

man, who for 30 years has traversed the lake in every direction and in all weathers, showed me a place where in winter fish abound because the waters are warmer there than anywhere else ; this is evidently the point of emergence of a sub-lacustrine tributary stream.

On the whole these thermometrical observations, incomplete as they are, tend to confirm what the soundings had already demonstrated, to show that the general features of the Lake of Tiberias are those of a shallow lake, the maximum low-water mark of which scarcely exceeds 40 to 50 metres. If there exists opposite the Wady Semakh—at the point indicated by Lortet—an abyss 250 metres in depth, it can only be a narrow shaft with precipitous walls. The question, I repeat, is now clearly stated, and cannot fail to be soon settled.

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THE HÆMATITE WEIGHT, WITH AN INSCRIPTION  
IN ANCIENT SEMITIC CHARACTERS, PURCHASED  
AT SAMARIA IN 1890 BY THOMAS CHAPLIN, ESQ.,  
M.D.

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THE METHODS OF THE HIGHER CRITICISM.

London,

October 20th, 1893.

JUST before leaving Europe, I have had the good fortune to receive a lesson in the methods of that "higher criticism," which we poor Englishmen are told to accept humbly from the Germans.

"Scientific criticism" has long since decided that the Song of Solomon was composed several centuries after the date to which it lays claim, and one of the proofs of its lateness is found in the little word *shel* "of." This, it has been revealed to the critics, had no existence in Hebrew before the Exile. Three years ago, however, Dr. Chaplin, when visiting the site of Samaria, purchased a small hæmatite weight, which had just been found there, containing an inscription in two lines. The letters are very distinct, and were accordingly read without any difficulty by Dr. Neubauer and myself. I gave the reading in the *Academy*, and Dr. Neubauer published his translation of it elsewhere, of which Professor Driver has subsequently made use.

But unfortunately the word *shel* occurred in it, and as the letters belonged to the seventh or eighth century B.C., this was awkward for the critics. "Scientific criticism," however, soon found a way out of the difficulty. First of all, the genuineness of the object was denied; and when this argument failed, it was asserted that the reading of

Dr. Neubauer and myself was wrong. Stupid Englishmen, who are not "scientific critics," might suppose that the denial and assertion were made after a careful examination of the original object. But such a proceeding is not at all in accordance with the methods of the "higher criticism," and might have inconvenient results for "scientific" theories. So an imperfectly-executed cast was obtained, and those who had seen the original were informed that the cast was much to be preferred to it. As it happens, the part of the weight where the word *shel* is engraved is somewhat worn, and the cast has consequently failed to reproduce all the lines of the letters.

Fortunately, the weight is in the possession of Dr. Chaplin; and as he now resides in England, those who care to do so will have little difficulty in convincing themselves that the reading of the inscription which I have given is correct.

Of course the "scientific critics" will prefer what Professor König in his recently published "Einleitung in das Alte Testament," p. 425, calls the "authentische Nachbildung," and will maintain with him that the same text is repeated in both lines of the inscription. In this way the obnoxious *shel* can be got rid of, and the dogmas of the critics remain intact. Plain people like myself, however, have a foolish preference for facts.

A. H. SAYCE.

Christchurch, Oxford,

October 23rd, 1893.

I am sorry to trouble you; but I cannot refrain from entering a protest against the injustice of Professor Sayce's letter in the *Academy* of last week, on the inscribed weight obtained by Dr. Chaplin on the site of Samaria.

The facts of the case, omitting what is irrelevant, are simple. The inscription in question was read by Professor Sayce in 1890 (*Academy*, August 2, p. 94) as containing the Hebrew particle *shel*, and was referred by him, on account of the form of the characters, to the eighth century B.C. As the use of *shel* at this period harmonised with the early date to which (upon other grounds) I assigned the Song of Songs in my "Introduction" (1891), I mentioned the fact, giving a reference to Professor Sayce's letter in the *Academy*, as well as to one by Dr. Neubauer, which appeared simultaneously in the *Athenæum*. Professor König in his "Einleitung in das Alte Testament" (1893), p. 425, states that he procured an "authentische Nachbildung"—by which, I suppose, he means a cast—of the inscription from the Palestine Exploration Fund in London, which he submitted to the eminent Semitic palæographer, Professor Euting, of Strassburg, who read the inscription differently, and declared that in his opinion it did not contain the particle *shel*. Professor König adds that his own judgment of the inscription agrees with that of Professor Euting.

