

for those "whisps of tuz-leaves", as Masson terms them (*Ariana Antiqua*, pp. 60, 84, 94, 116), meaning of birch-bark, the scanty fragments of which are our earliest remains of Indian manuscripts.

F. W. THOMAS.

#### THE TAXILA INSCRIPTION OF THE YEAR 136

By the courtesy of Dr. Marshall I have received in advance a proof of his paper on "The Date of Kanishka" (p. 973 above) and an invitation to write a note on it. It will be a great relief when this matter can be settled, in any direction. But I can only say for the present that Dr. Marshall's case is not at all as plain to me as it is to him. His argument based on his discoveries at the Chir Stūpa site depends on views about art; and that is a difficult subject, there being so great a divergence of opinion among the authorities in that line. His argument based on his discoveries at the Sir-kap site is the *argumentum ex silentio*, which, however suggestive it may be, cannot be taken safely as conclusive. A pointed illustration of that is on record in connection with this same topic: in 1904 and again in 1908 it was asserted confidently that there could not be a Kushān king Vāsishka, either between Kanishka and Huvishka or after the latter, because no coins of him had been found. But, though the fact still remains that no coins of Vāsishka have been found or at any rate recognized, in 1910 there was brought to light an inscription which proves undeniably that there was such a king between Kanishka and Huvishka.

The real interest of Dr. Marshall's paper lies, for me, in the new inscription of the year 136 which he has discovered near the Chir Stūpa. This is another record in the north-west dialect and the Kharōshthī characters. One point of interest in it is (see p. 1009 below) that it endorses so clearly the other evidence that the name of

the race to which Kanishka and his connections belonged was *Kushāna*, *Kushān*, and presents the variant of that name, *Khushāna*, *Khushān* (with *kh* instead of *k*), which we know otherwise only from coins of Kadphisēs I. Another is that, like the Panjtār inscription of the year 122, = A.D. 65 (see pp. 372 above and 1002 below), it mentions a *Kushān* king without stating his name.<sup>1</sup> But the chief point in it is the way in which it is dated.

According to Dr. Marshall's figuring of the record it is dated thus:—

sa 136 ayasa ashāḍasa masasa divase 15.<sup>2</sup>

He takes *ayasa* as the genitive singular of a name *Aya*, for which he substitutes *Azes* because there are well-known coins of two kings whose name is presented as *Aya* in the *Kharōshthī* legends on them, and as *Azēs* in the Greek legends. And he gives the following translation:—

“in the year 136 of *Azes*, on the 15th day of the month of *Āshāḍha*.”

He takes this record as being dated in the year 136 of an era founded by *Aya-Azēs* I (p. 976). He puts the beginning of the reign of *Azēs* I and of his era close enough to B.C. 58 to allow us, without necessarily identifying this era with the *Vikrama* era, to take that year itself for practical purposes.<sup>3</sup> And he thus places the record in A.D. 79 (p. 977).

<sup>1</sup> Dr. Marshall has quoted me (p. 977) as identifying the king of the Panjtār record with *Wēmo-Kadphisēs*. I said that he may be either *Wēmo-Kadphisēs* or *Sōtēr Megas*, “the Nameless King” (this *Journal*, 1913, p. 1011). Dr. Marshall seems to prefer to identify him with *Kozoulo-Kadphisēs*, who was reigning, according to him, in A.D. 65 and 79. It is only a question of how the date may be found to fit in best.

<sup>2</sup> For the sake of simplicity I substitute 136 for the 100 20 10 4 1 1 of the original, and 15 for 10 4 1.

<sup>3</sup> This idea, that there were two eras, beginning in and about B.C. 58, is not new: it is part and parcel of an almost unintelligible desire to try to interpret the early Indian records by means of any reckonings rather than the well-established Indian ones.

Now, the immediate predecessor of Azēs I (p. 979) was a king whose name was Moa according to the Kharōshthī legends on his coins and Mauēs according to the Greek legends on them. Dr. Marshall follows "the most orthodox view" (p. 984), which identifies with Moa-Mauēs a king whose name is presented as Moga in the record on the "Patika copper-plate", that is, the Taxila plate which is dated in "the year 78" of some reckoning which it does not specify.<sup>1</sup> He puts the initial date of Moa-Mauēs in or about B.C. 95 (p. 986). He takes the year 78 of the Taxila plate as the year 78 of the reign [? of an era] of Moa-Mauēs, beginning in B.C. 95.<sup>2</sup> And he thus places this last-mentioned record in B.C. 17.

The position in which this combination lands us is as follows:—In B.C. 95 there began an era of Mauēs, which was still in use in B.C. 17: nevertheless, Azēs I, the successor of Mauēs, started in B.C. 58 a new era, which was still in use in A.D. 79: and so the two eras, in use by the same dynasty, ran on alongside of each other for at any rate forty-one years, down to B.C. 17. Comment on this seems unnecessary.

