose that he could have boasted a 'conquest' of Britain by conquering the more civilised south-eastern part, he did

not ride round this part on the elephant, and thus could not claim to have equalled Domitius' grandfather in conquering a new province. His second invasion was undoubtedly a political failure, not a success.

What about the possible less tangible benefits from the invasions? Collingwood has to admit that "from a financial point of view success was more doubtful." He points out that the existence of the lead, and even gold, mines in Britain was still unknown to the Romans: "slaves, however, were to be had in plenty." Indeed they were: if Britain were conquered - which it was not! The financial success of the invasions is not doubtful: it is non-existent.

The Economic Factor

Let us now examine, as far as possible, the importance of economics in prompting the Romans to invade.

The common and infuriating bias of our ancient sources is that of scarcely mentioning economics. To some extent this no doubt reflects the smaller understanding of such matters by those in power. (Which is not to say that our understanding nowadays is so much greater.)

Where our sources fail to mention economic factors, then, that omission does not prevent fiscal considerations being even the most important factor in a particular decision. And where they are mentioned, we can be pretty sure they were an important factor. The exceptions might be cases where we can suspect imperial propaganda

is being put over. Though even if we suspect this in the case of, say, the Strabo passage discussed below, it is interesting that the reader's consciousness of possible economic factors was thought sufficient for the statement to appear plausible. But, for example, when Suetonius makes no mention of a financial advantage in the Claudian Invasion, or when Dio implies that the only reason for Severus' expedition to Britain was to stop his sons quarrelling, that is certainly no reason for assuming that economics did not enter the question.

Given the scarcity of evidence, perhaps the best way to start our assessment of the importance of economic factors would be to look for occasions when we can feel sure they did not play any part, or where they were without doubt influential.

In the case of Julius Caesar, we have already seen how unlikely they are to have been a factor: the only mineral resources he is even likely to have known about were not in the area he can have intended to conquer.

Agricola's remark about Ireland (Agr. 24), that its pacification 'etiam adversus Britanniam profuturum, si Romana ubique arma et velut e conspectu libertas tolleretur' might seem a clear case where invasion was contemplated for wholly non-financial reasons. But in context this is less clear. First, the removal of 'the sight of a free country' is given as an additional reason for invading. The primary one is ostensibly the arrival of an Irish king thrown out after an internal

revolt. One gets the impression that this would have been a good excuse, rather than the actual reason, for invasion. (It also suggests a parallel with the king exiled from Britain in 43, and 'prompting' the Claudian invasion of Britain.

But even if the removal of libertas was regarded as a major reason for invasion, the fact remains that Ireland was not invaded. Given that Ireland is surely a place where we can consider any proposed invasion to have had little to do with economics, its non-invasion then leaves us with the possibility that economics was a major factor when an invasion was carried out.

Turning to Scotland let us suppose, for the sake of argument, that either Agricola or Severus did at some time intend to invade and conquer the whole of it. This would not been for any 'profit' to be made out of the occupation. But the policy could still have been prompted by financial advantage. Both Hadrian's and the Antonine Wall were unquestionably very expensive to maintain. Had the Highlands been conquered, the three legions stationed down in the south would naturally be moved up there, probably with reinforcements from the auxiliaries who had occupied the Wall.

Let us not forget there are faint indications from recent excavations that one legion may have been moved up to Carlisle for a short time in the early third century, and for along time it was thought that Corbridge was being prepared for a similar force. However that may be, the

three British legions were always stationed remarkably far back from the frontier line, if one compares them with other frontiers, such as Germany, where most of the legions were stationed on the line.

This might suggest that even after the Highlands had been conquered, a permanent occupation force would have been necessary. That was certainly true in Germany and other provinces along the Rhine-Danube frontier, which was always threatened by barbarians across the river. However, the situation in Britain was quite different: it could be compared more with somewhere like Wales. Once Wales was conquered, a few auxiliary forts remained to keep an eye on things, but there was no need for a full legion. The Highlands, once conquered, might require a few auxiliary units in the same way; the three legions could be withdrawn to strengthen another province. Thus, with its legions withdrawn, and only a few auxiliary forces left, Britannia as a whole would have been a financial asset to the Empire, rather than a drain on its resources.