Upon the strength of these facts, Professor Sayce brings a series of

charges against the "higher criticism"—of prejudice and an obstinate refusal to listen to facts—which I cannot think that the circumstances at all justify. For Professor Euting, who is the chief authority for questioning the reading *shel*, though distinguished for his paleographical knowledge and acquaintance with Semitic inscriptions, is quite unknown as a critic; and of all the men in Germany (or elsewhere) who are "critics," Professor König, as those who have read any of his writings well know, is one of the most honest, exact, and painstaking that could be named, and the very last man to go with the stream, or to adopt a view, unless he had satisfied himself by independent personal investigation that it was adequately borne out by facts.

According to Professor Sayce, however, Professor König, finding the *shel* inconvenient for his theory of the date of the Song (though why he should have done so, seeing that it occurs in Jonah and is common in post-Biblical Hebrew, it is difficult to see), and being addicted to the slovenly methods of the "higher criticism," which has no regard for facts, and is never at the pains to examine original objects, was determined at all costs to get rid of the "obnoxious" word; "so an imperfectly executed cast was obtained, and those who had seen the original were informed that it was much to be preferred to it."

All that is here attributed to Professor König is destitute of foundation in fact. As though either Professor Euting (whom Professor Sayce, strangely, does not mention at all), or Professor König, would work wittingly upon an imperfect copy, or adopt such an unworthy procedure as is attributed to them, for the purpose of evading or suppressing the truth! Even if it be the case (as it very probably is) that the cast used by Professors Euting and König was one which imperfectly represented the original, the blame (if their reading of the inscription should on this ground have been incorrect) rests, surely, not on the two German scholars, but on the authorities of the Palestine Exploration Fund, who supplied them (as they afterwards, I presume, supplied me) with the imperfect facsimile.

I cannot imagine why Professor Sayce could not have written to say simply (if the facts so required it) that the two German scholars had misread the inscription in consequence of their having been supplied with an imperfect copy, instead of gratuitously employing the occasion for indulging in acrimonious taunts and baseless insinuations.

S. R. DRIVER.

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ROSTOCK,

October 26th, 1893.

In der Nr. vom 21 Oct. hat Herr Prof. A. H. Sayce eine Stelle meines Buches "Einleitung in das Alte Testament, mit Einschluss der Apokryphen u. der Pseudepigraphen Alten Testaments" (Bonn, 1893) angegriffen. Die uns gemeinsame Liebe zur geschichtlichen Wahrheit zwingt mich, auch meinerseits zu dieser Sache das Wort zu ergreifen.

Als ich im Sommer 1892 den linguistischen Character des Hohenliedes untersuchte, kam auch ein Gewichtsstück in Betracht, welches Herr Dr. Med. Chaplin in Samaria gekauft hat. Die Aufschrift dieses Gewichtes war in der "Academy" (2 Aug. 1890) veröffentlicht worden. Um ein Urtheil fällen zu können, schrieb ich an Dr. Ad. Neubauer in Oxford, ob er mir nicht eine Nachbildung jenes Gewichtes verschaffen könne. Er rieth mir, dass ich mich an das Committee des deutschen Palästinavereins wenden solle. Prof. Socin in Leipzig aber gab mir den Rath, bei Mr. George Armstrong, dem Secretär des Lond. Palestine Exploration Fund anzufragen. Ich war so glücklich, die Antwort zu erhalten, dass er mir eine Nachbildung jenes Gewichtes liefern könne. Als ich dieselbe bekommen hatte, habe ich sie erst selbst untersucht. Dann habe ich sie an Hrn. Prof. Jul. Euting in Strassburg, den bekannten Erforscher der semitischen Inschriften gesandt. Sein Urtheil habe ich wörtlich auf S. 425 meiner "Einleitung" abdrucken lassen. Das Wesentliche war, dass wir beide das Wort *shel*, "of" auf der Inschrift nicht finden konnten. Denn sie besteht auf beiden Seiten aus je sechs gleichen Schriftzeichen.

