

The Hebrew and the Seven Year Cycle

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Recently Oswald LORETZ has tried to write the definitive dissertation about the problem of the *habiru*/Hebrews¹. In this study LORETZ is anxious to demonstrate that we no longer have any reason whatsoever to consider the *habiru* an ethnic group in the Late Bronze Age. Without doubt, *habiru* is a sociological designation of some sort². Any simplistic identification of *habiru* and Hebrews must be refuted as false; it is an indication of the application of false methodology, a mixture of different categories, a too rash linking of Biblical and Oriental studies³. LORETZ's second thesis says that the term "Hebrew" in the OT must without qualifications be interpreted as a national designation. It is never a sociological designation⁴. On the other hand LORETZ maintains that the identification of Accadian *habiru* and Hebrew עִבְרִי is unproblematic from an etymological point of view⁵. Accordingly, we are entitled to reckon with an analogous development from a sociological interpretation of *habiru*/Hebrew to an ethnic understanding. LORETZ, however, abandons any attempt to describe such an evolution because of his third assumption that none of the OT references

1 O. LORETZ, *Habiru-Hebräer. Eine sozio-linguistische Studie über die Herkunft des Gentiliziums 'ibrf vom Appellativum habiru*, BZAW 160, Berlin 1984.

2 LORETZ, *Habiru-Hebräer*, pp. 78ff.

3 LORETZ, *Habiru-Hebräer*, p. 195.

4 LORETZ, *Habiru-Hebräer*, pp. 181f.

5 LORETZ, *Habiru-Hebräer*, p. 245, but with reservations. We must acknowledge that of course it is a problem that *habiru* is an appellative whereas עִבְרִי is a gentilic. On the other hand, a separation of the two terms because of this philological problem is over-hasty, because we may speak of a development from *habiru* to עִבְרִי peculiar to the Hebrew language - after all the term עִבְרִי is generally used of Israelites in the OT and is not known outside the OT before the Hellenistic Period.

to עברי is pre-exilic, all of them being post-exilic designations of members of the Jewish community⁶.

Evidently, his assumptions are based on the firm conviction that the OT passages which mention the Hebrews are all late, as is the case with the Joseph Novella⁷ and the "Passover Legend"⁸. However, LORETZ does not give up the idea of an earlier date for the narratives in 1 Sam, in which the Hebrews appear, where the term always refers to an Israelite population element⁹. In the case of 1 Sam LORETZ follows a different path and simply eliminates all passages which include references to the Hebrews. According to LORETZ these sections must be reckoned late (post-exilic) additions to the older narratives¹⁰.

Outside of these fairly extensive parts of the OT the designation "Hebrew" also appears in three interrelated texts, all of which seemingly contain a slave law: Ex 21,2-11; Dtn 15,12-18 and Jer 34,8-20. Contrary to the usual interpretation, LORETZ suggests quite an original theory since he argues that the slave law in the Book of Covenant in Ex 21,2ff. must be later than the appearance of the same law in Dtn 15. The reason is that the seven-year-cycle in Ex 21,2 clearly presupposes the "idea of the Sabbath", or so LORETZ says, and it is an indisputable fact that this idea did only originate in the Exilic and Post-exilic periods¹¹. Finally, LORETZ denies - and here he is expressively at variance with this author - that the two mentions of "Hebrews" in Jon 2,9 and Gen 14,13 contain any sociological connotations in spite of the Greek translation of עברי in Gen 14,13 as ὁ περότης "the straggler". According to LORETZ the Greek

6 LORETZ, Habiru-Hebräer, pp.181f.

7 Gen 39,14.17; 40,15; 41,12; 43,32.

8 Ex 1,15.19; 2,7.13; 5,3; 7,16; 9,1.13; 10,3. Cf. also Ex 2,6.

9 1 Sam 4,6.9; 13,3.7.19; 14,11.21; 29,3.

10 LORETZ, Habiru-Hebräer, pp.117-122. In advance we may say that his method is most unsatisfactory because he does not try to solve the difficulties in the text except by cutting them away. LORETZ would have been more cogent from a methodological point of view in case he had passed the same sentence on the narratives in 1 Sam as on the Joseph Novella and the "Passover Legend". i.e. if he had considered all of them post-exilic (thereby I do not intend to say that I by necessity consider the narratives of the Philistine Wars to be post-exilic).

