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Author(s): N. Whatley

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ON THE POSSIBILITY OF RECONSTRUCTING MARATHON AND OTHER ANCIENT BATTLES

[THIS paper was read to the Oxford Philological Society on October 22, 1920. Present: J. L. Myres (in the chair), A. C. Clark, J. A. R. Munro, J. K. Fotheringham, G. B. Grundy, E. M. Walker, W. W. How, J. U. Powell, R. H. Dundas, D. C. Macgregor, N. R. Murphy, G. H. Stevenson, M. N. Tod, H. M. Last, F. P. Long, J. Bell, and five visitors. It has never been published, though Mr Whatley has given a brief account of Marathon in the *Proceedings* of the Hellenic Travellers Club, 1927. It was intended to provoke a discussion (in which it failed) rather than for publication, and the author had thought that the publication of *CAH* iv made it out of date. It has, however, been in private circulation for many years, and Mr Hignett, among others, has acknowledged its influence. Mr Whatley has been persuaded to believe that it is perhaps only superficially out of date, and it is printed here more or less as it was delivered.]

* * * * *

I must begin with an explanation of my reasons for writing this paper. I have for some years been interested in modern attempts to reconstruct ancient campaigns and battles—especially those between Greece and Persia—in fact, most of the arguments I am using tonight were first written down in a rather different form in 1913. But I should not have thought it worth while to read them to this Society had not Admiral Custance's book, his lectures, his address to this Society and the discussion which followed it made me feel that this subject is still one of fairly general interest, that we are still far from arriving at certainty with regard to the history of ancient fights, and that it may be worth while to raise some general questions such as, 'How far it really is possible to reconstruct ancient battles with any finality' and 'how far the methods of attempting to do so usually followed by modern writers really are the soundest methods to employ'. I felt, for instance (and I think others did too), in the case of Admiral Custance, that so long as he was using his expert knowledge in cases where there was no doubt about the essential facts and no question of motive or intention, he was most convincing and illuminating. The Athenian fleet at Syracuse *was* made to wait upon the army and this *did* nullify its possible usefulness. Similarly his professional knowledge led him to insist on the fact (already emphasised by Macan, Grundy and Tarn, but constantly forgotten) that ancient ships in line of battle cannot have been packed together side by side like hoplites. But he was less convincing when he proceeded to fix the interval between ships, when reconstructing the detail of the different fights and when interpreting the minds of generals and statesmen. I do not propose tonight to cover the same ground as Admiral Custance. But I hope it will be clear that whatever ancient campaign is selected for illustration the same fundamental problems with regard to the evidence arise in more or less degree. I want, if possible, to raise these fundamental questions for others, better qualified than myself, to discuss.

Nor do I propose to offer any fresh reconstructions. I am afraid that the more I study the subject the more sceptical I become about the possibility of reconstructing the details of these battles and campaigns with any certainty and of discovering what was in the minds of the admirals and generals who conducted them. In saying this I do not, of course, mean to suggest that the attempt at reconstruction should not be made. Not only would such a suggestion be useless, for no one can read Herodotus' account of Marathon, for instance, without automatically trying to reconstruct it; but also the mere fact of playing about with such problems must, I suppose, increase our familiarity with—

ultimately, perhaps, our knowledge of—ancient life. But it must be frankly admitted that there is so far very little to show as a result of a generation of controversy in the way of undisputed new knowledge. Almost the only new fact concerning the Persian Wars about which all modern writers are agreed is the negative one that Xerxes' army was not really nearly so big as Herodotus represents it. And I cannot help thinking that modern writers often spoil a valuable piece of criticism or research by finding it necessary to tack on a constructive suggestion of their own. Obst's *Feldzug des Xerxes* is a good example of this. The book consists mainly of a thorough and valuable analysis of the narrative of Herodotus in relation to his sources and a summary of the chief modern views. Obst appears to have just the kind of mind for doing that part of the work excellently. Unfortunately professional etiquette compels him to tack on original views of his own with regard to the chief battles and these, with possibly one exception, are merely futile. I have some pet theories of my own about the Persian Wars, but they are the merest theories and to drag them into this paper would merely obscure my main idea of reviewing the whole situation. My rather brief experience of teaching makes me strongly of opinion that such general reviews are occasionally healthy. For inexperienced students at any rate the undiluted study of reconstructive theories is apt to have two undesirable effects: first, that they accept what are only clever hypotheses as established truths (I have constantly found this to be the case with the Battle of Marathon); second, that they lose the wood for the trees. We shall never know exactly what happened at Marathon, but we know enough to be able to allot it its place fairly satisfactorily both in the History of the Art of War and in the Histories of Greece and Persia. I am not sure that this fact is not sometimes overlooked in the excitement of the hunt for the Persian Cavalry.

Of all controversial subjects, military history seems especially to stir very deep passions. I suppose there is usually so little evidence that what there is must be used with unrestricted force if it is to carry any conviction. But in this country, where military reconstruction is particularly popular, strong opinions have been expressed and criticisms uttered without much bitterness. We have not descended to the personal abuse which accompanies the controversies conducted by Delbruck on the one side and by Kromayer and Veith on the other. I hope that any controversial criticisms I make in this paper will be regarded as being much more humble in spirit than might be suggested by the rather bald expression of them which limitations of time make necessary. I want to emphasise this point, for the matter is a rather delicate one seeing that several of the authors I shall criticise are senior colleagues of my own (and actually present in this room). I only wish that there was time for me to begin by attempting to express my consciousness of the immense amount I owe to them.

* * * * *

To begin with I want to make a few remarks about military history generally. Battles of all periods are difficult things to reconstruct. In battle many and different events happen simultaneously and changes are rapid. The actors are in a state of excitement and extreme nervous tension—the worst possible condition for viewing a situation with a proper sense of proportion. It is impossible for anyone to know what is happening in every part of an engagement and there is unlikely to be the occasion, even if there is the desire, for an impartial inquiry and examination of representative witnesses while the battle is recent and its memory fresh. There is the greatest difficulty in distinguishing what was foreseen from what was unforeseen, able generalship from a stroke of good luck.

It is particularly difficult to discover what was in the mind of a general. The general himself may not find it easy. No battle follows one simple plan. There are not only constant improvisations to meet new situations, but constant flukes and, above all, constant mistakes. But it is only human to forget the mistakes if they do not lead to disaster and

the flukes if they lead to success. Similarly, outside opinion inevitably tends to regard what happened as having been carefully thought out and intended, which is by no means always the case. But I must give one or two illustrations. A Fellow of this College, being interested in the question of the value of the evidence of participants with regard to the details of a battle, tried an experiment on the spot in the recent war. He had been out at night with a patrol of quite intelligent Territorials. Immediately on returning to our trenches he asked each man how many bombs he had thrown. The total of the answers came to 21: the correct total was 7. The N.C.O. was positive that the officer had recharged his revolver and that he and the officer between them had fired at least 12 shots. Really only 3 had been fired and the officer's revolver had not been recharged. In the same Battalion the Adjutant in describing a night working party said that there was a bright moon and he was surprised that the Germans did not fire: the Colonel, reporting the same occasion in the Battalion War Diary, said, 'Luckily it was a very dark night'. Similar instances will occur to everyone who has tried to reconstruct any military operation from the reports of participants. But there is similar disagreement about infinitely more important occurrences which one would at first think must from their very obvious importance have stamped themselves indelibly on the minds of all spectators. Who raised the white flag at Nicholson's Nek? Every Irish Fusilier with, I believe, absolutely sincere conviction asserts that it was a Gloucester: every Gloucester, with equal sincerity, that it was an Irish Fusilier. To whom belonged the credit of shaking Napoleon's Old Guard as it came up the slope at Waterloo? To the British Guards in front or to the 52nd Light Infantry on the flank? The point has been disputed for 105 years, and even impartial spectators of that memorable scene were at variance. There is the same sort of doubt as to what was in the minds of the generals on great occasions. Was the Battle of the Marne premeditated by Joffre or did Gallieni let him in for it? Who is right about le Cateau, French or Smith-Dorrien? And yet, how many of us on November 11, 1918, innocently thought that now we should really know all about these things. Some of them may be cleared up when reputations have no longer to be saved, but only by means of official evidence such as did not exist in the Greek world. For in the case of modern wars we are in a comparatively advantageous position. For establishing a true narrative of events we have, to begin with, a keen contemporary interest in getting at a true history; specialists employed for this purpose; the Press and its correspondents; official War Diaries kept by every unit in the field and handed over at once to the official historians; written operation orders which are preserved; field messages written or signalled whenever possible and copies of them kept. The numbers of units present on any occasion are known from orders: the strength of these units from Ration States and similar evidence. Above all, we are completely informed about the organisation of the armies engaged, their drill, formations and methods of fighting; the nature and limitations of the weapons they employ.

