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## The Gezer Inscription

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evident that the layer of clay on which these stones rest indicate the original surface of the ground, in which the press is sunk.

The mode of operation is obvious. The olives being piled on the flat stone A, were crushed with the rolling stones that, when not in use, were piled up in D. The expressed juice ran into the bowl, B—which actually contained a number of olive-stones—and was collected by an operator standing in the space C.

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The only important "find" since the last report was written was a small bronze weight, bearing two marks thus || and the word לַמֶּלֶךְ in characters similar to those in which the same word is used on the jar-handles. The standard is evidently the same as that of the weights inscribed Q. Probably we have here a *government* standard, distinguished from more or less unauthorised local standards; an exact analogy is offered by the local and government currencies in modern Turkey. This weight is of interest and may be of importance in discussing the problem of the jar-handles with "royal stamps."

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### THE GEZER INSCRIPTION.

By the Rev. G. B. GRAY, D.D., D.Litt.

MY notes, which accompanied the first publication of this inscription, had to be put together more hastily than I could have wished, and I gladly take the opportunity afforded me by the Editor of explaining how far subsequent discussion has modified views which I then expressed.

Father Vincent, who has discussed this inscription most fully and with great acuteness (see *Revue Biblique*, April, 1909), has had the advantage of seeing the stone. I fully recognize that an examination of the stone might bring me into greater or more decisive agreement with him on some points on which, with only the facsimile before me, I cannot see reason for abandoning my first conclusions.

1. I maintain my opinion that the calendar inscription is complete. It is true that Prof. Lidzbarski thought an original

eighth line has been lost, and Mr. Pilcher suggested that a large part of the original has disappeared. But in view of Mr. Macalister's remarks (*Q.S.*, p. 89), and the arguments of Prof. Ronzevalle and Father Vincent, it appears to me probable that the completeness of the inscription will be generally admitted. I therefore refrain from further discussion of the point at present. There is indeed a possibility that the name written perpendicularly is incomplete; but it is not certain that even this is so; Abi, according to 2 Kings xviii, 2, is a complete proper name: see *Studies in Hebrew Proper Names*, pp. 24, 83.

Except for six letters which I marked doubtful, and the letter which I read four times certainly and once doubtfully as ך, my reading of the inscription (with which Prof. Lidzbarski, working independently, agreed) has been accepted, and may now be regarded as certain.

Of the letters marked doubtful, the ל in l. 5 may be considered certain, and I accept כ as a more probable reading than ת (my first suggestion) for the sign that precedes it.

Three questions of reading remain:—

(1) Is the word that follows the second ירה in l. 1, זרע or נמע? I should certainly prefer to read זרע, but only the ע is really certain. As to the letter that precedes ע, something turns on whether the inscription is a palimpsest as Prof. Lidzbarski suggested and Father Vincent maintains; Mr. Macalister being inclined to dissent (*Q.S.*, p. 88). If ר is right, ז is the correct guess for the first letter of the word. Yet though there is room for a כ at the end of l. 1, there scarcely seems room for a ז, and certainly not for one of the same size as the ז in l. 5.

(2) Does כ form the last letter of l. 4 (Lidzbarski, Ronzevalle, and myself) or l. 5 (Vincent)? If of l. 4, it finishes as in l. 6 below its line; if of l. 5, *above*.

(3) What is the letter really intended by the sign which occurs four or five times, and which was read by myself and Prof. Ronzevalle as ך, was tentatively equated by Prof. Lidzbarski with ה (so also Dr. Dalman), and was read by Father Vincent as כ (so also in one case by Mr. Pilcher)? I rule out at once the second of these suggestions:—For (i) the assumption of a Sabaeen form for a single letter when all the rest are of well-marked North-Semitic type is in the highest degree hazardous. (ii) Though, on certain interpretations,

the article before **אסף, זרע, לקש, זמר** would be very appropriate, there is no adequate explanation in this case for its omission before **קץ**. We could more readily account for its absence in the compound expressions **קצר שערם, עמד פשת**, though Dr. Dalman is incorrect in saying that the article is never used "in the designation of barley harvest and wheat harvest"; see Ruth ii, 23. (iii) The reading **קצר הכל** (l. 5) gives an unsatisfactory phrase whether we interpret "harvest of all (the rest)" with Lidzbarski or, with Dalman, the time when harvest is in progress in all parts of the country.

I agree with Father Vincent that the choice lies between **נ** and **ך**: and I should like to feel sure that he is right in deciding for the **נ**, for there is no question that that permits the most attractive translation. But it may be wise to arrive slowly, if we arrive at all, at this conclusion. I observe (1) that there is almost complete agreement that the signs look like **ך**; (2) that the **ך** is given up on the ground that the inscription so read yields no sense: thus Prof. Lidzbarski asserts that *waw* "would not be in place either here (in l. 1) or elsewhere." Father Vincent says our choice must be between **ך** and **נ**, and "La première ne conduisant à rien d'acceptable, il faut opter pour la seconde." Yet see the discussion (*Q.S.*, p. 109) of Prof. Ronzevalle who accepts the **ך**. (3) That the assumption that the letter is **נ** involves the further assumption that in all five cases where the letter occurs, a surviving trace of the supposed earlier inscription or a parasitic stroke has converted a sign more or less resembling a normal **נ** into a tolerably good **ך**. Possible, no doubt, but strange. (4) That even the two signs which Father Vincent selects as the clearest cases of *nun* (the first in l. 1 and that in l. 6) do not show that decisive inclination of the down stroke from right to left which is characteristic of *nun*. In l. 1 the direction is even slightly from left to right: the down stroke being almost parallel to that of the following **ס**. On the other hand the slight variations of direction in the four or five cases of this sign in the inscription correspond to the variations in the direction of the down strokes of the *waws* in the inscription of Mesha.