11 LORETZ, Habiru-Hebräer, pp.160f.

translation is based on an etymological interpretation of עברי on the basis of the verbal root עבר¹².

Certainly, LORETZ's showdown with the national or ethnic interpretation of *habiru* is very much to the point and extremely important. From now on any further discussion of this subject must cease. Nevertheless, this theme takes up so much space in his book that it is, somehow, difficult to escape the feeling that he "is hunting mosquitoes with a big-game rifle". Furthermore, his opposition to the "national" understanding has almost become an obsession to him and therefore he does not realize that his own theses have led him astray so that he draws quite untenable conclusions.

To make a start we may refer to his thesis of the exclusively late origin of the designation "Hebrew". This hypothesis is evidently very problematic because he is now forced to postulate that the post-exilic writers so to speak choose the term "Hebrew" at random among perhaps scores of other possibilities. Here he would have improved his own thesis if he had maintained that *habiru* should correctly be read *hapiru* ('*apiru*) thus denying any etymological connection between *habiru* and עברי¹³. Now he - quite correctly - gives up this easy solution. Instead he decides for the most difficult explanation and links *habiru* and עברי etymologically. Thus the problem is unsolved: why this designation? Here the Greek rendering of Gen 14,13 might have served him well as a point of departure. He would have been entitled to argue that the Greek interpretation of עברי here, based on the verbal root עבר, clearly demonstrates why the designation "Hebrew" was chosen to designate Israelites: this term evidently describes the Israelites as foreigners, that is, "immigrants" in Palestine according to the commonplace and canonical picture of Israel's past in the Post-exilic Period. Instead his massive argumentation in favour of a national interpretation of עברי opposes such a logical conclusion.

Therefore he also underestimates the importance of the fact that the designation "Hebrew" is not spread out randomly throughout the OT; it

12 LORETZ, *Habiru-Hebräer*, p.242, concerning ὁ περᾶτης as an etymological rendering of עברי, and pp.173-181 concerning Gen 14,13 and Jon 2,9.

13 Cf. to this LORETZ, *Habiru-Hebräer*, pp.239f., in a chapter completely devoted to the question of the etymology of *habiru*.

nearly always occurs in specific contexts. The term is generally placed in the mouths of either Egyptians or Philistines¹⁴. This must be compared to another fact, namely that "Hebrew" is never used of post-exilic Jews in the sections in the OT which expressly refer to this period. As a logical conclusion to his own thesis LORETZ might have postulated that the term "Hebrew" in the Post-exilic Period was used of Jews in a specific social context, and a most obvious theory would be that the designation was commonly attached to the Jews of the Egyptian Diaspora by Egyptians, though I very much doubt whether there is any evidence of such an application in the sources pertaining to Jews in Egypt before the generally supposed dead-line of the Greek translation of the OT, i.e. before the second century B.C.¹⁵

In spite of LORETZ's meticulous survey of the individual passages including עברי in Gen-Ex and 1 Sam we must conclude that he loses his way in a mass of details. This is the reason why he has not been able to explain the motives of the redactors who used the designation in exactly these sections and not elsewhere. It is certainly not accidental that the term "Hebrew" is placed in the mouths of Egyptians and Philistines; the redactors clearly had some kind of reason to put it there. Our task is to investigate what these reasons might have been. Accordingly, in my paper from 1979, "*Hebrew' as a National Name for Israel*", I tried to delineate the semantic field according to which עברי was used to designate Israelites by non-Israelites - Jonah being the only exception to this rule (Jon 2,9). I also tried to uncover the secondary connotations which were conclusive for the application by the Biblical writers of exactly this term at exactly those places. Thus it was possible to conclude that "Hebrew" is never a "neutral" designation of Israelites in the OT, instead it always includes a special flavour: Abraham lives as a foreigner in Canaan, Jonah is a refugee from his home-land, the Israelites in Egypt are

14 Cf. N.P.LEMCHE, "Hebrew" as a National Name for Israel, StTh 33 (1979), pp.9ff.

15 Cf. that it is a well-known fact that the members of the Elephantine colony termed themselves "Jews", cf. f.ex. A.COWLEY, Aramaic Papyri of the Fifth Century, Oxford 1923, no.6:3.9.10 (and many other places): יהודי. I thank my colleague F.Cryer for this reference.