---

<sup>1</sup> I do not share in this "most orthodox view": see my remarks in this *Journal*, 1907, p. 1023; 1913, p. 1000; and p. 797 above. The grounds put forward for the identification of the two persons are two:—(1) That, if it is not made, then Moa-Mauēs has no inscriptions and Moga has no coins. A weaker reason than this could hardly be thought of. (2) That the word Moga is merely a dialectical variant of Moa. This is based on the point that a medial single *g* between two vowels was liable, like other single consonants so standing, to disappear. But not by any means has every such *g* disappeared. And in any circumstances, identity of name does not establish identity of person. Further, if Moa-Mauēs was also known as Moga, then how is it that the variant Moga has not been found on any of his coins, which are of at least sixteen different types? Also, it is to be borne in mind, as I have said on a previous occasion, that the form Moga presented in the record on the Taxila plate may quite possibly stand for Mogga (with the double *gg*), which could not possibly be even the same name with Moa.

<sup>2</sup> His actual expression is the "reign" of Mauēs (p. 986). But, as he only assigns thirty-seven years (B.C. 95–58) for Mauēs before Azēs I, I infer that he must mean an "era" of Mauēs.

Now, the record on the Taxila copper-plate is dated thus:—

samvachharaye athasatatimae 78 maharayasa maham-  
tasa Mogasa Panemasa masasa divase pamchame 5.<sup>1</sup>

Dr. Marshall has said quite rightly that the words *mogasa* and *ayasa* have the same relative position in the two records. But his interpretation of the passages, taking one as meaning “in the year 78 of [the era of] Moga,” and the other as meaning “in the year 136 of [the era of] Aya,” is wrong.

As I pointed out long ago in respect of the Taxila plate,<sup>2</sup> the word *mogasa* is dependent, not on what stands before it but on what follows it, and the record says:—

“In the seventy-eighth year, 78: on the fifth day, 5, of the month Panēma of the great king, the great one, Moga.”

In just the same way, in this new inscription the word *ayasa* (if, for the present, we accept the word itself and the meaning given to it by Dr. Marshall) is dependent on what comes after it, not on what is before it; and what the record says is:—

“(In) the year 136: on the day 15 of the month Āshādha of Aya.”

Even if the words *mogasa* and *ayasa* stood before the statement of the years, so that the translations would actually be “in the year 78 of Moga” and “(in) the year 136 of Aya,” these records would still, on the analogy of every known early Indian record, place Moga in the year 78 and Aya in the year 136 of some era or eras not founded by them. Compare on this point the words of Professor Bühler, who edited the Taxila plate: he said:<sup>3</sup>—

<sup>1</sup> Here, again, for the sake of simplicity I substitute 78 for the 20 20 20 10 4 4 of the original, and 5 for 4 1.

<sup>2</sup> This Journal, 1907, p. 1014.

<sup>3</sup> *Epi. Ind.*, vol. 4, p. 56.

"The year 78 is, of course, not that of the reign of Mōga, but of the era which he used." Compare also records in which the name of the king actually does stand before the year: we have such instances as:—(1) "In the year 51 of the great king, the son of the gods, Huveshka;"<sup>1</sup>—(2) "(In) the year 80 of the great king Vāsudēva;"<sup>2</sup>—(3) "In the seventy-second year, 72, of the king, the Mahākshatrapa, Rudravarman;"<sup>3</sup>—(4) "In the ninety-sixth augmenting-victory-and-reign-year of the great king of kings, the glorious Kumāragupta".<sup>4</sup> No one has ever suggested either that any of these kings, Huvishka, Vāsudēva, Rudravarman, and Kumāragupta, founded an era, or even that they reigned for 51, 80, 72, and 96 years: the statements simply place the records containing them in their reigns, and in respectively the years 51, 80, 72, and 96 of the eras used by them; and only the same interpretation would be justifiable in respect of the two Taxila records, even if the words in them stood in that same order. It may be added that the Hindūs never connected an individual name with any of their reckonings until a quite late time (the tenth century, as far as we can trace this detail), when they invented first king Vikramāditya and then king Śālivāhana as the founders of the eras of B.C. 58 and A.D. 78.

The idea that the Taxila plate of the year 78 indicates an era of Moga was first propounded (as far as I can find) in 1886 by Gardner,<sup>5</sup> an expert in coins but not an epigraphist. It was taken over in 1890 by Cunningham,<sup>6</sup> who can hardly be classed as a skilled epigraphist. And in circumstances which are beyond my comprehension

<sup>1</sup> Lüders, *List of the Brāhmī Inscriptions, Epi. Ind.*, vol. 10, appendix, No. 52.

<sup>2</sup> *Ibid.*, No. 66.

<sup>3</sup> *Ibid.*, No. 965.

<sup>4</sup> Kielhorn, *List of the Inscriptions of Northern India, Epi. Ind.*, vol. 5, appendix, No. 439.

<sup>5</sup> *Coins of the Greek and Scythic Kings of Bactria and India*, introd., p. 49.

<sup>6</sup> *Coins of the Sakas*, p. 22.

it was accepted by Bhagwanlal Indraji, as edited in 1894.<sup>1</sup> But, with the exception of the last-mentioned scholar, I do not know of anyone, ranking as an authority on inscriptions, who has endorsed it: and Bühler's words, written in 1895 or 1896 and quoted by me above, were of course a protest against it. It is most unfortunate that it should have been revived now, in propounding a similar idea in connection with Azēs I on an equally illusory basis.

