BATTLES, ANCIENT AND MODERN.

PROFESSOR BURROWS ON SPHAKTERIA.

IN the last number of the *Hellenic Journal* Professor Burrows has once more attacked Pylos and Sphakteria, this time with the aid of certain allies from the Peloponnese and a whole battery of photographs. I am, I confess, loth to dye this ink-stained field a deeper hue, and I have certainly no intention of sending friends of mine to Pylos with a view to reviving a controversy which has gone far enough. At the same time I do not wish to appear to undervalue the evidence of Messrs. Carr Bosanquet, Crowfoot, and Lindsay, though it is not of a very decisive character. I should therefore like to point out, as briefly as possible, its true bearing, and in doing so I have the satisfaction of not having to treat it as hostile, though brought into court by the other side.

It is rather difficult to deal with Mr. Lindsay's evidence, as given by Professor Burrows, relating to the south end of Pylos. Had we had Mr. Lindsay's first-hand account of the matter, it would no doubt have been easier to understand. I do not understand it sufficiently in its present form to attempt to discuss it in detail. I see, however, that Professor Burrows now not only imagines a sand-spit at the south-east corner of Pylos, but even makes Demosthenes draw up his ships there; and further, refers to this imaginary sand-spit [against whose existence the physiographical evidence at present obtainable is very strong] the expression ἀποβάσεως δὲ μάλιστα οὔσης. How could these ships have escaped destruction in such a position, when the Peloponnesian fleet was in undisturbed possession of the harbour? Surely the fact that these ships remained undestroyed shows clearly that they were drawn up at some point where landing was *not* easy, *i.e.* at the south-west corner; and the fact that they were outside the wall, shows that the latter must have been back from the shore at this point.

The photographs which are appended to the article justify to an eminent degree the distrust with which I have always regarded such aids to topography. If it be merely a question of the appearance of an object which can be taken at a distance of a few yards, then, no doubt, the picture is of value, but to found arguments dealing with minute detail on such photographs as those numbered Fig. 1 and Fig. 4 in Prof. Burrows' article is simply misleading. Fig. 1 (of the south-east angle of Pylos) has at first sight all the

appearances of a photograph of a gradual slope. It is not until you examine it closely that you see how steep the slope is. No photograph taken from the front of a slope can ever give anyone who has not seen it a true appreciation of its angle. The foot of the slope appears relatively much nearer to the eye than the top of it, and consequently the slope itself appears much more gradual than it really is. The angle is, as a fact, 36° . Moreover I have used the term 'slope' for want of a better word. It is really in parts precipitous, and perhaps the word 'cliff' may be taken as more nearly describing it. Can Professor Burrows quote any example from the fifth century of a Greek force attempting, in face of opposition, an assault on such a slope, or on anything resembling it? Fig. 4, which gives the same in profile from above, exaggerates somewhat in the opposite direction, and as it is out of focus the detail is lost in a distance. Still either photograph will justify, in the eyes of those who are not accustomed to imagine the Greek hoplite as making his way over ground which a deer stalker would with difficulty climb, my assertion that the practicable approach at the south-east angle of Pylos was so narrow that, even if landing there had been easy, which does not seem to have been the case, a few men could have defended it against immensely superior numbers, and a wall was neither required nor built by Demosthenes there.¹ The existence of the Venetian tower and wall at this point dates, of course, from times when the sand-bar had reached the foot of the cliff, and therefore affords no argument for the necessities of the case 2,000 years before.

The value of photographs in topography depends, as I have said, altogether on the use which you are content to make of them. They are of value in giving those who have not seen ground a general impression as to its appearance. For minuter details, unless taken of objects a few yards from the camera, they are absolutely misleading to those who have not first-hand knowledge of the ground they represent.

The question of the blocking of the channels is dependent on a far larger question, the condition of the lagoon at the time the events on Pylos took place. I have expressed my views with sufficient clearness on this point in another article in this number of the *Journal*. I have not the slightest doubt that Thukydides had very good grounds for his repeated assertion with reference to the intended blocking. The difficulty to him was, and to us is, that he failed to recognize the existence of two sheets of water, both of which

¹ Prof. Burrows on p. 149 of his article has quoted a long passage from a previous paper of mine, in which occur the words: 'at this south end of the east cliff, the summit of the cliff rises to a vertical height of sixty feet above its eastern foot, which is only at a horizontal distance of eighty-one feet from that summit.' Professor Burrows disposes of this remark by an appeal to the photograph, and says 'Comment is needless.' The effect of this criticism is unfortunately negatived by the fact that

though he has quoted my actual words, he has misread them. He says the photograph shows in profile the slope to which I refer. It does not. It shows in profile the slope to the southern, not the eastern foot. To this southern slope I referred in the *Classical Review* of Nov. 1896 in the words 'The cliff is sixty feet high within fifty yards of the Sikia.' But it is the east slope which is of importance to the question between us. As to comment we are in agreement.

he designated as 'the harbour.' The vagueness of his assertions on this point is all in accord with the caution of a cautious man who is not quite clear as to his information.