Palestinian fugitives, and in 1 Sam the Philistines clearly talk about run-away slaves and members of Israelite tribal society at the same time.

An interesting point is the fact that the semantic field covered by the various connotations of עֲנָרִי in the OT clearly stand out as soon as we make a more general survey of all the applications of the term in the OT. This field includes aspects which are very much the same as those we find when we deal with the old sociological designation *habiru*. Thus it is not unreasonable to argue that the Biblical redactors at least had some understanding of a pre-national meaning of עֲנָרִי and this knowledge was decisive, since they chose to make use of the term in Gen-Ex and 1 Sam - no matter whether we decide for an early date or a late date of the redactional work¹⁶.

Of course Klaus KOCH has pointed to a kind of explanation: The Biblical authors are reflecting actual usage f.ex. among the *Philistines* (in the case of Jonah, but certainly also in 1 Sam) even in the Post-exilic Period¹⁷. A variation of this theory - and perhaps more probable - is the thesis that in the 1.mill.B.C. the עֲנָרִי designation had kept some of the connotations which formed part of the old term *habiru* in the 2.mill.B.C.

Accordingly, it is not unimportant - in fact, it is rather decisive - if it is possible to point at an example of an application of the designation עֲנָרִי in a context which is not, perforce, very late and redactional. A natural point of departure for the analysis is, of course, Ex 21,2ff., including the two "citations" in Dtn 15 and Jer 34. It is LORETZ's purpose to demonstrate that Ex 21,2 is even younger than Dtn 15, 12-18 in its original wording. He also maintains that both texts (Ex 21,2ff.

16 Of course, in this study it is impossible to dwell on this theme. My account of the emergence of the historical tradition in the OT will be published in my *Early Israel. Anthropological and Historical Studies on Israelite Society in Pre-Monarchical Times*, which will appear in print in 1985 (cf. part II, chapter 6), but cf. also my *Det gamle Israel*, Aarhus 1984, pp.28-65. It goes without saying that to a large degree I am in accordance with the views of LORETZ as to the late redaction of the traditions in question.

17 Cf. K.KOCH, *Die Hebräer von Auszug aus Ägypten bis zum Grossreich Davids*, VT 19 (1969), pp.37-81, pp.43f., but also the refutation in LEMCHE, *StTh* 33, p.10 n.29. Cf. also LORETZ, *Habiru-Hebräer*, p.181.

and Dtn 15,12ff.) influenced each other reciprocally¹⁸.

The late date of Ex 21,2ff., or so LORETZ maintains, is dependent on its bearing on the idea of the Sabbath, which he reckons to be exilic or even post-exilic¹⁹. Now, here LORETZ is really on quite shaky ground. Of course he is right (and I.CARDELLINI is wrong²⁰) in maintaining that the formalized idea of the Sabbath in the OT reflects a rather late development of this notion. Therefore the sabbatical law in Dtn 15 is not a reflection of an old law of pre-exilic origin, understood as a whole. On the other hand, as I have demonstrated in my article dealing with the manumission of slaves, the law of the Sabbatical Year in Dtn 15 is composed of at least two different and originally independent elements both of which were only secondarily attached to the Sabbatical Year²¹.

The wording of Ex 21,2 clearly shows that this must be so because it regulates cases of individual, rather than collective manumissions. The same remarks apply to the actual wording of Dtn 15,12-18, which evidently also refers to individual manumissions and not to collective transactions every seventh year. This interpretation of Ex 21,2ff. and Dtn 15,12-18 is preferable to LORETZ's rather extensive cuts and emendations of the text of Dtn 15,12-18, which are mostly done on order to make the text fit his thesis²².

The fallacy, however, is the fact that LORETZ reckons the seven year scheme in Ex 21,2 to be dependent on the institution of the Sabbatical Year. This assumption is quite unnecessary because the number *seven* is obviously a round number. The mention of the "seven years" only indicates that the Hebrew has to be a slave for some years and it does not give a precise date of his manumission. The traditional importance of the round number seven in Oriental tradition is well-known and for this reason I

18 LORETZ, Habiru-Hebräer, p.161.

19 LORETZ, Habiru-Hebräer, p.141.

20 Cf. I.CARDELLINI, Die biblischen "Sklaven"-Gesetze im Lichte des keilschriftlichen Sklavenrechts, BBB 55, Bonn 1981, pp.366-368.

