

Supplementa Ismaelitica

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10. Rehobot ha-Nahar

It is always a pleasure to become the subject of competent criticism that contributes to one's own knowledge as well as it enhances the progress of the discipline in general. It is fairly unpleasant, however, to have to deal with criticism that lacks competence. W. ZWICKEL's remarks concerning the date of the Edomite "king list" belong to the latter kind of criticism (BN 29 (1985), 29-31). Admittedly, the article by the present writer in ZAW 97 (1985), 245-253 is poorly written and poorly edited. Transitions and conclusions are not always supplied, but left to the insight of the reader. Besides some stupid misprints like "Qans" for "Qaus" (fn. 6, from handwritten additions to the proofs), the first half of the last sentence in fn. 23 got lost, which renders the remaining parts of the sentence unintelligible. This sentence should have stated that Theman in Gen 36,11 (and 36,15, which is derived from 36,11) is a post-priestly addition to the original pre-priestly list, based on a misinterpretation of the prophetic reference to that region. This interpolation is paralleled by the additions of "Qedmah" to the "sons of Ishmael". Therefore, and because ZWICKEL's comments may gain followers among those who are even less familiar with Semitic philology and Jordanian archaeology than he is, the two basic points will be rediscussed here. This author will not refer to them again.

It is unpleasant to be blamed for mistakes one actually did not commit. This writer never identified the Edomite region of Theman with the Arabian city of Taima, as ZWICKEL supposes, and vehemently refutes (BN 29, 29f). The article in ZAW 97 (1985) states quite clearly what Teiman in the opinion of its author was: a synonym for "Edom", i.e. the region from Wādī l-Ḥasā in the north to Rās en-Naqb in the south (p. 249 with fn. 21, and 250, Edom's "Stamm-

land"). According to this point of view, a statement like "an Edomite from Teiman" does not make sense: it is equivalent to "an Edomite from Edom". Ancient scribes did not waste their ink for statements like this! The "land of the Teimani" in Gen 36,35 is supposedly "the land of the man from Taima". If Theman is not a synonym for all of Edom, but the name of a region in Edom, then it is the region around Bozrah (Am 1,12). The references Jer 49,7.20; Ez 25,13; Ob 9, however, render the first assumption more likely. Bozrah is already the home of "Edomite king no. II"; it is hardly conceivable that "Edomite king no. III" ruled the countryside of Bozrah at the same time.

Teiman is not Taima, but a "Teimani" could be both a "man from Teiman" or "a man from Taima", and it is only the latter meaning that makes sense in Gen 36,34. Whether Taima was politically included in Edom at the beginning of the sixth century B. C., cannot be decided. There were, however, contacts between Edom and the region of Taima at this time, due to the establishment of an Edomite colony at Dedan (ZAW 97, 250; more elaborate BN 22 (1983), 27-28). Therefore, the immigration of a sheikh from the Taima area into Edom at the end of the sixth or the beginning of the fifth century B.C. is a plausible possibility. (This is one of the dropped conclusions and assumptions in the ZAW article). Furthermore, it is quite likely that Edom and North Arabia were grouped together within the framework of the Persian administration (cf. ZAW 97, 251), as long as there was one in these areas, that means until ca. 400 B.C. (Ismael, 77; HÖGEMANN 1985: 23). Admittedly, this assumption needs more evidence than is available now. Be this as it may, nothing speaks against the assumption that one of the Arab sheikhs that ruled in Edom at the beginning of the fifth century B.C., and who are registered in Gen 36,31-39, immigrated from the area of Taima. The statement of Pliny the Elder, nat. hist. VI 28 (32) 157 Nabataeis Thimaneos iunxerunt ueteres "the ancients joined the Nabataeans (Qedarites) with the people of Taima", and the rule of the Qedarite sheikh Gušam in Edom around 450 B.C., provide parallels to this immigration (cf. Ismael, 104-108).

That ZWICKEL has not learned to read before he started to write is bad; that he does not know how to argue from archaeological evidence, is worse. The distribution of the areas of soundings and excavations over the surface of Buseirah do allow one to draw conclusions about the occupation of the site; and, ZWICKEL ought to be reminded that it is always the excavation that verifies or falsifies the preliminary assumptions based on the surface evidence. Surface pottery samples may be accidental sherd scatters with

no occupational history phase of the site that correlates to them; massive occupational phases may not be attested among the surface pottery at all; and, according to the present state of archaeological expertise; surface pottery field readings are only accurate within a range of plus/minus 100-300 years, for a variety of reasons (cf. BANNING 1986: 35). This makes readings like "Iron IC-IIA(1000-721 B.C.)" actually meaningless and equivocal to GLUECK's "Early Iron I-II" calls which do never indicate more than that he found some pottery which was produced sometimes between ca. 1300 B.C. and 400 B.C. (see below).