That it was such a drain even in its final form - with Scotland left outside - is told us by Appian, writing in the mid-second century: "The Romans hold more than half the country, but have no desire for the rest, for not even the part which they hold is profitable to them" (Praef. 5). Economics, then, may well have been a factor in the decision to move north.

Tacitus, of course, refers to 'Romani nominis gloria' (Agr. 23) as the chief stimulus for Agricola. While this

may have been Agricola's motive, one suspects the government in Rome was more hard-headed.

Let us now turn to an instance where we are specifically told that economics was the major reason for a decision - in this case, a decision not to invade.

The official reason for Augustus' decision not to invade Britain is that given by Strabo (IV,5,1-3). He tells us that certain British chiefs are very friendly, and have even dedicated offerings on the Capitol. Also, they pay large dues on both their exports and imports, 'so that the island does not require a garrison.' It would take at least one legion and some cavalry to collect tribute from them, and the cost of such a force would counterbalance the additional revenue. If tribute were exacted, the imposts would inevitably dwindle, and certain dangers would arise if force were employed.' The final clause there is so vague, and so much like a Civil Service handout, one can be virtually certain it represents an official viewpoint. But the whole statement is notable on several counts.

It does not seem to have been noticed that the reference to 'one legion and a few cavalry' bears close comparison with a remark in the Agricola (c.24). Tacitus says of his father-in-law, 'Saepe ex eo audivi legione una et modicis auxiliis debellari obtinerique Hiberniam posse.' It looks as though this was the common estimate for occupying a country where little resistance was anticipated. At any rate, when we compare this with the five legions which Caesar took

for his attempt at conquest, Britain must have been regarded as a far easier prospect by Augustus' time.

Dr John Mann has made the interesting suggestion, prompted by Agricola's remark about Ireland, that the 'legion plus a few auxiliaries' may have been the force used by Agricola in invading Scotland. The Augustan remark does add slight credence to this suggestion, but we have seen above that the size of Agricolan marching-camps indicates a much larger force, if one abides by the estimates made above for the size of Severus' forces.

I attempted to show elsewhere (Latomus 32) that the Strabo passage dates from before 7 BC, and refers to a British pact made with Rome in about 14 BC. Augustus came back to Rome to announce universal peace, which included diplomatic peace made with Britain --- and closed Janus for the third time, to symbolise this. The trade and other contacts mentioned by Strabo must have come about as a result of this entente cordiale.

In another paper (Historia 24) I tried to show that the Classis Britannica was founded at Boulogne as early as 13 BC. This seems the best way to interpret a mysterious remark of Florus (II,30,26) that Nero Drusus 'Bonnam et Gesoriacum pontibus iunxit classibus-que firmavit.' It was also suggested that the pontes concerned were a major highway, partially built on logs across marshy ground, running from Boulogne to Cologne, and probably originally to Bonn. This road, I said,

was originally built "to facilitate movement of troops and supplies, as well as improving communication between western Gaul and Germany." But of course improved communications are a natural incentive to trade, and the road concerned did become a major trade link from early on, until the Middle Ages. As to the British fleet, the protection of trade seems to have been its primary purpose at the start, in eliminating piracy, and at the same time enforcing the payment of customs dues. All this serves to reinforce Strabo's remarks about the importance of British trade at this time.

However, the friendly British chiefs to which he refers were ousted soon after by others inimical to Rome, and the trade relations ~~must~~^{may} have been severely curtailed at the same time. Mind you, the question bears further examination. Strabo has it that cross-Channel trade was the primary consideration counting against an invasion by Augustus. But as we have seen, this remark represents official policy, and was only true if what they said was true. By the time of Tiberius, rulers in Britain were less friendly, and during the reigns of Caligula and Claudius we have other cases of pro-Roman British royalty forced to flee from the country. Yet this need not imply trade was diminished. However much they disliked 'oily Italians', it was still to the Britons' advantage to trade with the Continent, and there is no reason to think the fleet became less effective at policing the Channel.

It is true that we have the reference to Britain 'tumultuantem ob non redditos transfugas' (Suet. Claud. 17). This might refer to a massacre of Roman traders in Britain. It might refer to piratical raids on the coast of Gaul. Whichever of these, it could scarcely represent a tangible threat to cross-Channel trade. So even if this 'tumult' was an important factor, the invasion would have been prompted by a wish to uphold Rome's name and remove such 'pinpricks', not to maintain trade.