Ist dieses unser Verfahren gerecht beurtheilt durch Herrn Prof. Sayce?

(1) Er würdigt nicht den Umstand, dass eine Nachbildung, die ich aus dem Palestine Exploration Fund bekommen habe, mir als zuverlässig gelten durfte und musste. Denn wie konnten wir vermuthen, dass die Nachbildung wesentlich ungenau sei? Weshalb hätte der Palestine Exploration Fund eine Nachahmung, die nicht ein hinlänglich getreuer Reflex des Originals war, in seine Sammlungen aufnehmen können? Aber wir durften vermuthen, dass die Entzifferung der Aufschrift nicht gleich zuerst völlig gelungen sei. Denn dies ist schon öfter geschehen.

(2) Herr Prof. Sayce scheint noch nicht die wirkliche Beziehung des Originals und der Nachbildung festgestellt zu haben. Denn die Nachbildung zeigt auf jeder Seite des Gewichtes die gleichen Buchstaben, und zwar je sechs. Prof. Sayce schreibt:—

"As it happens, the part of the weight where the word *shel* is engraved is somewhat worn, and the cast has consequently failed to reproduce all the lines of the letters."

Aber daraus, dass etwas abgebrochen ist, scheint sich nicht zu ergeben, dass die Nachbildung *mehr* Linien, als das Original, zeigt, und dass der Buchstabe š (*sh*) als zwei Buchstaben sich darstellt. Ausserdem muss auf der einen Seite gerade soviel abgebrochen sein, dass auf dieser Seite *in Folge* des Bruches genau dieselbe Buchstabengruppe entstand, welche auf der andern Seite *ohne* den Bruch zu sehen ist. Ich darf hoffen, dass ein englischer Gelehrter noch einmal das Original vergleicht und den Grad der Ungenauigkeit der Nachbildung feststellt.

(3) Herr Prof. Sayce setzt voraus, dass ich die Untersuchung jenes Gewichtes unternommen habe aus Liebe zur negativen Kritik. Ich appellire an die Gerechtigkeit der englischen Gelehrten. Meine Veröffent-

lichungen sind in England nicht unbekannt. Ich hege die Zuversicht, dass insbesondere auch meine "Einleitung" die Solidität meiner Untersuchungen documentiren wird. Ich bin mir bewusst, dass das gleiche feurige Interesse für die geschichtliche Wahrheit mich mit Hrn. Prof. Sayce verbindet.

Prof. ED. KÖNIG, D.D.

*Translation of the above Letter.*

In your number of October 21st, Professor A. H. Sayce has criticised a passage of my book, "Einleitung in das Alte Testament mit Einschluss der Apokryphen und der Pseudepigraphen Alten Testaments." The love of historical truth common to both of us compels me on my side also to say a word respecting this matter.

When in the summer of 1892 I examined the linguistic character of the Song of Solomon, there came into consideration a weight which Dr. Chaplin had bought in Samaria. The inscription on this weight was published in the *Academy* of August 2nd, 1890. In order to form a judgment respecting this weight I wrote to Dr. Neubauer, of Oxford, to ask whether he could obtain a copy of it. He advised me to apply to the Committee of the German Palestine Society. But Professor Socin, of Leipzig, recommended me to make enquiries of Mr. George Armstrong, Secretary of the London Palestine Exploration Fund. I was fortunate enough to receive the answer that he could supply me with a copy of the weight. When I received it I first examined it myself, and then sent it to Professor Julius Euting, of Strasburg, the well-known investigator of Semitic inscriptions. His judgment I had printed, word for word, on p. 425 of my "Einleitung." The essential point was that we both failed to find the word *shel* "of" in the inscription. For it consists on both sides of six similar characters.

Has this proceeding of ours been rightly judged of by Professor Sayce?

(1) He does not sufficiently consider the circumstances that a copy, which I had received from the Palestine Exploration Fund, I had to regard as reliable. For how could we guess that the copy was essentially inaccurate? Why should the Palestine Exploration Fund receive into its collections a facsimile which was not a sufficiently faithful reflex of the original? But we might have supposed that the deciphering of the inscription might not be at first entirely successful. For this has often been the case.