To assist us in discovering the plans of generals we have their correspondence with their governments, the orders they issued, their diaries, memoirs, and those of their staffs. We know what course of study they pursued at the military colleges, what previous campaigns they may reasonably be presumed to have studied. As a result we can in the case of almost all modern wars get a very good general picture both of the strategy and the fighting; but we are still constantly in doubt about two things: (1) the exact details of what happened on any particular occasion, and (2) exactly what was in the minds of the generals, what was foreseen and what unforeseen. Yet these are precisely the things which modern writers reconstruct most positively in the case of ancient battles.

It will be argued against this that ancient battles were much more simple, and from some points of view this is certainly true. Otherwise we could not even attempt to reconstruct them. Ancient armies were much smaller (though Macan and Grundy are

inclined to credit Mardonius at Plataea with an army several times larger than that of Wellington at Waterloo and than our own regular army in July 1914); but in the case of modern battles obscurity in detail is by no means restricted to the battles where large numbers were engaged, and ancient strategy and tactics were simple almost to crudity: ancient armies fought in close order and therefore in small space and the view of spectators was not impeded, as at Waterloo, for instance, by smoke. But here again there is another side. To begin with there were no expert spectators. Napoleon had an excellent view of Waterloo through glasses, waiting with his reserve, and even when his reserve went in he had no more thought of accompanying it than did Haig of assaulting the Hindenburg Line in person. But Callimachus had no reserve and was in the thick of it on the right wing; Miltiades was almost certainly with his tribal *τάξις*. Alexander in person led the charge of his cavalry into the unfortunate Persians and cannot himself have had any general view of his battles. Even Caesar, whom the Roman discovery of the value of a reserve made more detached, went into the front line when the situation was critical. Secondly, the Greeks, at any rate, had no permanent Staff and no War Office to prepare plans and organise reports—a fact so often forgotten by historians of the Persian Wars. Thirdly, close order fighting is not necessarily easier to reconstruct than open—at least in detail: an Association Football forward would find it easier to reconstruct a particular match than a Rugby forward, and a cricketer would find it easier than either. Fourthly, though Greek strategy and tactics were simple they seem to have been unscientific and rather illogical. War was treated rather as a religious ordeal. There is an element of the heroic combat about many Greek fights besides those referred to by Professor Gardner in the paper on the Lelantine War which he read to this Society last year.¹ In wars between Greeks and Greeks, at any rate, a request for the return of corpses led automatically to a cessation of fighting. There was no attempt to follow up a victory. The two sides went home with as little attempt to molest each other as do the rival teams after a modern football match. Similarly there is very seldom any attempt to take advantages or effect surprises—to attack an enemy's phalanx before it is properly drawn up, for instance, which was the Roman way of dealing with a phalanx. Polybius² tells us that *οἱ ἀρχαῖοι* thought little of victories gained *δι' ἀπάτης* and not *ἐκ τοῦ προφανοῦς*, and the history of Greek warfare in the fifth century so far as we know it on the whole bears this out. But this kind of simplicity is not a help to the historian. From the military point of view it is extremely illogical. In modern times we can assume that every army is at least aiming at the crushing defeat of the enemy and is only fighting a particular battle as a means to that end. But that lack of hard logic about Greek warfare makes it as hard to reconstruct the actions of generals on *a priori* grounds of strategy as it is with the conventional warfare (in many ways so like Greek warfare except that it was fought on horseback) of the age of chivalry. (These last remarks do not apply to Philip and Alexander or to the Romans.)

We lack, then, in the reconstruction of ancient battles, those sources of information which are our chief assistance in dealing with modern military history—written orders, states, diaries and the like. Equally important is the fact that the ancient historians on whose narratives we have to rely were to all intents and purposes as much without this form of evidence as we are. Herodotus, certainly, had practically nothing of the kind; Thucydides very little; Roman historians rather more, because the Romans had more national instinct for preserving this kind of record. The technical military writers—Aeneas, Arrian, Vegetius and others—only rarely throw light on particular campaigns, though they are of some help towards an understanding of Greek and Roman armies. We are thrown back on ancient historical writers who usually take a knowledge of military

¹ *CR* xxxiv (1920) 90–1.

² xiii 3. 2–3.

routine for granted (Polybius and Josephus, who describe what to them are foreign armies, are an exception to this and therefore particularly valuable) and whose narratives are based on very few real military documents, but on more or less careful examination of participants in the battles or of previous similarly unscientific histories. We get the best results with painstaking people like Thucydides and Polybius. When they give a reasonably coherent story (Thucydides' account of the Sicilian Expedition, for instance) I think we may safely accept at least its main outlines. But where their accounts are not wholly acceptable or do not give us as much detail as we should like (Thucydides on Mantinea, for instance, or Polybius on Cannae) and we want to reconstruct or supplement, we at once have to fall back on mere conjecture which, as far as I can see, is very unlikely in most cases ever to become anything else.

Of all ancient campaigns we undoubtedly know Caesar's best. In studying them we can start with a very fair knowledge of the topography of his various campaigns; quite reasonably good knowledge of the numbers engaged—at any rate on Caesar's side; and of the organisation, equipment and methods of fighting of Roman armies of the period. On top of this we have in Caesar's commentaries a clear, in many ways short, but at any rate authoritative, account both of what happened and of what was in the general's mind. Yet even with Caesar we are helpless the moment his account is incomplete or open to the suspicion of partiality. Where, for instance, was Caesar's cavalry in the last stage of the battle of Pharsalus? Did they not rally after being beaten back at first and take part in the flank attack on Pompey, or was this really carried out as Caesar suggests by about eight cohorts of infantry only? If so this looks like another case of *χωρίς ἰππέως*. In particular we suffer from not having a similar record written by one of his opponents. Caesar's estimates of their numbers, for instance, are much less convincing. It must be remembered, too, that we do not know enough of the method of fighting of Caesar's army to reconstruct its battles in exact detail. We do not know, for instance, what was the depth of a cohort, whether there were gaps between cohorts, how the change was made from fighting *densis* to fighting *laxatis ordinibus*; and here again continuous controversy does not seem to be leading to a decision. Nor, I fear, is fresh evidence very likely to solve these problems of battles and tactics. It is very significant that in what is practically the only sphere of ancient history in which we are undoubtedly getting new military knowledge, as distinct from theory, namely, the Roman Empire, it is with regard to the organisation of the Roman Army and its frontier defences that our knowledge is enlarged. We do not get any nearer to solving the problems connected with particular battles (those at Bedriacum, for instance). In all branches of history I suppose we have a much better chance of reconstructing organisations and institutions than events and motives; but I hope I have shown that in the case of military history reconstructing the latter is quite exceptionally difficult and especially in the case of ancient history.

I now want to consider under separate headings the different methods employed by modern writers in their reconstructions. All, of course, start from the ancient texts, though they vary considerably in their attitude to these; but as the texts are unsatisfactory reconstruction can only be attempted with the use of certain Aids.