What then of mineral wealth, the most obvious financial asset of a successful invasion? Tacitus specifically says, in the context of Claudius' invasion, 'fert Britannia aurum et argentum et alia metalla, pretium victoriae' (Agr. 12,6). Most recent authorities are happy to indicate that it was one of the factors, while being careful not to indicate how important they think it was. Richmond, however, is an exception. Basing himself on the passage quoted, he states (Richmond-Ogilvie Commentary p. 47): "Economic advantages, such as they were, are firmly regarded as the reward of victory, not as its objective." This is surely stretching the word pretium too far: it can carry both meanings. Nor is the logic sounder, as can be seen by an analogy. Supposing a foreign army nowadays were to invade South Africa to gain control of its mineral wealth. If they succeeded, the minerals could be described as 'the reward of

victory': but they would have been just as much the original objective.

Nor must we forget that in this passage Tacitus is using his usual derogatory language, and is undervaluing the mineral wealth, if anything. After all, we find Solinus referring to 'metallorum larga variaeque copia quibus Britanniae solum pollet venis locupletibus' (CRM 22,11). Tacitus' remark should indeed be taken more seriously than such remarks from theoretical historians, since he discussed the Scottish invasions with his father-in-law, who would have been considering such points when contemplating his own invasion. (Or Agricola would at least have heard from his superiors about the reasons for invasion.) Sadly, however, Tacitus' remark does not tell us whether or not minerals were a factor in the Invasion.

If we consider specifically the mineral wealth of Wales, knowledge of this must surely have reached Roman traders well before Claudius' reign. Since Tiberius chose not to invade, Claudius must have had more in mind than just minerals.

This argument applies especially to the gold mines, which must likewise have been known to exist. It has been shown that the original plan for the conquest of England involved 'cuncta cis Trisantonam et Sabrinam' (Ann. XII,31), and that a limes was erected along this line. This clearly shows they did not intend to annex Wales, at least at this stage (cf. Webster,

Arch. Jnl. 115, 49f.). Doubtless they realised that subjugation of Wales would not pay for itself for a long time, and could only do so if the main troops could be withdrawn later (as they eventually were). To start with, then, they were quite prepared to leave the Welsh mines out of the Empire. Indeed, in his novel 'Imperial Governor', George Shipway makes the interesting suggestion that the Welsh goldmines "were not worth a campaign", whereas the lead mines were much more important. After all, it is from leadmines that silver is normally obtained - and it was silver in large quantities which was needed for the Treasury. Yet the decision to leave these alone, and rely on the few leadmines southeast of the Fosse Way frontier, must show that Richmond was right in his conclusion, though wrong in his argument. Claudius' intention in invading cannot have been primarily to obtain Britain's mineral reserves.

We shall probably never know whether it was military glory which primarily motivated the Invasion, or whether it was the opportunity of obtaining a further profitable province for Rome. After all, the rich and fertile southeast of England should have provided enough corn and other goods in tribute to pay for its conquest, as long as no more than, say, one legion were needed to control the territory.

Sadly, that did not come about. Every frontier the Romans tried in Britain was costly and difficult to maintain. Hadrian's Wall, certainly the most

long-lasting of these frontiers and probably the most successful, also may have been the most costly. Likewise, we know that the province remained a drain even in its final form, with Scotland left outside, since the writer Appian,,writing probably soon after Hadrian, remarks: "The Romans hold more than half of the country, but have no desire for the rest, for even not even the part which they hold is profitable to them." (Appian, Praefatio 5)

With hindsight, one could conclude that Nero had the right idea - from the point of view of Rome - when he considered abandoning the province altogether. But the military expansionists won the day, with results we know. And who are we to say they should have thought again?