(2) Professor Sayce appears to have not yet made up his mind as to the actual relation of the original to the copy. For the copy shows on each side of the weight the same letters, six in number. Professor Sayce writes: "As it happens, the part of the weight where the word *shel* is engraved is somewhat worn, and the cast has consequently failed to reproduce all the lines of the letters."

But it does not appear to follow that because a portion has been broken off, the copy shows more lines than the original, and that the

letter *sh* presents itself as two letters. Besides this, there must be just so much broken off on the one side, that on this side, *in consequence* of the fracture, exactly the same group of letters appeared which on the other side may be seen *without* the fracture. I venture to hope that an English scholar may be able to compare the original and to decide the degree of inaccuracy in the copy.

(3) Professor Sayce presupposes that I undertook the examination of the weight out of love to negative criticism. I appeal to the justice of English scholars.

My publications are not unknown in England. I entertain the assurance that my "Einleitung," especially, will afford documentary proof of the genuineness of my examinations.

I am quite convinced that Professor Sayce and I have the same deep interest in arriving at historical truth.

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18, Anerley Park, S.E.,  
October 31st, 1893.

It was with some surprise that I read in the *Academy* of October 21 the statement of Professor Sayce, that the cast of the ancient Hebrew weight brought by me from Samaria, which has been circulated by the Palestine Exploration Fund, is "imperfectly executed." After very careful examination of the weight and the cast, both Mr. Armstrong, the assistant secretary of the Fund, and myself are of opinion that the cast accurately represents the inscription on the original. Of course, with such a small object and with some of the letters much worn, it may happen that not every specimen of the cast is equally perfect.

As a member of the Executive Committee of the Fund I am anxious that this question should be set at rest; and in the interests of learning it is most desirable that the true reading of the inscription should be determined. I have sent the weight and cast to Professor W. Robertson Smith, of Cambridge, to be examined and reported on by him. Should Professor Driver, or any recognised authority, desire to see the original and compare it with the cast, I shall have great pleasure in endeavouring to arrange for their doing so.

THOMAS CHAPLIN, M.D.

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Christ's College, Cambridge,  
November 6th, 1893.

1. The size and form of this object are accurately represented in the woodcut given by Dr. Chaplin in *Palestine Exploration Fund Quarterly Statement*, October, 1890, p. 267. Professor König ("Einleitung in das A.T.," p. 425) describes it as something like a date stone (*etwa in Form eines Dattelkerns*), which gives a fair general notion of the size, but misses the characteristic point of the form. The weight is, in fact, a very perfect

and beautifully finished specimen of a genuine ancient type—spindle-shaped with a flat oval surface in the middle of one side. I have no special acquaintance with ancient weights, and cannot say anything as to the distribution of this particular type; it is known to me by specimens from Egypt, of much larger size but similar pattern, two of which I myself purchased at Gizeh in 1891. The flattening of the middle of one side is obviously convenient as providing a surface on which the weight rests without rolling; but I imagine also that the final adjustment to the standard was made in the process of rubbing down the flat base. The whole aspect of the weight and the skill with which it is shaped and polished seem to me to be strong presumptive evidence that it is genuine. If it be spurious, it is a forgery of a perfectly novel kind, and the first efforts of forgers in a new direction are not generally happy. Of course this argument in favour of the weight does not necessarily apply to the inscription; for it is a well-known trick to put a false inscription on a genuine object with a view to enhancing its value.

2. The inscription has been studied by Professor Sayce, on the original, and by Professor Euting, the celebrated epigrapher of Strassburg, on the cast published by the Palestine Exploration Fund. The copy of this cast used by Professor Euting was sent to him by Professor König, and the results of his examination are briefly communicated by the latter in his "Einleitung," p. 425 *note*. On one side of the weight Professor Sayce reads רבע של and on the other רבע נצג. As regards the first side, Professor Euting accepts רבע, but can find no trace of של; as regards the second, he admits that Professor Sayce's reading is possible if only נצג were a real word and gave sense. But he urges that נצג gives no sense, and that the last letter may be taken as ף instead of ן, in which case the words on the second side are not genuine Hebrew, but the Arabic *nazf*, "half," in old Hebrew characters, and so necessarily spurious. To all this Professor Sayce replies that the cast is imperfect and does not represent all the lines of the original, which in his opinion can only be read as he has read it.