The *first Aid* is the study of geography and topography of the theatre of war and is employed by all modern critics, but especially by Grundy, Kromayer and many writers of specific articles (such as Sir William Ramsay's investigation of the topography of Xerxes' march through Asia Minor).³

Until someone with the necessary qualifications takes the trouble to do for an ancient campaign what Grundy has done for the Persian Wars and Kromayer for some of the

³ *JHS* xl (1920) 89 ff.

less known later wars, examination of the campaigns is more or less futile. We cannot even argue about them. Topography gives us negative evidence which is almost irresistible. If a particular move is rendered absolutely impossible by the nature of the country, that move (unless there has been an earthquake since) never took place. An examination of a scaled map of the Straits of Salamis shows decisively that the battle of Salamis never took place as Herodotus described it and with the number of ships which he states. The importance of the study of topography to the military historian cannot, therefore, be exaggerated. It is an essential study, but cannot give positive results of itself. Let me take a simple imaginary instance. Suppose that someone had in the distant future to reconstruct a battle fought, say, in the neighbourhood of Shotover, his information consisting of rather vague Herodotean accounts which gave a general but no full or very clear account of the movements, but mentioned certain definite features—The Brickworks, The Reservoir, Open Brasenose, Blackbird Leys Farm—in connexion with these movements. By visiting the district the reconstructor might identify these sites rightly, but he would still be far from reconstructing the battle unless he also knew the numbers engaged, the orders issued, the formations adopted and the training manuals of the period. Only so could he discover among other things how much ground was covered, who was visible from where and when. I have fought many sham battles over that ground: the same topographical features were prominent in each, but the battles were entirely different. I have also given different bodies of cadets the same battle to fight—that is to say issued the same scheme and the same orders. But the resulting battles were never identical or even very similar, though precisely the same tactical features were there. Herodotus, in speaking of what is usually called the second position of the Greeks at Plataea, says that they were drawn up near the fount of Gargaphia and the precinct of the hero Androcrates. We will suppose that Herodotus is right in this. We will suppose, too, that some modern historian has rightly identified these spots (though there does not seem much prospect of general agreement). We are still far from knowing what position the Greeks took up, for even if we accept Herodotus' estimate of the numbers we still do not know what formation the 38,000 Greek hoplites adopted. Supposing they were 8 deep: then they covered $2\frac{1}{2}$ miles frontage; if 16 deep, only $1\frac{1}{4}$ miles: if 4 deep, 5 miles. It makes all the difference, but we do not know. Where too were the 69,000 light-armed and attendants? Till we know that, and I do not think we ever shall, it is hopeless to try and trace on the ground the exact movements of the troops. It is impossible to say at exactly what stage of the battle the Greeks were hidden from the Persians by this or that hill. If the numbers for the two armies usually accepted in this country are even approximately correct the whole district must have been thick with troops; and it would be a clever staff officer who worked out orders for the supposed crossing of one another in the night (during the retirement from the second to the third position) by the Greek left and centre (16,000 and 20,000 strong respectively) which would not break every ordinary military rule of space and time. It is easy enough to draw little squares representing troops on the map, but it is extraordinarily difficult to make these squares correspond to the facts and realise how immensely they should increase in size the moment they are put in motion.

Topography, then, is an essential Aid to military history, but we want much more knowledge than it can supply by itself. It gives good negative results: to positive results it can contribute, but only in a limited degree.

The *second Aid* may be called the use of *a priori* deductions from modern works on Strategy. It is the method particularly followed by Henderson in his *Civil War and Rebellion in the Roman Empire*, in which the evidence of Tacitus is treated with suspicion and the campaigns are rewritten in accordance with excerpts from Von der Goltz's *Nation*

in *Arms* and Hamley's *Operations of War*. No other critic carries this method so far as Henderson, but it is very commonly employed. If the Persians, argues Munro, had intended to march on Athens from Marathon, they would have occupied the passes: they did not occupy the passes, therefore they did not intend to march on Athens. Custance employs this method constantly and every critic inevitably uses it to a certain extent. But it is a method which can justly be used only with the greatest caution, for it tends to make two false assumptions: first, that generals never make mistakes, whereas it is notorious that success in war consists in making rather fewer mistakes than the man on the other side; second, that there are certain great military principles which have been fully understood in all ages. Now it may be true that with a number of qualifications and if allowance is made for changes in armour, the introduction of gunpowder, etc., certain strategical principles are always true in the sense that their employment on suitable occasions always makes for success. Napoleon certainly found it helpful to study Caesar's Commentaries. But it does not follow that generals have always been guided by these principles even today when they have Napoleons as their examples, a vast literature of military science, maps, an intelligence service, trained war staffs, and, above all, when they lead trained and disciplined armies. Much more was this the case in the Greek world before Alexander and Hannibal and Caesar had discovered the art of war and when untrained generals led half-trained troops. I doubt whether Napoleon himself could have been clever with a fifth-century Greek army unless he were given opportunity to train it—certainly not if his own experience of leading large armies was as small as that of, say, Pausanias, and if he had Homer's *Iliad* as his Field Service Regulations. Yet modern writers take up modern books on strategy and rewrite ancient wars in the light of them. The result is magnificent, but it is not ancient war.

Only three things seem to be universally true of all armies:

(1) That a man takes up a certain amount of room and that therefore a large army, especially on a narrow road, takes up a great deal of room. (Xerxes' army, for instance: compare what I have said above about Plataea.) Henderson seems to me to leave this out of account altogether in his reconstruction of the movements before the first battle of Bedriacum. From Herodotus downwards many writers about ancient wars have treated armies on the march as if they were flags stuck in with pins on a *Daily Telegraph* war map.

(2) That a man takes time to move and that with a long column when the head halts the rear takes a long time to come up with it (and yet someone is always surprised if an army on the march delays at all before delivering battle).

(3) That a man has a stomach which must periodically be supplied with food.

I am almost inclined to add, though they are not of quite such universal truth:

(4) That generals make mistakes and do idiotic and irrational things, and

(5) That large bodies of troops are awkward things to handle, and when in contact with the enemy always tend to settle a fight in their own way.

As a note to my discussion of this Aid, I should perhaps just mention the drawing of analogies from modern battles. This again is not only irresistible, but may be of considerable help. But it must be used with great caution, for no two military situations ever are alike. Let me take just one example. Casson⁴ has compared the action of the Persians at Marathon with that of Von Kluck in the great German sweep of 1914. So far as this is a reminder that generals make blunders the quotation seems to me admirable;

⁴ *JHS* xl (1920) 44.

but when he states that the Battle of the Marne is the modern counterpart of Marathon and argues from the supposed parallel action of General Manouri that the Greeks were deployed at the foot of Mount Agriliki, he is, in my opinion, on very dangerous ground. The analogy of modern battles is good argument for the possibility of certain general occurrences in certain types of situation in battle, such as delay, irresolution, over-confidence, etc. It will never prove reconstructive detail.

The *third Aid*—which no doubt overlaps the second—may be called *Sachkritik*, the attempt to reconstruct in accordance with *die Realität der Dinge*. It is the favourite German method. Like the last it is an *Aid* which must be used—I am trying to use it myself in this paper—but is very constantly misused. On the whole I think we may say of *Sachkritik* as of Topography that it is much more valuable as negative than as positive evidence. To take a stock instance. Delbruck⁵ shows that if Xerxes' army really numbered the five millions credited to it by Herodotus and if, as in many places the geography requires, it marched along one road in a narrow column, even if it had much less baggage and as good march discipline as a modern army, then just about when the Advanced Guard reached Thermopylae, the Rear Guard was leaving Sardis. This is a very good way of proving that Xerxes' army was anyhow much smaller than Herodotus states. Hauvette's⁶ reply that the army marched in great squares can easily be destroyed by further employment of *Sachkritik*. A literal interpretation of the mile run at Marathon may I think be similarly disproved in spite of what Hauvette⁷ declares himself to have witnessed in the case of French soldiers. But the moment anyone tries to get positive results from *Sachkritik* the result is much less convincing. Take, for instance, Delbruck's method of fixing the size of the Persian army at Plataea.⁸ If the Persians had considerably outnumbered the Greeks at Plataea Mardonius would have detached a turning force. He did not do so (according to Delbruck). Therefore the Persians did not outnumber the Greeks. As Delbruck by rather similar methods has fixed the strength of the Greeks at 20,000 hoplites and 40,000 light-armed, he concludes that therefore the Persians had altogether 60,000–70,000 men.