BIBLIOGRAPHY

- Alfoeldi, A. 'Bonner Historia Augusta Colloquium 1966-7', Bonn 1968, 9-18.
- Asbach, J. 'Analecta Historica et Epigraphica Latina', vol II, Bonn 1878
- Bean, G.E. and Mitford, T.B., 'Journeys in Rough Cilicia 1964-68' (Oesterr. Acad. der Wiss. Phil.-Hist. Kl. Denkschr.), Wien 1970
- Becatti, G. 'Scavi di Ostia' vol. IV, Rome 1961
- Birley, A.R. 'Septimius Severus', London 1971
- Birley, E. Trans. Cumb. and Westm. Ant. and Arch. Soc. 39² (1939), 210
- Birley, E. 'Roman Britain and the Roman Army' Kendal 1953
- Brady, S.G. Classical Journal 47 (1952), 305-16.
- Breeze, D.J. and Dobson, B. 'Hadrian's Wall', London 1976
- Burn, A.R. 'Tacitus: Studies in the Latin Literature and its Influence' ed. T.A. Dorey, London 1969, 35-61.
- Calza, G. Not. degli Scavi di Ant. 11 (1914), 284-91
- Caprino, C. 'La Colonna di Marco Aurelio', Rome 1955
- Chevallier, R. 'Roman Roads', trans. N. Field 1976
- Cichorius, C. 'Die Reliefs der Traianssäule', Berlin 1896

- Cohen, H. 'Medailles Impériales', Leipzig 1880-92
- Crawford, M.H. 'Roman Republican Coin Hoards' London 1969
- Crawford M.H. 'Roman Republican Coinage', Cambridge 1974
- Crawford, O.G.S. 'Roman Scotland', Cambridge 1949
- Cuntz, O. 'Itineraria Romana' I, Leipzig 1929.
- Daremberg, C. and Saglio, E. 'Dictionnaire des
Antiquités', Paris 1877
- Dayet, M. Rev. Arch. de l'Est, 1960, 42-7
- Degrassi, A. 'I Fasti Consolari', Rome 1952
- Edson, C. Class. Phil. 46 (1951), 1-16
- Frere, S.S. 'Britannia: A History of Roman Britain'
2nd edition, London 1974
- Gallivan, P. Class. Quart. 24 (1974), 290 f.
- Gesche, H. 'Caesar' Darmstadt 1976
- Gillam, J.P. and Mann, J.C. Arch. Ael. 48 (1970), 1-44
- Gnecchi, F. 'I Medaglioni Romani' vol II, Milan 1912
- Goodburn, R. and Bartholemew, P. (ed.) 'Aspects of the
Notitia Dignitatum' (British Archaeological
Reports, Supp. Series 15), Oxford 1976
- Goodchild, R. Berytus 9 (1949), 91-128
- Graham, A.J. J.R.S. 56 (1966), 92 f.
- Grenier, A. 'Manuel d'Archéologie Gallo-Romaine'
vol. II, Paris 1934
- Grueber, H.A. 'Coins of the Roman Republic in the
British Museum', London 1910
- Hammond, N. 'A History of Macedonia' vol I, 1972
- Hawkes, C.F.C. 'Britain and Julius Caesar', Proc.
Brit. Acad. 63 (1977), 125-192.

- Head, B.V. 'The Coins of Ephesus', London 1880
- Henderson, B.W. 'Life and Principate of the Emperor Nero'
London 1903
- Jarrett, M. and Mann, J. Bonner Jahrbuecher 170
(1970), 178-210
- Jones, A.H.M. 'The Later Roman Empire' Oxford 1964
- Jones, C.P. J.R.S. 56 (1966), 66-9
- Jones, G.D.B. 'Rhyn Park Roman Fortress: Excavations 1977'
Border Counties Archaeological Group 1977
- Kraner, F. and Dittenberger, W. 'Caesaris Commentarii'
Berlin 1879
- Lacey, W.K. Classical Quarterly 7 (1957), 118 f.
- Lacroix, B. 'Orose et ses Idées' 1965
- Levi, A. and M. 'Itineraria Picta' Rome 1967
- Levick, B. 'Hommages à M. Renard' vol. II,
Bruxelles 1969, 426-446
- Lugand, R. Mélanges de l'Ecole Francaise à Rome
(1926) 124-39
- Macdonald, G. Archaeologia 68 (1917), 161-220
- Macdonald, G. Proc. Soc. Ant. Scot. 52 (1917-18)
203-276
- Maier, I.G. Latomus 27 (1968), 96 f.
- Mann, J. and Jarrett, M. J.R.S. 57 (1967), 61 f.
- Mattingly, H. 'Coins of the Roman Empire in the British
Museum' vols. III, IV, V, London 1936-50
- Melber, J. Jahrb. für Klass. Phil. Supp. Bd. XIV
(1885), 417-683
- Millar, F. 'A Study of Cassius Dio', Oxford 1964