21 Cf. N.P.LEMCHE, The "Hebrew" Slave, VT 25 (1975), pp.129-144, and The Manumission of Slaves - the Fallow Year - the Sabbatical Year - the Jubel Year, VT 26 (1976), pp.38-59. See especially VT 26, pp. 43-45.

22 LORETZ, Habiru-Hebräer, pp.153-160.

shall only draw attention to a paper dealing with this subject by M.LIVERANI dating from 1967²³. Because of this false identification LORETZ's second argument concerning the interrelationship between doorposts in private houses and in temples is rather unimportant²⁴. His argument might just as well lead to the opposite conclusion²⁵.

Furthermore, in my papers dealing with the "Hebrew" slave and the manumission of slaves I assumed that Ex 21,2ff. must be considered the original source of the law of Sabbath in Dtn 15 and of the deuteronomistic version of the edict of Zedekiah in Jer 34. Since, after all, "Hebrew" does not appear in a random way in the OT, this explanation of the application of "Hebrew" at all instances is still the most likely; it is at all cost to be preferred to an assumed post-exilic date of even the wording of Ex 21,2. Another argument in favour of this traditional understanding of the relationship between the three examples of the slave law is the fact that the priestly legislation of the Year of Jubilee, which clearly presupposes the deuteronomistic idea of the Sabbath and elaborates on it, has suppressed the term "Hebrew", and it goes without saying that this idea of the "Jobel Year" is post-exilic²⁶.

The speciality of the law in Ex 21,2ff. is the combining of the two terms *hupšu*, Hebrew אֶפְשִׁי, and *habiru*, Hebrew עֲבָרִי. Exactly this combination of the two terms made it possible to claim that this law must have had a Pre-Israelite origin²⁷. Against this interpretation LORETZ argues that it presupposes that אֶפְשִׁי was sometimes used of a specific social

23 M.LIVERANI, "Ma nel settimo anno ...", Studi sull'Oriente e la Bibbia offerte a P.Giovanni RINALDO, Genova 1967, pp.49-53.

24 LORETZ, Habiru-Hebräer, pp.145f.

25 LORETZ's interpretation of Ex 21,6 is based on Dtn 6,9; he argues that Ex 21,6 is dependent on Dtn 6,9, i.e. it is a consequence of the deuteronomistic centralization of the cult. אֱלֹהִים must be monotheistic, LORETZ maintains, and therefore it is impossible to consider this god a "household god". Nevertheless, it is difficult to understand why a centralization of the cult should result in a decentralization of the worship, at least on this level (the individual house). Accordingly, Ex 21,6 might just as well represent a pre-deuteronomistic custom. Whether אֱלֹהִים then is a "household god" or Yahweh or some other god (the "local" god) must be decided on the background of Ex 21,2-11.

26 Cf. LEMCHE, VT 26, pp.46-51.

27 LEMCHE, VT 25, pp.136ff.

class in Israel, but there is so far no evidence of the existence of such a class of עַמְּסִי - in fact a kind of argument related to my own criticism of LORETZ's talk about post-exilic Hebrew-Jews²⁸. I have previously voiced the idea that in the note in 1 Sam 17,25 we have an example of עַמְּסִי used as a social designation of some sort, since it was my point that Saul's promise to the person who killed Goliath was that he would be the king's client, i.e. he would become an employee of the king²⁹. Evidently in the narrative of David and Goliath we find themes which belongs to the genre of fairy-tale: The youngest son is the only one who stands up to the test, and as a consequence he marries the king's daughter and inherits the kingdom. As is well-known, David received Saul's daughter, Michal (or so the narrative runs), and obtained a very high position in the service of Saul. The usual happy ending to the fairy-tale is very near. As is also well-known, the author of "David's Rise" made use of literary patterns most likely borrowed from the realm of the fairy-tale³⁰. In spite of the objections