One of the chief difficulties seems to be that it is hard to get agreement as to exactly what *die Realität der Dinge* is. Delbruck⁹ is very anxious, for reasons which I need not go into here, to prove that in the Macedonian phalanx which met the Romans at Pydna and elsewhere each man was allowed a frontage of only $1\frac{1}{2}$ feet. To support this argument he apparently borrowed the long spears in the Zeughaus in Berlin, armed his seminar with imitations of them and found that they could work quite well as a phalanx with only $1\frac{1}{2}$ feet to each man and claims to have proved his point. But Veith,¹⁰ who is a real *Hauptmann*, says that this is nonsense. He has drilled a lot and his whole military experience makes him certain that a man must have more room in the ranks than $1\frac{1}{2}$ feet. Again Delbruck¹¹ wants to prove that the gaps usually supposed to have existed between maniples in the Roman army before the time of Marius were not really there at all. To do this he has to ridicule the well-known statement of Polybius¹² that at Zama, because of Hannibal's elephants, Scipio placed the maniples of the *principes* behind the maniples and not as usual behind the gaps of the *hastati* (i.e. so as to allow the elephants an attractive avenue to bolt along when they became excited). Delbruck thinks the whole idea mere imagination. Why make avenues for elephants? Elephants, even infuriated ones, could

⁵ *Die Perserkriege und die Burgunderkriege*, 138.

⁶ *Hérodote*, 311–12.

⁷ *Ibid.*, 261.

⁸ *Op. cit.*, 144.

⁹ *Geschichte der Kriegskunst*, i³ 433.

¹⁰ E.g. in Kromayer-Veith, *Heerwesen u. Kriegskunst*, 358.

¹¹ *Perserkriege u. Burgunderkriege*, 269 ff. esp. 303–4.

¹² xv 9. 7.

perfectly well zigzag. He does not, unfortunately, seem to have borrowed elephants from the Zoologischen Garten in order to demonstrate this point with his seminar, but quite apart from the far-fetched nature of the argument, the inherent weakness of it is that even if Delbruck is right about the habits of elephants, Scipio may not have been equally well-informed, or if well-informed, may have been unwilling to take the risk.

I could continue this sort of thing indefinitely, but I hope I have said enough to show that the moment Sachkritik is used to give positive constructive results it is very easily abused.

Note to Aid 3 on Numbers. I am not in this paper going very fully into the problem of the numbers of ancient armies, immensely important though it is. The details of a campaign, for instance, cannot be reconstructed without fairly exact knowledge of numbers or at least of the number of units (legions, for instance) present on both sides. The exact number present with each legion, so long as we know it approximately, is not quite so important. For the general understanding of a campaign exact detail as to numbers is not quite so essential, though here too we must have approximate knowledge and above all we must know the relative strength of the armies engaged. Three methods are available for dealing with numbers:

- (1) To accept the numbers given to us by the ancient authorities.
- (2) To argue from probabilities and possibilities.
- (3) To deduce the numbers from what we know of command and organisation.

With regard to these:

(1) No one accepts all the figures given by the ancient authorities. Often they are incredible (Herodotus' numbers for Xerxes' army, for instance), and often the authorities contradict each other or themselves. Many modern authors, however, select a number here and there which suits their theories, but there is considerable variety of choice. Many suspect round numbers but jump at exact figures such as 1,207 or 53. Others (Tarn for instance) are more inclined to accept the round numbers because they look like evidence of a definite organisation. Beloch rejects both: round ones because they are round: odd ones because if you make a judicious selection and add them together they become round. Numbers of troops based on the calculations of participants in battles, even generals, are so notoriously unreliable that I think it impossible to put much faith in the numbers given by Greek historians until we have evidence that scientific methods of counting were employed.

(2) The argument from possibilities and probabilities helps, as I have already argued, to reduce absurdly big numbers. It does not help so much to fix exact numbers. We cannot deduce the numbers from the tactics employed or from the casualty list (even if the latter is accurate). Munro's suggestion¹³ that 20,000 Persians fought at Marathon on the ground that 6,400 were slain and the Persian centre wiped out and that $6,400 \times 3 =$ about 20,000 is to my mind unacceptable as an argument even if the conclusion be approximately correct. A centre is not very likely to be exactly one-third of an army. It is a rough tactical not an exact mathematical division, nor is it at all likely that the centre was really exterminated or that the wings got off scot free, or that the survivors of the centre were equal in number to the killed of the wings. Still, Munro does not urge that argument at all strongly.

¹³ *JHS* xix (1899) 189 n. 1.

The argument from the analogy of what we know of the population of Greek states later in the fifth century is of some help with regard to Greek armies in the Persian wars, but every link in such chains of argument is weak and the conclusions arrived at cannot really be so positive as Beloch would have them.

(3) The argument from command and organisation is more promising at any rate with armies which, like the Persian, appear to have had a thorough organisation. Nothing is more attractive in the writings about the Persian Wars than Munro's discussion of the organisation and strength of the Persian army¹⁴ and Tarn's corresponding treatment of the navy.¹⁵ But even if the recurrence of certain round numbers such as 10,000 and 60,000 does point to some real bit of Persian military organisation, it is still possible to argue for 300,000, 180,000 and even 60,000 as the strength of Xerxes' army. Even if new evidence from Asia teaches us more about the Persian army we shall still probably remain uncertain as to what units of this army came to Greece and how far these units were up to establishment. There is enough evidence to justify ingenuity: not enough to hold out much prospect of certainty.

The *Fourth Aid* is what I think I may call the Sherlock Holmes method. This again is used inevitably by all historians, especially in this age of *Quellenkritik*, but its own particular master is Munro. Reading his article on Marathon leaves me with just the same feeling as reading Conan Doyle. It is so attractive and such an artistic whole that it seems almost a crime to take it prosaically to pieces and inquire whether the steps in the first argument do follow one another so irresistibly as at first appears.

This Aid consists in a combined use of the three Aids I have previously mentioned together with an ingenious selection of statements from ancient authors of different periods and a subtle interpretation of them. Here I am touching on a very big question which affects a great deal of the modern interpretation of, at any rate, Greek history and I want if possible to avoid raising that question in too general a form tonight, but ancient history does lend itself particularly to this kind of treatment. The subject, we have already seen, is one in which we often know little of facts and still less of motives, and thus a wide field is open for speculation. The evidence is very incomplete. There are obvious difficulties and gaps and the temptation to try to solve these difficulties and fill in the gaps is very great. It goes against the grain to admit that we cannot find out with greater certainty. The ancient accounts (that of Herodotus, for instance) are often tinged with a supernatural colouring and do not appear to be the work of military experts. This gives many opportunities for accepting one statement and rejecting another so as to suit a particular theory; also for inventing motives and strategical designs to explain the selection of facts which one has made from the unmilitary ancient historians. Furthermore, the ancient tradition of the Persian Wars grew and grew until by the early years of the Christian era it had incorporated many details which are absolutely contradictory to the statements of our ancient historians. I do not see any prospect of the most exhaustive *Quellenkritik* ever really deciding which of these details are late additions and which really go back to Ephorus or, even if it could, of deciding for Ephorus against Herodotus. It seems so clear that the Sherlock Holmes method began to be employed in a mild way very early in Greek historiography and the chances that the earliest account of a Greek war that we possess is the best seem to me very great indeed. But the modern method is so often to accept as sound any element in the later stuff which suits a particular theory (for instance, Nepos on the Athenian defensive arrangements at Marathon, Suidas on the Persian cavalry

¹⁴ *JHS* xxii (1902) 295-8.

¹⁵ *JHS* xxviii (1908) 202 ff.

or Diodorus on Salamis) and to reject the rest as valueless. I cannot help thinking that a sounder attitude is that declared by Tarn to be his,¹⁶ that we cannot make much use of this late evidence, though it is at least interesting when we find that our own conclusions agree with it.