3. In this state of the controversy a fresh examination of the original and a thorough comparison between it and the cast were clearly desirable. Through the kindness of Dr. Chaplin, I have had the use of the original for two entire days, during which I have studied it in every way, by natural and artificial light, with the naked eye and under weak and strong lenses. For the purpose of comparison I have been able to use two copies of the cast, one of which was supplied from the office of the Palestine Exploration Fund, while the other was lent me by Professor Driver. Both these copies appear to me to be excellent, and faithfully to represent every line of the inscription. On this point I entirely agree with what has been already stated by Dr. Chaplin in the *Academy* of November 4, from his own observation and that of Mr. Armstrong; and I may add that, at my request, my colleague Professor Bevan and Mr. F. C. Burkitt, both of whom are very competent judges in such

matters, were good enough to compare the cast with the original in the disputed place, and could detect no failure in the reproduction. Of course, the metal cast cannot perfectly represent the texture of the stone surface, and the lines are not always quite so sharp as in the original, but they are all there.

4. It is not asserted that there is any difference between the lines on the cast and those on the original, except in the place where Professor Sayce reads  $\text{שׁל}$  and Professor Euting cannot read that word; and as five witnesses are agreed against Professor Sayce in saying that they can see on the cast every line that appears on the original, it seems reasonable to conclude that Professor Euting with the cast, and Professor Sayce with the original, really *saw* the same lines, but interpreted them differently. In point of fact, neither the cast nor the original shows a complete Old Hebrew  $\text{שׁ}$  (which would have, approximately, the shape of an English W), but certain detached pieces, which must be prolonged and connected by imaginary lines before we can get out of them the one letter  $\text{שׁ}$  which Professor Sayce desires, or the two distinct letters  $\text{נצ}$  which Professor Euting suggests as possible. When it comes to filling up the missing parts of letters which either were imperfectly formed from the first, or have been partly defaced by wearing, the question is not one of pure eyesight, but of eyesight and judgment combined. And here the man who has the original before him has undoubtedly a great advantage over him who uses the cast, for he is in a much better position to judge how far defacing by attrition has been carried. Professor Euting's conjecture that the place where Professor Sayce reads  $\text{שׁל}$  may originally have contained three letters, corresponding to the  $\text{נצף}$  or  $\text{נצג}$  on the other side, implies an amount of wearing sufficient to obliterate entirely several of the principal lines. But the sharpness and depth of the lines that remain, and especially the sharp definition of their terminations, together with the absence of any trace, however faint, of lost lines, appear to be fatal to this hypothesis; and I am confident that Professor Euting would never have advanced it had the original lain before him. Whether Professor Sayce's  $\text{שׁל}$  is more defensible is a question that cannot be answered without going into somewhat complicated details. The  $\text{ל}$  of his  $\text{שׁל}$  seems to me to be clear enough both in the cast and in the original. Moreover, the cutting is deep and clear, showing that in this place there has been very little wearing (as might indeed be expected, since the point of the spindle would naturally be less worn than the middle), so that it is out of the question to suppose that any material part of the letter has disappeared. If it is not a  $\text{ל}$  it is not a letter at all. But as regards the *shin* (which I again ask the reader to think of as an English W), the facts are not so favourable to Professor Sayce. The two middle lines of the W are there, no doubt, and to the right of them there is a detached stroke which would do very well for the right-hand stroke of the W, if only it were connected instead of detached. One might