ZWICKEL's argumentation becomes ridiculous when he tries to refute BENNETT's results from her excavations with GLUECK's survey results from 1934. According to the stratigraphic evidence, Buseirah was not founded before the end of the eighth century, and probably somewhat later (HART 1986: 57 with fn. 1). According to the same evidence, the pottery called "Edomite" by GLUECK and dated by him, on the basis of unjustified inferences from Biblical history, to the thirteenth through eighth centuries B.C., has to be dated to the eighth/seventh through sixth/fifth centuries B.C. (HART 1986: 54; 57; HART-KNAUF 1986: 10). If ZWICKEL would have read GLUECK's description of the "Early Iron Age" pottery more carefully (GLUECK 1935: 97), he may have figured out that GLUECK did not find any other "Early Iron I-II" pottery at Buseirah than that which is called "Edomite" today, and dated to the eighth/seventh through sixth/fifth centuries B.C. As far as southern Jordanian Iron Age pottery and the validity of GLUECK's statements about it are concerned, B. MACDONALD is surely no authority, who has not published a single sherd up to now. In the article quoted by ZWICKEL (p. 31 fn. 12: MACDONALD 1983), MACDONALD simply records the field readings of J. SAUER, who is no authority in the field of Southern Jordanian pottery either, and who's pottery reading categories ("Iron IC - IIA (1000-721 BC ": MACDONALD 1983: 19) do not make sense to the present writer (721 B.C. is not a meaningful date in the history of southern Jordan, nor is there any logical connection between political events - traditional historical year-dates - and the production and distribution of pottery types; cf. LENZEN (fc.). What has to be said about GLUECK's pottery dating terminology, has been said already by M. WEIPPERT (1979: 28-30).

The point still stands: there were no or nearly no permanent settlements on the Edomite plateau prior to the end of the eighth century B.C. (HART 1986:

54; 58). From the end of the eighth to the seventh century B.C., the Assyrian and Neo-Babylonian inscriptions provide a list of kings of Edom none of which can be identified with a ruler from Gen 36,31-39 (WEIPPERT 1982: 295). Therefore, the Edomite "king list" Gen 36,31-39 cannot derive from any period prior to the middle of the sixth century B.C. It is the period from ca. 550 to ca. 450, when both the names of the list and the structure of their arrangement make sense.

As far as ZWICKEL's identification of Rehoboth ha-Nahar with Rās er-Riḥāb is concerned (BN 29, 32-34), the textual evidence (from Gen 36,31-39) requires a site that was occupied, or at least frequented, in the sixth/fifth centuries B.C. The archaeological evidence as it is presently available is inconclusive. MACDONALD 1938: 26 does not mention any Iron II C/ Edomite ware; the present writer does not know, however, what MACDONALD means by "Iron I/II". This category may well hide some Iron IIC coarse ware.

Philologically, ZWICKEL knows the weak point of his identification, but tries to evade it high-handedly (BN 29, 32 with fn. 15) in order to prove his own point. Wādī l-Ḥasā, the only "nahar" Rās er-Riḥāb can be referred to, is, like Wādī Mūjib, not a *nahar*, but a *naḥal*, especially seen from above. *Nahar* signifies the presence of running water. Since Rehoboths and Rihabs are fairly omnipresent in ancient as well as in modern Palestine, Jordan, and Syria, toponymy is no viable argument for the equation.

The only wadi system in the territory of Edom that may have been a nahar in antiquity, is the system of Wādī l-Ghuweir that cuts the Edomite plateau into two. Even today, it is not completely without water in summer. The flow of water would have been considerably higher when the Edomite plateau and its slopes to the west were more densely wooded than they are today. This was probably the case in the Iron Age. From the survey of the slopes of the Moabite plateau comes evidence for a movement of settlements from the slopes, where they were centered around springs, up to the plateau, where cistern water had to be used, after the end of the Iron Age. This indicates that the springs probably had dried out between the sixth/fifth centuries B.C. and the first centuries A.D. (WORSCHKECH-KNAUF 1986: 75).

There are two possible identifications of "Rehoboth ha-Nahar". First, the name may not signify a place at all, but a region. In this case, it may refer to the plain on the Edomite plateau which is the catchment area of the Wadi

l-Ghuweir wadi system, and divides el Jibāl, northern Edom, from esh-Sharā, southern Edom; or, it signifies a settlement situated somewhere within this system. In this case, Khirbet el-Jāriyeh, one of the larger Iron Age copper smelting sites within this region, would be a candidate. Human presence is attested at Khirbet el-Jāriyeh until ca. 400 B.C. (KNAUF-LENZEN *loc. cit.*). Its topographical situation, lying within a wadi bench on a relatively broad terrace, enclosed by mountains on all sides with relatively narrow approaches through the wadi (GLUECK 1935: 23), would suit the needs of the ancient name as well.

Considering these two choices, this author still prefers to classify "Rehoboth ha-Nahar" as unlocalized. After what has been said, the demands of the historian are met as honestly as possible. It seems that it is only the Biblical map maker who, being confronted with the request to fill maps with names and to distribute as many Biblical names on his maps as he could, may be unhappy about this conclusion.

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