Lastly, the wars I am chiefly alluding to tonight belong to a period of history which every classical scholar studies and on whose interpretation there is, here in Oxford in particular, great concentration of energy. The evidence being what it is this has of itself, I think, made for rather excessive ingenuity. Each new writer or teacher tries to screw one little bit more out of it. Compare the very elaborate causes which are now usually alleged for the Peloponnesian War and contrast any ancient war which has not attracted many historians, where it will be found that people are still allowed to go to war for quite simple reasons and to fight in quite simple ways. I am going to deal with Marathon in a few minutes, but a few points about Marathon here will illustrate my point. Munro's theory argues from, among other things, a supposed silence of Herodotus on one point, a rather strained interpretation of one phrase in Herodotus (about the Persians being in their ships when the shield-signal was made) which involves supposing that Herodotus has got the phrase from someone else and has surrounded it in his account with statements which are false, from an explanation in Suidas of the proverb *χωρίς ἰππεύς* and from a statement in Nepos (though this is not urged strongly) that 100,000 Persian infantry were present at the battle, when he has previously mentioned 200,000 as the total Persian infantry strength—from these and other similar points it is argued that half the Persian infantry and all the Persian cavalry were on ship-board during the battle. (Incidentally Nepos says that all the cavalry were present at the battle and Suidas never says that they were on ships.) A comparatively slight supposed silence of Herodotus is thus employed as an argument in favour of a whole theory of the battle about which Herodotus is entirely silent and which in my opinion involves shutting up Herodotus as absolutely valueless at any rate on this battle. Yet How,¹⁷ in replying to Casson, argues for Munro's theory and maintains that in so doing he is arguing for the authority of Herodotus. It seems to me that it would be at least equally legitimate to argue back again from the silence of Herodotus as to the cavalry being in the ships and from Nepos' statement that the cavalry were present, to a battle very like that which one would naturally deduce from Herodotus' narrative. It is merely a question as to which of many flatly contradictory bits of late evidence you select. But more about Marathon later.

Note to Aid 4. Another rather popular method of dealing with the ancient evidence is to cling to one main ancient authority (Herodotus, for instance) and to try and get some fixed rule for his interpretation. The rule most commonly adopted is to suppose that Herodotus heard the views of soldiers who took part in the battle, but not the views of officers: that the facts in Herodotus are, therefore, more or less right, but that the interpretations put upon them are wrong or distorted. Henderson takes much the same view of Tacitus and Custance of Xenophon. With regard to this matter I would urge:

(1) That to my mind fixed rules of this kind cannot be applied at any rate to Herodotus, who is good and bad on no fixed system. It is part of his charm.

(2) Even if it were correct it would not follow that we have got the facts right. I have already tried to show that a private soldier's imagination soon gives him a very false picture even of that part of a battle in which he was personally engaged and also he certainly applies what was only local to the whole fight. A much more

¹⁶ *Ibid.* 203, but *cf.* 232.

¹⁷ *JHS* xxxix (1919) 48 ff.

critical investigator than Herodotus would be required to find out even the facts. Details of distance and the like would be particularly unreliable.

(3) The attempt to work out the theory in detail leads to what seem to me rather strained suppositions.

(4) I find it hard to believe that even in democratic Athens Herodotus, the friend of Pericles, should have got to know only the families of privates and never those of officers. This is at least equally true of Xenophon and more so of Tacitus or his sources.

I think that camp gossip did probably lead to many of the absurdities which have crept into our accounts, but recognition of the fact will only help us part of the way towards reconstruction.

The Fifth and last *Aid* consists in making the most thorough study from all sources of the armies engaged, their strategy and tactics, their weapons and method of using them, their system of recruiting and organisation, their officers and staff. This, when taken in conjunction with a judicious use of the other Aids I have mentioned, seems to be so extremely helpful, if only as a guide to the understanding of ancient authors, that it is surprising that it has not been more consistently employed. In the case of the Persian Wars, Macan makes frequent allusions to Greek military peculiarities, but his book contains no full consideration of the subject. He never, for instance, to the best of my knowledge, emphasises the difference between a Greek and modern army in the matter of a staff. Grundy draws constant attention to the difference between Greek and Persian methods of fighting, but his book on the Persian Wars does not, like his book on Thucydides, review the whole subject in full detail. Tarn, in his article on the Fleet of Xerxes, does base many of his conclusions, which are, however, rather far-fetched, on a thorough study of ancient naval warfare. But to the best of my knowledge Delbruck is the only historian of the Persian Wars who employs what seems to me to be the correct method of starting off with a study of the Greek and Persian armies. He is a historian of wars throughout the ages and has thus realised that in any age you must understand the armies before you can understand the wars. Unfortunately he is not a reliable Greek historian and his excellent method is spoiled: (1) By not being thorough enough. He omits an adequate treatment of the higher command of Greek armies and seems to assume that somewhere at the top is a German war-lord with a trained staff. (2) By misinterpreting the evidence about the Greek and Persian armies when, for instance, he says that the Greeks had no light-armed combatants and the Persian army was a small army of picked troops with no general levy. (3) By the blatant misuse of Sachkritik which I have already illustrated.

But on the whole modern writers have not made a sufficient use of what we can learn, little though that is, from the study of ancient armies. Occasionally a reference is made and occasionally a parallel quoted. But often, I think, further investigation will show that the parallel is not a real one, or that there are other instances which point to a different conclusion. I will illustrate this when I discuss the inferences usually drawn with regard to Marathon from the behaviour of the Persian cavalry at Plataea.

The armies of different countries differ as much as the constitutions. No one would endeavour to interpret the political history of the age of the Gracchi without making a thorough study of the Roman constitution. I suggest that a study of ancient armies is equally necessary for an understanding of ancient military history.

In discussing these five Aids I have tried to give some idea of what is in my opinion the legitimate and the illegitimate use of them. I am afraid my discussion has consisted mainly of a scattered collection of remarks, but would it not be possible for some better

qualified historian to propound principles of criticism whose general acceptance might act as a steadying influence in military reconstruction? Have we not been rather carried off our feet by the great flood of research? Where is it taking us? The propounding of these questions is really the chief object of this paper. But I feel that in order to justify many of the criticisms I have suggested I must give rather more illustration than I have done. So I propose, in conclusion, without attempting to go into all the problems raised by the battle of Marathon, to offer a few reflections on one or two of the salient points in modern reconstructions of that battle.

I select Marathon for several reasons. Not because I agree with Casson's statement¹⁸ that 'Marathon, perhaps the most important battle in antiquity, is the least accurately described'. The importance of Marathon seems in many ways to have been exaggerated by most ancient writers except Herodotus, and even Herodotus shares in the exaggeration in Book ix, Chapter 27. It certainly was not one of the decisive battles of the world. It decided nothing, for the Persians came again in ten years. Certainly it illustrated, possibly for the first time, the superiority of the hoplite in close order to the skirmishing Persian; but not in a way which the Persians accepted as decisive. As for accurate description, one has, I think, only to wade through half a dozen of the campaigns that are known to us only through Diodorus, Plutarch and others, to realise that it is just because we do know quite a lot about Marathon that we are all so desperately anxious to know more. My reasons are rather:

- (1) It is the first battle in Greek history that lends itself to criticism.
- (2) The whole campaign is so small that the main problems stand out clearly.

(3) (My chief reason) It is the battle of all others in which at any rate in this country what are mere theories are tending to be regarded as established truths. Practically every English writer from Macan and Bury onwards has accepted the theory that during the battle the Persian cavalry were on shipboard; practically every English writer since Munro has accepted his suggestion that half the infantry was on shipboard as well and that the landing at Marathon was a mere feint to lure the Athenians out. Grundy agrees with Munro on most points. How and Wells almost entirely; How, in the *JHS*,¹⁹ speaks of 'Theories already well known and in England at least widely accepted'. Caspari, in an earlier number of the *JHS*,²⁰ says, 'Why did the Persians offer battle at all in an unfavourable position and why did their cavalry take no part in the action? Since no adequate answer has been given to these questions, the presumption is in favour of the alternative theory which has been adopted by the leading English historians.'

So strong is the tendency to regard the embarkation of the cavalry as established that Casson in his earlier article²¹ while arguing for the acceptance of Nepos' view of the battle, which states that the cavalry were present and that the Athenians constructed special works (of a suspiciously Roman type, by the way) to render them ineffective, still accepted the fact that the cavalry were on board. 'Undoubtedly the Persian cavalry was re-embarked, not so much for its supposed utility on the plains of Phalerum, but because of the discovery of the deceptive nature of the place which had been specially selected for its manœuvres.' In his later article,²² logically, I think, he modifies this view. 'The presence or otherwise of the cavalry is of less importance if the battle proves to be the

¹⁸ *JHS* xl (1920) 43.