2. *Translation and Interpretation.*—I modify my first opinion that only eight months are specified in the inscription. It is, I think, no accident that we have *eight* processes specified and *four* occurrences of the ambiguous sign immediately following **ירח**. Whether we

read יִרְחַן (Vincent), or יִרְחַו, I believe these four clauses cover two months (so Ronzevalle), and thus the whole inscription covers *twelve* months—internal evidence of the completeness of this inscription.

I feel much surer that זִמֶּר contains the reference, which could scarcely have been absent, to viticulture. But even the late pruning which takes place at the present day in June or July (Dalman, *Q.S.*, p. 119) falls rather too early to suit the place of the word in the calendar, which points to the time between mid-July (or, according to an alternative transliteration, mid-June) and mid-August. I think, therefore, that the term must cover the vintage itself.

It is valuable to have Mr. Macalister's evidence that at the present day flax at Aleppo is reaped in exactly the same manner as wheat. But the inference from the use of the two different terms (קֶצֶר and עֶצֶר) that barley and flax used not to be reaped in precisely the same way round Gezer seems to me probable. Unfortunately we cannot go much beyond this.

Of both the transliteration and the meaning of l. 5 I am still uncertain. Father Vincent's reading יִרְחַ קֶצֶן כֹּלם "month of all the reapings" is admirably (perhaps even suspiciously) idiomatic and sufficiently self-explanatory. What we most naturally look for after קֶצֶר is the name of another specific crop, but there seems little likelihood that such actually is expressed by the rather uncertain letters. Prof. Ronzevalle may be right in seeing two processes mentioned, but it would be hazardous to give קֶצֶר a different meaning from that which it has in the previous line—a meaning, moreover, which is not otherwise established in Hebrew. Still he may be right as to כֹּל, and it is possible, if the reading be וְכֹל, that the line means "a month of (continued) reaping and of measuring."

3. *Date.*—"The workmanship is rough, but the type of the letters is closely akin to the earliest inscriptions in the North-Semitic alphabet that we possess." I see no reason to withdraw this statement, so far as the letters are concerned on which the various decipherers agree. In particular no one has challenged my argument that the ם of the inscription has no analogy in inscriptions later than the eighth century. But what if the letter, which I took to be an early type of the *waw*, is really *nun*? The *nuns* in that case are certainly not good examples of the *nun* in use in the ninth and eighth centuries B.C.; but do they approximate more

closely to the *nuns* in use at the close of the seventh century or in the sixth century—the period to which Father Vincent assigns the inscription? There is indeed some analogy to one characteristic of these hypothetical *nuns*, viz., in the unusual direction of the down stroke, in one inscription assigned by some on palaeographic grounds to the sixth century B.C. This is an inscription found at Nora in Sardinia (*C.I.S.*, i, 144). But inasmuch as this tendency did not perpetuate itself, it would be precarious to argue that the Gezer alphabet must be as late as that of Nora.

Father Vincent also attaches weight to the differences that certainly exist between different examples of the same letters. But nothing adverse to an eighth century date seems to me to follow unless it can be shown that some of the examples are of a distinctively later type, and this has not, I think, been shown. As Father Vincent himself has well pointed out, the workmanship of this tablet and of the Moabite stone or the Siloam inscription is very different; to this rougher work of a rustic engraver I assign the noteworthy differences in the several examples of the same letters.

In conclusion, I should like to correct the impression which Father Vincent appears to have received, and perhaps Mr. Macalister himself, that I abandoned too easily (“avec tant de désinvolture”) the suggestion that the inscription belonged to the sixth century. It was obviously natural and proper for Mr. Macalister to mention this date, but it seemed to me obviously possible, as Mr. Macalister himself expresses it in his April Report (p. 88), that “chance should preserve a portable object of the kind so that it should be found in buildings of a date later than its own.” In other words, the date (say sixth century) of the stratum in which the inscription was found suggests only a *minimum* date for the actual writing of the inscription. This stone may surely have stood a couple of centuries or more pegged to a wall, and have fallen, when it fell, among potsherds that belonged to the date of its fall and not of its erection. From the position in which the stone was found it is illegitimate to argue that the inscription must be of the sixth rather than the eighth or ninth century. In any case, if it belongs to the eighth or ninth century, it is the work of some stonemason following, as best he could, the forms of the letters current in his own day; if it belongs to the sixth it is the work of someone who, for reasons not obvious, gave a certain archaic appearance to his work by introducing in certain cases (the  $\text{D}$  and the  $\text{r}$ ) ancient types of letters.