- Mitropoulou, E. 'Deities and Heroes in the Form of Snakes', Athens 1977
- Mouterde, R. C.R.A.I. 1952, 355
- Ogilvie, R.M. and the late Sir Ian Richmond
'Cornelii Taciti, De Vita Agricolae'
Oxford 1967
- Oman, C. Numismatic Chronicle 11 (1931) 137-50
- Overbeck, J.C. Am. Jour. Phil. 90 (1969), 143 f.
- Paget, R.F. Vergilius 17 (1971), 22-38
- Pauly-Wissowa 'Realencyclopaedie'
- Peskett, A.G. 'Commentary on Caesar's Gallic Wars' 1887
- Petch, D. in Jarrett, M. (ed) 'The Roman Frontier in Wales', Cardiff 1969, 33-42
- Rambaud, M. 'L'Art de la Déformation dans les Commentaires de César' Paris 1953
- Rice-Holmes, T. 'Ancient Britain and the Invasions of Julius Caesar' London 1907
- Richmond, I.A. and Mc Intyre, J. Trans. Cumb. and Westm. Ant. and Arch. Soc. 34 (1934), 50-61
- Richmond, I.A. Proc. Brit. School at Rome 13 (1935), 1-40
- Richmond, I.A. 'Roman Britain' London 1955
- Rickman, G.E. 'Roman Granaries and Store Buildings'
Cambridge 1971
- Rivet, A.L.F. 'Town and Country in Roman Britain'
2nd edition, London 1964
- Rivet, A.L.F. (ed.) 'The Roman Villa in Britain'
London 1969
- Rivet, A.L.F. Britannia I (1970), 34 f.

- Robertson, A. Proc. Soc. Ant. Scot. 103 (1970-1),
113-168
- Rodwell, W. Britannia 6 (1975), 76-101
- Rostovtzeff, M. 'Social and Economic History of the
Roman Empire' (rev. 1957)
- St. Joseph, J.K. J.R.S. 55 (1965), 74-89
" J.R.S. 59 (1969), 104-128
" J.R.S. 63 (1973), 214-246
- Scullard, H.H. 'The Elephant in the Greek and Roman
World' (Cambridge 1974)
- Seel, O. Rhein, Mus. 103 (1960), 230-271
- Shipway, G. 'Imperial Governor' Manchester 1968
- Skeat, T.C. 'Papyri from Panopolis' Dublin 1964
- Smith, R.E. Historia 21 (1972), 481-500
- Stevens, C.R.^E Antiquity 21 (1947), 3-9
" "Britain between the Invasions" in
'Aspects of Archaeology: Essays
presented to O.G.S. Crawford', ed.
W.F. Grimes (London 1951), 332-344
- Stevens, C.R.^E History Today, September 1959, 626-7
- Strack, P.L. Untersuchungen zur Römischer Reichs-
prägung des II Jahrhunderts, vol I,
Stuttgart 1931
- Sydenham, E.A. 'The Coinage of the Roman Republic'
London 1952
- Syme, R. 'Tacitus' Oxford 1958
- Syme, R. 'Emperors and Biography' Oxford 1971

- Taylor, M.V. *Antiq. Journal* 24 (1944), 22-26
- Thompson, E.A. 'A Roman Reformer and Inventor' Oxford 1952
- Townend, G. *Hermes* 92 (1964), 467-81
- Toynbee, J.M.C. 'Animals in Roman Life and Art'
London 1973
- Uslar, R. von *Gymnasium* 78 (1971), 213-24
- Van Berchem, D. *Mem. Soc. Nat. des Antiq. de France* 80
(1937), 166-81
- Van Berchem, D. *C.R.A.I.* 1973, 123-126
- Whittaker, C.R. Loeb 'Herodian' 1969
- Wright, R.P. *Britannia* 5 (1974), 289-92