suppose that the angle of junction has been worn away, but in that case one would expect the two converging lines to thin off and become gradually weak as they approach, and this is not the case. Or, on the other hand, one might admit that the angle was never closed, but argue that this is only a piece of carelessness on the part of the engraver; which is not impossible, though hardly probable. But the real difficulty of Professor Sayce's interpretation lies in the left hand line of the supposed W. A first glance at the weight or cast does indeed show something which looks like a fragment of the upper part of the desired line. But on more careful examination under a powerful lens this fragment resolves itself into two elements (1) a clearly defined but very short cut, which has not the direction required for the left limb of a W, but rather runs parallel to the main or upright line of the  $\zeta$ ; (2) a splintered break proceeding from the lower part of the right hand edge of this cut, and trending downwards to the right. The distinction between the true cut and the break is perfectly clear to me in the original, but of course not so clear in the cast, which does not render the toolmarks quite sharply, and does not show at all the difference of surface between a saw cut and a splintered break. After having made out the composite character of this little stroke on the original, I persuade myself that with great care and strong magnifying power I can see even on the cast that the line is partly sawn, and partly due to splintering; but the study of the cast alone would hardly suggest this distinction, and so would leave it a very open question whether the whole stroke is cut (in which case it can hardly be anything else than a fragment of the fourth arm of a W) or the whole due to a superficial fracture (in which case a W is impossible). My own opinion as to the nature of the stroke is hardly more favourable to the reading W than the view that it is wholly due to a fracture; for it is the break alone which, by trending to the right as it descends, gives the line as a whole the appearance of running in the proper direction for the fourth limb of a *shin*.

5. I am afraid that these observations on the difficulties attending both the rival interpretations leave the matter more puzzling than ever; but there is one point not hitherto noticed on which I think that I can throw some light. I am convinced that the inscriptions on the two sides of the weight are not of the same date. The whole  $\text{רבע של}$  inscription (to name it after Professor Sayce's reading) is much more worn than the  $\text{רבע נצח}$ . How can this have happened if the two inscriptions are contemporaneous? Not by weathering, one side being protected and the other not; for then there would be a difference in the surface texture of the two sides. But that is not the case, as can be seen by taking the points of the weight between the thumb and forefinger and gently rotating it, at the same time observing the reflections of the light that falls on the surface. The whole surface has been worn by similar agencies, producing a uniform texture and polish. At the same time, the weight has no tendency to roll over upon the more worn side; so that there is no

physical reason why one side should be more worn than the other unless the stone is greatly softer on one side, which in so small a piece of an evenly grained hæmatite may be regarded as impossible. I conclude that the second inscription was engraved after the weight was worn by use. To verify this conclusion I requested a practical physical observer to look at the stone, and after careful examination he declared that he could not understand how anyone believed the two inscriptions to be of the same age. For further verification I took a strong lens and examined the toolmarks on each side, with the result that I found the second inscription to exhibit a different and inferior technique. To a certain extent the technical inferiority of the **רבע נצג** side is manifest even on the cast; notably in the letters **רבע**. But on the original the same thing appears in other letters—*e.g.*, in the **צ**. Straight strokes, which the first artist effected by a clean and uniform sawcut, are produced on the second side by two or three cuts, made by an uncertain hand, which could not keep a single direction truly.

6. Of course, if this be so—and the matter is one on which I appeal with confidence to all who will take the trouble to examine the original with minute precision—the idea that the two inscriptions are continuous and mean “quarter of a quarter of a **נצג**” (whatever the last word may mean) falls to the ground. And here I may notice another little point which possibly leads in the same direction. If the weight is set on its plane base, the second inscription is right side up, and the first is upside down, which hardly looks as if they were meant to be read continuously. At all events, it is now plain that the older inscription is complete in itself, and if it really reads **רבע של**, it may best be interpreted as standing for **רבע שלם**, “a quarter of full weight.” This use of **שלם** is Biblical, the contraction is strictly in accordance with analogy, and the phrase as a whole finds its exact parallel in the adjective *wāfin* “of full weight” on the glass coin weights of the Arabs.

According to old Hebrew idiom, “a quarter,” without specification of the unit, can only mean a quarter shekel. Now Mr. Petrie, in *Palestine Exploration Fund Quarterly Statement*, 1890, p. 267, makes our weight 39·2 grains, which would give a shekel of 156·8 (or something more if we allow for wearing). The weight of the old Hebrew shekel is still disputed, but the balance of evidence seems to me to favour the conclusions of Professor Ridgeway, who puts it at 130 to 135 grains. In that case, our quarter is too heavy; but it came from Samaria, and we know from Amos viii, 5, that the merchants of Samaria made the ephah small and the shekel great in order to cheat their customers.