¹⁹ xxxix (1919) 48.

²⁰ xxxi (1911) 104.

²¹ *Klio* xiv (1915) 69 ff.

²² *JHS* xl (1920) 44.

counterpart of the Marne.' I do not think it will prove to be that; but it is at least comforting to find Casson, the first Englishman for thirty years or so, hesitating to accept the embarkation of the cavalry.

First, then, how far are we well equipped with evidence with regard to Marathon? Herodotus' account contains a number of difficulties, some, I think, less serious than is often made out; but still, difficulties are there. They are not sufficient to prevent a general understanding of the battle, but they are sufficient to prevent a detailed reconstruction unless we have any other really good evidence. Unfortunately we have very little. The other literary sources either help us very little, such as Pausanias, or are of indisputably doubtful worth, such as Nepos and Suidas. The topography of the district is well known and assists a general understanding of the campaign; but although it limits the possible number of detailed reconstructions of the battle I do not see how it is ever going to decide definitely for any one. The Soros is a great asset, but though I entirely agree with those who argue that it probably marks the actual battleground, that of itself only rules out a certain number of hypotheses.

Archaeology has done a great work in proving the antiquity of the Soros, and it is quite conceivable that there is still something to discover by excavation—the site of the Greek and Persian camps, for instance. This would rule out still more hypotheses, but it would probably start a number of new ones. With regard to the numbers we can, I think, regard 10,000 Athenians and Plataeans as right within a few thousand. But of the Persian numbers we are in complete ignorance. The fact that they came in ancient ships rules out the vast numbers of post-Herodotean writers, but whether they were equal to the Greeks, twice their number, or what, is merely a matter for conjecture.

I think, then, that it is reasonable to start an inquiry into Marathon without expecting either to solve all the problems raised by Herodotus' account or to establish much certainty on points of detail. I believe that if we do this we shall get nearer to the truth than we are likely to if we expect to explain everything, especially if we can put Marathon definitely in its proper place in the history of warfare. That involves seeing what use can be made of the fifth and last of the Aids I mentioned above, but unfortunately it is not possible within the limits of this paper to embark on a full discussion of Greek and Persian armies and their methods of conducting war. I must be content with a very few short extracts from what was originally a much fuller treatment.

The Persian Army. There is not time to discuss organisation, strategy and policy, but I must say a few words about tactics. How was the Persian army armed? Herodotus' description of the dress and armour of the different nations is almost certainly incomplete (unless a great many of them were very naked) and is probably not altogether accurate, but certain prominent features stand out which there is no reason to dispute:

(1) There were great varieties of armour and therefore varieties in method of fighting.

(2) There is (except in the case of the Assyrians and a few others) a marked absence of strong defensive armour. Therefore the infantry was very vulnerable at close quarters.

(3) There is a marked predominance of distance weapons (the bow, javelin, etc.) which confirms the impression that the natural role of the Persian infantryman was not hand-to-hand fighting, and this is borne out by what we know of their tactics in the wars with the Greeks. The Persian (whether mounted or on foot) hustled the Greek with arrows; at close quarters he was no match for the Greek hoplite. At Thermopylae, Plataea and Mykale this was clearly the case, and what we know of Marathon bears this out, although the army of Datis and Artaphernes was very likely

a picked force. What formation the Persians employed we do not know, but probably it was much more open and less symmetrical than the Greek. Both at Plataea and Mykale we hear of Persians fighting gallantly in small groups.

The Persian cavalry was obviously an important arm of the service. Its tactics seem to have been like those of the infantry and it made great use of the bow. It did not employ shock tactics, that is to say it did not close with unbroken infantry. (The action of the Theban cavalry at Plataea²³ is an exception which proves the rule. It was Greek cavalry and its victims were advancing in disorder.) The ordinary method was to ride up close to the infantry and shoot, then they wheeled and went back to prepare for another advance. (Masistius at Plataea.) Even in Asia against inferior infantry the Persians did not employ cavalry shock tactics until the opposing infantry was much broken.²⁴

With regard to Persian *ships* I need say very little here, but one can safely say three things: (1) That the number of horses carried in any fleet of the period cannot have been large. The facts, so far as I know them, of all ancient seagoing expeditions support this. (2) Ancient transports were uncomfortable, crowded and dangerous. They were merely a means of crossing water when this could not be avoided. Their first object was to get to land as soon as possible. (3) Though I do not know how ancient horses were embarked and disembarked, it was, if modern analogies are worth anything, a troublesome business not undertaken more often than absolutely necessary.

The Greeks. Here again I can only mention a few points. Most of our evidence comes from a later period. It is not impossible that in 490 the Athenian Army had better organisation and training than sixty years later, but it is unlikely. The discipline quite possibly was better. No Greek army of the fifth century, except Sparta's and possibly Boeotia's, has left to us these little glimpses of organisation which, I believe, always appear in the military records of states like Sparta, Rome and even Persia which have a proper system of discipline and subordinate command. Armies which have not got this are likely to rely on an orthodox type of tactics and on the use of men in bulk more than on individual trustworthiness. The Greek phalanx was a suitable formation for such an army, and there can be no doubt from what Herodotus says both of Marathon and Plataea that the Greeks already employed the phalanx (though we do not know its depth) whose members were armed as hoplites, that is to say their defensive armour was very strong and their offensive armour very strong at close quarters. But until he got to close quarters the hoplite could do nothing. He had not even the pilum of the Roman legionary. The hoplite phalanx on suitable ground at close quarters was a most formidable army. Its weaknesses were:

(1) Its clumsiness. Everything depended on the whole phalanx being properly drawn up. It was quite unsuited for sudden surprises. That is why when Greek meets Greek there is no hurry. Each side let the other's phalanx draw up in peace and it took two to make a fight.

(2) Its inadaptability. It held no reserves. Nothing was left to subordinates. It could only fight one type of battle.

(3) Its liability to confusion. So much depended on the unbroken line. It could not seek out an enemy: only a battlefield. Nor could it attempt subtleties. For much the same reasons it could not pursue.

The Greeks who fought Persia employed no cavalry at all. Light-armed troops were

²³ Hdt. ix 69. 2.

²⁴ Hdt. vi 29. 1.

doubtless present on all occasions, but until the Peloponnesian War light-armed troops seem to have been almost untrained and to have played little part in the battle.

The Greeks and Persians from the point of view of one another. What did the Persian know and expect of the Greek? He had met and conquered the Greek of Asia Minor, who had, however, put up a good fight. How far the Persian had actual experience of Greek infantry tactics we do not know; we do not, I think, even know whether the Ionians employed the hoplite phalanx. They probably did; but, as Grundy points out, the Ionian revolters can have had no military training for some thirty years or more; their phalanx would therefore be out of practice and easily thrown into confusion. This would give more opportunity to the Persian cavalry and may have led to an underestimate of Greek infantry. But what could the Persian do when he met a trained phalanx? If it encamped in the open his cavalry could ride up and shoot arrows at it, as in the second position at Plataea. His infantry could do the same, as against the Spartans at Plataea. But the enemy the Persians were used to were in the habit of fighting in this way too. It is easy to imagine that the Persians were puzzled when at Marathon, the first engagement, the Athenians advanced to close grips at once. (That, I think, was what surprised the Persians: not a mile run literally interpreted but the fact of closing without the preliminary bow and arrow business.) How were the Persians to deal with this? The cavalry which could hustle the Greeks at rest seems to have been quite ineffective against the phalanx in action at close quarters (in the final stage of the battle of Plataea, for instance). Mardonius' curious treatment of the Phocians might even be fancifully interpreted as an attempt to find out what would happen if he charged unbroken hoplites with his cavalry. 'The Persian horse having encircled the Phocians charged towards them as if about to deal out death with bows and arrows ready to let fly, nay here and there some did even discharge their weapons. But the Phocians stood firm, keeping close together and serrying their ranks as much as possible; whereupon the horse suddenly wheeled round and rode off.'²⁵

On the other hand, what would the Greeks think of the Persians? No doubt they were frightened of them, but Athenians in small numbers had dared to go to Sardis; the Ionians had held out for a long time, and Miltiades at any rate knew how the Persians fought. They had every reason to feel fairly confident about their prospects when they got to grips: the problem was how to get there. For, in the open, Persian arrows, if not very destructive, were at least most unpleasant.