Mr. Lindsay's evidence with regard to the position of the north wall is, considering the fact that he had the opportunity of ascertaining in detail Prof. Burrows' views before he went to Pylos, and went there, apparently, at the latter's request, not unfavourable to the views I have held. As to the fifth century (*sic*) wall, Mr. Carr Bosanquet speaks with the characteristic caution of an experienced archaeologist. He says 'it may well be rough fifth century work.' Of course it may be, but is it in the slightest degree likely that it is? It may just as equally well be the work of 5, 10, 15, or 20 centuries later. There is no special characteristic to determine its age. The Greek and the Cumberland shepherds at the present day build the same kind of wall (v. Figs. 6 and 7, in Prof. Burrows' article) in constructing a sheep shelter; so would anyone else constructing a wall with similar material. The survival of such a wall on a site which has had such a subsequent history as that of Pylos is so improbable as to render identification valueless for historical purposes.

I doubt whether anything can be gained by attempting to determine the date of this wall. Though the importance of Pylos is at present almost nil, it stood to the Venetian trade with the East in mediaeval and modern times in much the same relation that Gibraltar does to our own Eastern trade. It was the scene of repeated attack and defence, and even so recently as 1825 was maintained by the insurgent Greeks for six weeks against the assaults of Ibrahim Pasha's force. On ground such as this, which does not afford soil for entrenchment, the rough stone wall is the only form of defence possible.

The opinion of the professional architects of the French expedition, quoted by Mr. Bosanquet on p. 155 of the *J.H.S.* Vol. xviii. must surely be of greater weight than that of the archaeologist, M. Bory de St. Vincent. Archaeologists are not exempt from the unfortunate tendency to believe what they wish to believe.

Of the identity of the site of the *παλαιὸν ἔρμα* there can be no doubt. I am in full agreement with Professor Burrows on this point. I was also inclined to think that he had found traces of the work above ground which I myself had missed. His original account of the remains he had discovered led me to believe that they possessed definite characteristics pointing to an early origin. But if these photographs, Figs. 2, 8, 9, in Professor Burrows' article, and the print Fig. 11 in the text convey a correct impression, these wall-remains present no such definite characteristics. That marked Fig. 9 I have not seen. Those marked Figs 2 and 8 I have. I have never seen walls elsewhere in Greece which at all resemble them, except such as are notoriously modern (*e.g.* the remnants of the breastworks erected by the Greeks at the siege of Tripolitza), roughly put together of the material lying at hand. Nor must it be imagined that this site has not passed through vicissitudes in mediaeval and modern times which must

have led to the construction and reconstruction of numerous rough walls upon it.

The north end of Sphakteria is the key to Pylos and Navarino, as Hussein Djeritli pointed out to Ibrahim Pasha. Apart from the question of artillery fire—and it was here that the Egyptians established their batteries in 1825 in the attack on Pylos—it was an essential *point d'appui* for any force attacking this peninsula, affording as it did a bird's-eye view over three-quarters of it, and what more natural than that such a force should guard the summit with a rough wall, since the soil for entrenchment is conspicuous by its absence. Compare, too, the wall of Fig. 2 alleged to have been built by the Messenians with the so-called fifth century wall of Fig. 6. The latter is more perfect than the former, but they are the same in character. Either might belong to any age. Mr. Crowfoot's careful plan tends more towards conviction, but presents this difficulty. How is it that the wall-foundations in the hollow show not merely a want of connection but, in the case of the northern one, a manifest disconnection with the fortification of the summit? The exigencies of the ground, if Mr. Crowfoot's plan be, as it seems to be, absolutely correct, demand no such thing. And yet it might be supposed that people who were, as the plan shows, acquainted with the use of flanking towers, would not have constructed their work in this form. In fact it does not seem as if the wall on the summit and the walls in the hollow were part of the same design. If so, which is the remnant of the *παλαιὸν ἔργον*, and which is not?