---

My preceding remarks are made on the understanding that the word next after the year in this new record of the year 136 really is *ayasa* and means "of Aya". Even if that is the case, the record does not speak of an era of Aya, but distinctly places this Aya in the year 136 of an unspecified era founded by someone else, and tends, in fact, to mark him as the Kushān king to whom it refers in line 3.

But is that the proper treatment of the word at all? I doubt it. And Dr. Marshall himself has pointed out (p. 976) a very sound objection, namely, "the absence of any royal titles attached to the name of Azes." From the vast mass of inscriptional material which is now available I cannot quote a single record in which the name of a real king,<sup>2</sup> whether living or dead at the time of the record, —or even of any official,— is mentioned in such a connection without some title or another. And for this reason, if for no other, I am of opinion that the word *ayasa* does not give a proper name.

What would stand most naturally here is a genitive singular in apposition to the immediately following word *ashaḍasa*. But even such a word is not found in *ayasa*: and we must consider now what may be the original word

<sup>1</sup> This Journal, 1894, p. 553.

<sup>2</sup> I mean, of course, excluding the fictitious Vikramāditya and Śālivāhana.

which could be presented to us wrongly under this guise in the figuring.<sup>1</sup>

By the very simple alteration of Dr. Marshall's 7, *a*, to 2, *e*, —(done by merely finding one more punctured dot and making a connecting line to it from the bottom of the *a*),— we could get the word *eyasa*, = *ētasya*, “of this”.<sup>2</sup> But the resulting sense, “of this month Āshāḍha”, hardly seems appropriate, even if the form is admissible (which seems doubtful) for the dialect of this record.

Now, at the time of this record, —in A.D. 79–80 according to Dr. Marshall's opinion and my own; and some three centuries before the introduction of the Greek astronomy,— the Indian calendar was regulated by mean or uniform instead of true time. The intercalation of months was governed by a hard and fast rule. According to the Jyōtisha-Vēdāṅga the fixed intercalated months (one half-way through the five-years cycle, and the other at the end of it) came next after Āshāḍha and Pausha, and so would be a second Āshāḍha and a second Pausha. And this, in my opinion, very possibly explains the record.

Without stating any reasons for the question, I asked Sir George Grierson to tell me what would be, in his opinion, the north-west form of the Sanskrit *dvitīya*,

<sup>1</sup> I am not to be taken as depreciating in any way Dr. Marshall's figuring of the record: it is an admirable piece of work. But we have to bear in mind (1) that, the dimensions of the scroll being 6½" by 1½", we have before us a full-size figuring, showing a quite small original lettering: (2) that, as Dr. Marshall has told us, the figuring is not a mechanical facsimile, but is the result of drawing by hand: (3) that on the original scroll the lettering was done, not by continuous strokes as in the figuring of it, but in punctured dots: and (4) that the lettering was covered by a metallic efflorescence which had to be removed by cleaning as far as possible before it could be deciphered at all. In such circumstances it is hardly possible that the reading and figuring can be absolutely correct all through: and there are, in fact, places in which the figuring is certainly wrong, unless the original itself has mistakes.

<sup>2</sup> Pischel, *Grammatik der Prākṛit-Sprachen*, § 426.

'second', for a Kharōshthī record of the first century A.D.; inviting his attention, at the same time, to the Prakṛit forms *bia*, *bīa*, *bīya*,<sup>1</sup> and asking whether *bīya*, *vīya*, would be admissible. His answer is:—"Judging from the modern North-West languages, the North-West Prakrit for *dvitīya* must have been *vviya* or *vvīya*, or something of that sort. Cf. Sindhī *bbiyō* or *bbījō*, 'second', Lahndā *bbēyā*, 'another'; Kāshmirī *biya*, 'another'. . . . Sindhī often preserves words in forms older than those found in other North-Western languages." Since a double *vv* would of course be written in Kharōshthī by the single *v*, this gives exactly the word that is wanted, *viyasa*: and it is got by simply altering Dr. Marshall's 7, *a*, into 7, *vi* (by finding two more dots and making the connecting lines); which is quite justifiable in view of the nature of the original lettering and the figuring of it. And this may quite well be the solution of the matter; namely, the text should be taken as:—

sa 136 viyasa ashaḍasa masasa divase 15:

"(in) the year 136: on the day 15 of the month the second (i.e. intercalated) Āshāḍha."

It is to be hoped that Dr. Marshall may see his way to giving us, by some means or another, an exact facsimile of at any rate that piece of the scroll which contains this particular word. But in any case, whatever may be the ultimate settlement of this detail, the fact stands that this record does not set up an era of Aya (Azēs I). So far as this record and that on the Taxila copper-plate go, there is no evidence, and no sound reason for thinking, that there ever was either an era of Azēs I or an era of Mauēs. The chronological structure built up on the basis of that idea falls at once.

J. F. FLEET.

<sup>1</sup> Pischel, op. cit., §§ 165, 449.