I once umpired a Field Day on Roehampton Common between the Kensington Cadets armed with carbines and the Westbourne Boy Scouts who relied on the use of the staff at close quarters. I thought of the Persian Wars, for the real interest in the detail of the fighting in these wars is that the two sides fought in quite different ways. It was the spear versus the bow, as Aeschylus says. How could the Greeks get to close quarter without heavy preliminary loss? No wonder that at Marathon, Thermopylae and Plataea there were delays before the fights.

One last point. In all ancient warfare the Intelligence service seems to have been bad (in the preliminaries to the battle of Issus for instance). I think the Persians and the Greeks were probably much more in the dark about each other's movements than is often supposed.

From this point of view let us approach the modern English theories of Marathon; and I will first deal with the two points common to all the theories and go on to deal with a few of those raised more particularly by Munro. The two points are: (1) Where was the Persian cavalry during the fight? (2) Why was there delay and why did the battle finally take place?

²⁵ Hdt. ix 18. 1.

(1) The Persian cavalry. It is argued that the cavalry were embarked before the battle. The only evidence put forward for this is (i) the absence in Herodotus of any reference to the cavalry during the battle, during the Greek pursuit and in the casualty list. (ii) Suidas' explanation of the proverb *χωρίς ἵππεύς*. Other evidence has been put forward in support of some division of the force, but no other for the specific inclusion of the cavalry in the separated portion.

First, I should like to repeat that, though it is not necessary to suppose, as Beloch²⁶ very typically does, that only horses sufficient for officers and orderlies can have been brought over, the evidence of other naval expeditions is in favour of the cavalry being few in number and small in proportion to the infantry. Also that simply on grounds of general probability, which I admit are not final, nothing is in itself more unlikely or more absurd from a military point of view than a re-embarkation of the cavalry. Incidentally they were the only troops who would have had any chance of stopping the Athenians returning to Athens. Infantry cannot act as a retaining force to infantry unless they block the line of retirement.

Now as to the silence of Herodotus. It is argued that as at the battle of Plataea Herodotus tells us that Persian cavalry checked the Greeks' pursuit we should have heard the same at Marathon if they had been there. At Plataea the Persians were retreating to a camp which cavalry would enter last: at Marathon to the ships which, owing to difficulties of embarking, they must obviously reach ahead of the infantry if they were to get on at all. I do not think I need say more. As to the absence of all reference to horsemen and horses in the casualty list, this is very regrettable; but if it means that no cavalry took part in the battle we must, to be logical, argue that in practically all ancient fights the cavalry were removed before battle. There is, for instance, no such reference in Herodotus' much fuller account of the battle of Plataea. The argument proves nothing at all. So we pass to the more serious argument from the supposed absence of reference to the cavalry in the actual battle. I say 'supposed' because Herodotus seems to me most clearly to imply that they were there, not so much in his remarks about the Persian reasons for landing at Marathon as when, in one of those passages which read so very much as if they came from a Persian source—possibly a Persian prisoner—he says how surprised they were to see a mere handful of men coming on without horsemen or archers. If I read in an account of a golf match that Braid on coming to the first tee expressed surprise that Taylor's bag did not contain a niblick, I should be justified in inferring that Braid had a niblick himself. I have always inferred from Herodotus' account that the cavalry were there with the infantry. But what did they do? I imagine that they were at the very least reduced to the value of Persian infantry by the rapid closing of the Athenian phalanx. This is entirely supported by Herodotus' account of the battle of Plataea, which is almost universally quoted to prove the opposite. At Plataea so long as the Athenians are at rest on the hills the Persian cavalry harass them, but do not close and are driven off; when the Greeks advance and encamp in the open the Persian cavalry makes itself very unpleasant, but again without closing. When the Greeks retire the Persian cavalry hustle them, but the moment the Spartans and Tegeans take the offensive and close with the Persians the cavalry disappears from the picture as completely as at Marathon until it as suddenly reappears during the pursuit, and that in spite of the fact that Herodotus' account of Plataea is much fuller in its detail and the Persian cavalry were much more numerous. Mardonius, we are told, was on a white horse; otherwise the one bit of Plataea which resembles Marathon is described by Herodotus with a precisely similar silence about horses and horsemen. Herodotus does mention (ix. 63) *λογάδας τοὺς ἀρίστους χιλίους* as fighting especially gallantly with Mardonius. These

²⁶ *Griechische Geschichte*, ii² 2, 80-1.

may be the 1,000 picked cavalry. Herodotus expressly says that they were on the defensive and resisted attacks, i.e. they did not do what everyone says the Persian cavalry must have done at Marathon if it had been there, namely, attack in flank or rear. The Greeks had no cavalry at all in either case. Clearly Persian cavalry did not dare to close with an unbroken phalanx or dared and failed. Herodotus, who is writing of things that happened and seldom mentions things which did not happen, is quite consistent in saying nothing about them.

But what about Suidas? It seems to be generally agreed that he lived about A.D. 1000—considerably closer to Macan and Munro than to Marathon. Great as is his apparent honesty, is it really conceivable that a version of Marathon so entirely different from that of Herodotus can have been a genuine version which blushed unseen not only by Nepos, Trogus, Pausanias and their sources, but also by the well-read author of the *de malignitate Herodoti* who is at special pains to find fault with Herodotus' account of Marathon? That there was a proverb *χωρὶς ἰππέης* is of course probable. But it is a not very likely or is at any rate a very pointless proverb for fifth-century Greece, in whose states cavalry existed not at all or in very small numbers and was quite ineffective in battle. On the other hand, it was a very natural proverb in the years after the battle of Adrianople, when cavalry came into its own. But once the proverb existed its attribution to Marathon was not unnatural. Marathon became *the* ancient battle, an earlier Hastings; and any intelligent reader of Herodotus' account in the centuries following A.D. 378 would, judging by the warfare of his own day, be surprised at the apparent inactivity of the cavalry, just as we are today until we study the warfare of the period. But, argues Macan, the circumstantial detail in Suidas goes to prove the genuineness of his statement. I cannot help feeling that the circumstantial detail, though it may support the origin of the proverb in some definite incident, is only one more argument against its having any connection with Marathon. Ionians, says Suidas—and it is surprising to find Ionians in a picked force so soon after the revolt; still, they might have been sailors—Ionians climbed up into trees and signalled to the Athenians that the cavalry were away. Now to me this is merely incredible. Did they use Morse or Semaphore? In the Peloponnesian War there is a good deal of evidence for the use of simple signals, probably of a pre-arranged type. But this implies a definite alphabet. That the Athenians and Ionians had such a thing which they could mutually understand and which could be employed from trees and read at a distance before the discovery of telescopes is more than I can swallow. And why should it be necessary? The embarkation of horses is not only a noisy business, but must have been considerably more visible at any rate than an Ionian up a tree. I mention all this not very seriously but simply to show what the accepting of Suidas involves, especially when he is further strained to support modern theories. For Suidas says nothing about ships; at least I can find nothing though I read in one recent work, 'The fact that the cavalry were embarked (Suidas) is the strongest evidence that the move on Athens was to be made by sea'.

If Suidas supported anything at all it would be the absence of the cavalry foraging or its employment on some such tactical turning movement as that of Hydarnes at Thermopylae. But there really is no evidence at all in favour of this embarking of the cavalry. It is in itself most unlikely, and I cannot see any serious difficulty in supposing that the cavalry was present at the battle.