I see that Mr. Lindsay, in deference, no doubt, to Thukydides, suggests an improvement on Professor Burrows' design of taking the Messenian captain and his men in boats to the bottom of the cliff beneath the summit of Sphakteria. The remodelled theory rests on the insecure foothold of an imaginary path from the Panagia to that point. How insecure the foothold is will be seen by reference to the Admiralty chart, where the cliff is shown as going down sheer into a depth of between thirty and sixty feet of water for three-quarters of the distance between the Panagia and the bottom of Professor Burrows' chimney.

MR. WOODHOUSE ON PLATAEA.

Those who are interested in Greek history should certainly read Mr. Woodhouse's able article on Plataea. I think he exaggerates the differences between our views, though absolute concord on so complicated a question is not to be expected. He also seems to forget that I had to take up the work of inquiry practically *ab initio* and to construct a practical basis on which to found an explanation of the battle.

The points of difference between us are (1) as to the position of the Gargaphia spring, (2) as to the site of the Heroon of Androkrates, (3) as to the identification of stream A 1.

(1) *Gargaphia*.—That the identification proposed by Leake and myself is

capable of dispute, I admit: but Mr. Woodhouse's main arguments against it appear to me to be badly founded.

(a) He, identifying it with the Apotripi Spring, says that the Gargaphia of Leake is nearer the 'Island' than the Apotripi. The distance is almost exactly the same.

(b) He further says that the Gargaphia was by implication 10 stades from the R. Moloeis and the temple of Eleusinian Demeter. He evidently assumes that it was *within* the second position of the Greeks, and that, as they moved 10 stades before arriving at the Moloeis, etc., the spring must have been 10 stades from the latter.

It seems to me, however, that the detail of the taking of the Spring by the Persian Cavalry, *while the Greeks were still in the second position*, shows pretty clearly that the spring was not within that position, though, of course it must have been near it. The position was the top of the Asopus ridge: the spring was at the bottom of it.

(2) *The Heroon of Androkrates.*

Mr. Woodhouse would place it on the Asopus ridge, on the site of the Church of St. John. I cannot see how Thukydides' language can be so interpreted. He says, iii. 24, 'The Plataeans started from the ditch and took in a body the road leading to Thebes, having on their right the Heroon of Androkrates, thinking the beseigers would be less likely to suspect their having taken the road towards the enemy's country, and seeing, too, the Peloponnesians with torches going in pursuit along the way towards Kithairon and Dryos Kephalai which leads to Athens. For 6 or 7 stades the Plataeans went along the Thebes road, and then, turning, took the way leading towards Erythrai for Hysiai, and having taken to the hills, escaped to Athens.'

All the ways mentioned can be identified with ease, and are, in two of the three cases, existent.

The fugitives took the Thebes road, which must have been more or less identical in line with the present route from Kokla, which stands just above the ruins of Plataea, to Thebes, having on their right the Heroon. After going 6 or 7 stades they turned, evidently with intent to reach the trackway which leads from Thespiæ via Pyrgos to the pass of Dryos Kephalai and the sites of Hysiai and Erythrai, and, without ascending to the pass, took to that high bastion of Kithairon which projects into the plain east of Erythrai. It seems to me that Thukydides implies that the Heroon was in the angle through which they turned, and I am inclined to think that the remnants of it will be found at one of the two stone heaps marked on my map, about half a mile north-east of the remains of the 'Acropolis' of Plataea. They must have left the site of the Church of St. John far away, not on the right, but on the left.

(3) *Al and the Asopus.*

In a note at the end of his article Mr. Woodhouse repeats the charge which Dr. Frazer makes in his Pausanias. He accuses me of inventing this identification of Al with the Asopus with a view to the subsequent identifi-

cation of the 'Island.' This charge is utterly unwarranted by what I have said in my original paper, and I may add that at the time the difficulty with regard to Herodotus' use of the name 'Asopus' and its solution occurred to me, I had not identified the 'Island,' which came in the very last part of my survey. Dr. Frazer has withdrawn the charge in a letter which he has given me leave to publish, if I wish, wherein he admits that what I have said affords no justification for it.

There is in the article much beside what I have mentioned, but it is matter which I should not like to discuss without further consideration. I shall certainly take it very seriously into account in dealing with the wars of the 5th century in book form, as I hope shortly to do.

G. B. GRUNDY.