In truth Professor Sayce's reading of this side gives an interpretation so easy and good, that one is reluctant to abandon it, and wonders why he himself did not hit on it. But, as we have seen, the possibility of reading **של** is doubtful or more than doubtful. And, if this reading is given up, it does not seem possible to make any other letters out of the group of signs without inventing imaginary supplementary lines on a scale for

which the general appearance of the surface affords no justification. But is it not possible that the disputed signs are not letters but numerical symbols? On Phœnician inscriptions numbers are frequently expressed by symbols in lieu of words, but even when the words are written in full the equivalent symbols often follow. Similarly, on the Assyro-Aramaic lion-weights, the denominations are expressed first in words and then in symbols, some of them denoting fractions, which were doubtless intelligible to many persons who could not read. Of symbols for fractions among the Phœnicians and Hebrews we have hitherto known nothing; but that they existed is probable, since both the Egyptians on the one side and the Assyrians on the other had a fractional notation. If, then, we find the word רבע "quarter" followed by a group of signs that cannot be read as letters without adding supplementary lines of a very hypothetical kind, it seems reasonable to suspend our judgment for the present and keep our eyes open for fresh evidence as to Hebrew and Phœnician arithmetical signs.

7. As regards the later inscription, it is difficult to believe that it can be anything but a modern forgery. It is not, of course, inconceivable that a new inscription was cut in ancient times after the old one was partly worn down; but the probabilities are all the other way. For my own part, I have little doubt that Professor Euting is right in reading the second word as נציף, and explaining it to be the Arabic word for "half." But how did the forger, after copying the רבע of the other side, which means "quarter" both in Arabic and in Hebrew, come to follow it up with the word "half"? On this point I can, at least, make a suggestion, which I give for what it is worth. The lines immediately following רבע on the old side are (1) the detached oblique stroke which serves as the right limb of Professor Sayce's W; (2) the chevron-shaped stroke which he takes for the two middle lines of the W. Now the first of these is the usual symbol for  $\frac{1}{4}$  in modern Syria, and the second is the modern symbol for  $\frac{1}{2}$ , turned through a right angle, so as to point upwards instead of to the left (*see* Caussin de Perceval, "Gram. Ar.-Vulg.," Paris, 1824, p. 73).

8. It is not denied that it is graphically possible to read the second inscription "quarter of a נציף"; and if it could be shown that נציף is a genuine Hebrew word giving a suitable sense, or even that a suitable new word of this form could be derived from a known root on ordinary etymological principles, this reading would deserve consideration, and we might after all be justified in concluding that the second inscription is ancient, though not so old as the first.

Professor Sayce, in *Palestine Exploration Fund Quarterly Statement*, 1893, p. 32, reads the word *netseg*—i.e., נציף or נצינ (ts being his transcription of the peculiar Semitic s which modern scholars commonly represent by ş), and he thinks it possible that the word means "a standard weight," and is derived from the root יציג. But every Hebraist knows that, if the word is *netseg*, it cannot possibly come from יציג or

from any known Hebrew root. Professor Sayce cites Dr. Neubauer, but that scholar never made the grammatical blunder of deriving a segholate noun with initial נ from the root נצנ. Further, Professor Sayce thinks that he has found another occurrence of his new word on a hemispherical bead from Jerusalem (Palestine Exploration Fund *Quarterly Statement*, *l.c.*), of which he says that "the letters are those of the Siloam inscription, and must therefore belong to the same period as the latter." Through the courtesy of Mr. Armstrong I have been able to examine the bead itself, with a cast published by the Palestine Exploration Fund, and find that the first two letters may very well be צנ, but that the character resembles that of the early Hasmonean coins rather than that of the Siloam inscription. The third letter is certainly not נ but נ. What these three letters mean I do not pretend to guess; and I do not see how one can reason from an inscription of three letters, not forming a known word, on a bead the nature and use of which are unknown. I will, therefore, say no more about the bead than that the inscription it bears is certainly not נצנ.

W. ROBERTSON SMITH.