(2) The second problem—the delay before the battle and the reasons for its finally taking place—cannot be dealt with so precisely, because there is not the same agreement among modern writers. All seem to see a need for some ingenious explanation of the battle, but some (Grundy, for instance) are not worried by the delay. I will therefore deal with the delay very shortly. If the Greeks were going to attack, it is asked, why delay when there was fear of treachery? If the Persians were going to attack, why delay

when there was a chance of Spartan reinforcements to the enemy? I can only reply that there has been delay before half the battles in history. Opposing armies are always extremely unlikely to delay while they make preparations (disembark stores, for instance), conduct inquiries or hope that the other side will make a mistake. Delay is especially likely if, as probably at Marathon, the smaller army is the more strongly posted. Compare the delays at Thermopylae and Plataea which have also caused what seems to me unnecessary worry. Here at Marathon was the added reason that one side expected reinforcements and the other treachery (for that like so many Sherlock Holmes arguments can be used both ways). The two forces were differently armed and had no experience of one another. The warfare of the period made a battle unlikely until both sides wanted it. Compare the numerous occasions in ancient history in which one army 'offers' battle: for instance Caesar's in the days preceding Pharsalus. More modern parallels are equally numerous, but I will only mention one. The manœuvres of Cromwell and Leslie which culminated in the battle of Dunbar afford a parallel with Marathon which superficially (compared for instance with the battle of the Marne), is quite extraordinarily close. I do not want to deduce any reconstructive detail from the parallel: there were of course great differences. But it is legitimate to use it as an illustration that delay in war is quite usual.

What then led to a fight? Two opposing armies constantly delay. On the other hand there is almost sure to be a fight before long. A situation such as that at Marathon produces a gradually increasing tendency to collision. Hunger, cold, ambition, mere impatience, and above all misjudgement—all have their influence. There is very likely no sufficient military reason. The history of war is very largely the history of generals selecting the wrong moment for attack. Why did Pompey finally accept battle at Pharsalus? Why did Leslie finally come down into the plain at Dunbar? The latter, at any rate, is usually recognised to have made a blunder; and Cromwell's 'The Lord has delivered them into our hands' is a parallel to the Persian 'Look at these fools coming on without cavalry and archers', with just this difference—that Cromwell was right and the Persians wrong.

It also seems to me that the expression 'taking the offensive' is often used rather wildly. It is often hard to say which side did take the offensive. At Pharsalus Caesar was trying to bring on a battle for some days, but the actual day was fixed by Pompey drawing up his army for battle. Then in the battle itself the tactical offensive was taken by Caesar. At Dunbar the strategical offensive was taken by Leslie when he came down to the plain, but the tactical offensive by Lambert's cavalry. Wellington's usual tactics in Spain can only be described as defensive-offensive. At Marathon the tactical offensive was, I think, certainly taken by the Athenians; but from Herodotus' account it seems extremely likely, as has been pointed out by others, that the Persians challenged them by drawing up their army and offering battle; just as the Peloponnesians did, though without success, outside Athens during their first invasion of Attica.

But I do not pretend to be able to interpret exactly what passed through the Athenian generals' minds. I do, however, insist that there is no justification for the way Marathon has been discussed as though it were very peculiar in these matters.

Finally, just one or two remarks about Munro's theory. Munro, I should say, never claims that it is more than a theory. 'The theory put forward', he says, 'does not contradict any well-accredited fact in the evidence, nor involve imaginary causes.' I cannot subscribe to that, but it is the more recent tendency to regard his theory as final which makes a further reference to it imperative. I have already dealt with the cavalry problem. For the supposed division of the infantry the additional evidence is so slight that there is hardly anything to argue about. Everything in Nepos' account, especially the numbers, seems to me palpably unreliable. The suggestion that the shield signal, which according

to Herodotus was only shown when the Persians were in their ships, was really when half of them were in their ships, still leaves the same difficulty that the Persians took action before the signal. If their intelligence service was so good that they knew that the signal was imminent, as Munro suggests, why was it necessary to employ so cumbrous a method of sending the final information? With what we now know of the value of tales about signalling to German submarines, I think we may well give up all hope of fixing the exact place of the shield signal in the story. An at first sight stronger argument of Munro is that the landing at Marathon must have been a feint to lure the Athenians from Athens. 'Why burden themselves', he says, 'with a march of five and twenty miles through the enemy's country when their fleet might have put them at once within striking distance of the city?' How and Wells put it more strongly still. 'Nor is it likely that the Persian leaders doubted their power to force a landing on the open coast near Phalerum.' But the whole of military history, ancient and modern, shows that landings on foreign soil are generally made, if possible, away from the defending army. The Athenians, it is true, landed near Syracuse, but they first drew the Syracusans away by a fictitious message (they did not divide their force), for, says Thucydides,²⁷ 'They knew that they would fail of their purpose if they tried to disembark their men in the face of an enemy who was prepared to meet them'. Compare the great difficulty Caesar has in landing in Britain, where he fails to secure an unopposed landing. Contrast his easy landing in Epirus. But there is no need to go far afield for parallels. Immediately before Marathon the Persians attacked Eretria. In this case they landed at three different places, one of which, Tamynae, was some fifteen miles distant from Eretria.²⁸ Why? Did they doubt their ability to land closer or were they intending to lure the Eretrian army out of Eretria? I cannot find that any modern writer has noticed this simple parallel. I suggest that the landing at Marathon was the most natural thing in the world and was due to a combination of simple causes. It was a good landing-place, fairly near Eretria and more or less on the direct route to Athens, where the Persians could disembark their cavalry and stores undisturbed. As I have said before, the ancients used ships as little as possible when they could use land, and to land at Phalerum would not only involve a longish sea voyage round a promontory, but would at the very least have been an extremely difficult operation in the face of the Athenian army. But, says Munro, if they intended to march on Athens from Marathon the Persian generals had quite time enough to send an advanced guard to occupy the passes. How and Wells are again more positive. 'The idea that the Persians intended to march from Marathon on Athens is decisively negated by the fact that they made no attempt to seize the passes leading from the plain of Marathon towards Athens.' As so often it is here argued that a general cannot make a mistake, though history is crowded with instances of omissions by generals to take obvious precautions. And what does 'occupy the passes' mean? There is no Thermopylae between Athens and Marathon. Even if there had been, the detaching of a small body of Persians would not have denied it to the Athenians. The language of an age of machine guns is inapplicable to an age of bows and arrows. To occupy the passes the Persians would have had to detach several bodies each of which would have had to hold a position which could easily be forced or turned by superior numbers. It would have been a mere waste of men. Nor, even if successful, would it have secured the Persians an approach to Athens unless they occupied the Athenian end of the passes. To send scouts out would be reasonable; they may have done it, but if so the fact is recorded neither here nor on most other occasions in ancient warfare when the sending out of scouts seems reasonable. There are very few references in Greek historians to scouting, still fewer to the more solid protective duties, especially to the holding of advanced positions by small outposts.

²⁷ vi 64. 1.

²⁸ Hdt. vi 101. 1.

To me it seems much more likely that the Persians had the surprise of their lives when the Athenians came out to Marathon. What the Persians finally did, or would have done, when thus cornered I do not know. They might have tried to re-embark their whole force, they might have tried to force their way through to Athens. Their position was a very difficult one. The Athenians finally accepted a challenge to battle. That at least seems most likely from Herodotus and is intrinsically most probable. If anything more elaborate happened we can only say that the means of discovering what it was are irretrievably lost.

Yet I do not wish to argue that there are no unsolved problems connected with Marathon. The importance to be attached to the possibility of treachery is a great problem which I do not pretend to solve. I would only point out that it is unlikely that, as seems often to be supposed, all the loyal men came to Marathon while the traitors were graciously excused the levy and allowed to remain behind. Nor have I attempted to meet all the arguments raised in favour of modern theories. If it is urged that their strength is in their cumulative effect I will try to deal with others during the discussion. I feel that I have said enough to make clear my point that though we cannot hope to reconstruct all the detail, Herodotus' account may not be quite so inadequate when judged in the light of what we know of contemporary warfare as it is if we take it apart from its context, and that where the evidence fails it is better to admit the fact. As Strachan-Davidson writes of German pamphlets in the introduction to his Polybius: 'I find myself in agreement with each of them in turn as each upon one point or another is content to accept the plain statements of Polybius or to draw obvious common-sense inferences from his language. Beyond this, unless I am mistaken, little has been discovered or can be discovered of sufficient certainty to justify us in receiving it as history. Of most of the propositions advanced I feel myself compelled to repeat "It is probable and the contrary is also probable"'. When, as is too often the case, the theory leads along an elaborate series of deductions into direct contradiction with a statement of Polybius, a more decisive verdict may be pronounced upon it.'

N. WHATLEY.

Oxford.