28 LORETZ, Habiru-Hebräer, p.259 n.33. We must ass that as a matter of fact nobody has yet made a penetrating study in order to uncover client groups in Israel in the Period of the Monarchy. Nevertheless, if LORETZ had demonstrated a little more sociological insight he might not have been so categoric, since the phenomenon of clients is a common feature in societies of the type represented also by the historical Israel, both in the pre-national period and under the kings. To speak of a "Bevölkerungsklasse" is also wrong. In fact, the notion of "class" is unsuitable as an analytic instrument when dealing with such societies. The clients do not form a specific "class"; they are attached to their patrons in an individualistic way. The society is thereby not structured horizontally as a "class society", but vertically, in parties or fractions. It follows that it may be quite difficult to detect "classes" in ancient Israel.

29 N.P.LEMCHE, עַמְּסִי in 1 Sam xvii 25, VT 24 (1974), pp.373-374. To be honest, this hypothesis has generally not been well received. Normally scholars stick to the traditional interpretation and consider exemptions from taxes or the like to form the background of Saul's promise. However, scholars have forgotten that it was not a bad life to be a client of the king; nor does they understand that to be employed by a king is the same as to be his client. The clients received their sustenance from the palace - in the case of David this "sustenance" would have been quite substantial because of his important position at the court. Cf. also the description of the gifts to the king's clients in 1 Sam 8,11ff.

30 The literature on this topic is now quite extensive. Here I shall only mention N.P.LEMCHE, David's Rise, JSOT 10 (1978), pp.2-25; P. KYLE

by T.WILLI to my interpretation of 1 Sam 17,25³¹, we may not discharge the possibility that his application of חפשי may indicate that this author, who presumably wrote at some time during the Period of the Monarchy, had some information about the existence of a social category which we term "clients" whereas the designation for "client", חפשי, was emptied later on of its real meaning only to become a rather bleak reference to a man who is "free"³².

To be true, it is not possible to prove the correctness of this thesis, even though the possibility of the existence of such a client group in Israel is not unlikely since it was a wide-spread social phenomenon in the Near East (or better in the Mediterranean world) in ancient times³³, as it is even to day³⁴. The interesting fact is, however, that the law of

MCCARTER, *The Apology of David*, JBL 99 (1980), pp.489-504; cf. also the newest study by K.W.WHITELAM, *The Defense of David*, JSOT 29 (1984), pp.61-87. Concerning the fairy-tale motif I must refer to M.LIVERANI, *Partire sul carro per il deserto*, AIUON 22 (1972), pp.403-415 (dealing with the Idrimi inscription). Cf. also LIVERANI's use of this theme in his *L'histoire de Joas*, VT 24 (1974), pp.438-453. Regrettably, he has not submitted the narrative of "David's Rise" to an analogous evaluation. On the other hand, the basic similarity between the careers of David and Idrimi has long ago been pointed out by G.BUCCELLATI, *La "carriera" di David e quella di Idrimi re di Alalac*, *Bibbia e Oriente* 4 (1962), pp.95-99.

31 Cf. T.WILLI, *Die Freiheit Israels*, *Beiträge zur alttestamentlichen Theologie*, Festschrift W.ZIMMERLI, Göttingen 1977, pp.531-546. In spite of LORETZ's objections to my refutation of WILLI, in *StTh* 33, p.2 n.4 (LORETZ, *Habiru-Hebräer*, p.259) I stick to my remarks that the economical interpretation of the slave legislation in Ex 21,2ff. by WILLI and by LORETZ himself forces upon them another understanding of עבדי in Ex 21,2; 2 Sam 17,25, etc. Nevertheless, the law in Ex 21,2ff. never discloses that economical troubles should be the reason for the transactions; to this extent the law is "neutral" as to the motives behind the acts of the "Hebrew".

32 N.P.LEMCHE, VT 26, p.45; cf. also O.LORETZ, *Die hebräischen Termini חפשי "Freigelassen, Freigelassener" und חפשי "Freilassung"*, UF 9 (1977), pp.163-167, cf. his *Habiru-Hebräer*, p.263.

33 Se n.28.

34 The relevant information may be found in many ethnographical descriptions of Middle Eastern society even to day. Parts of this material will be presented by me in *Early Israel* (part II, chapter 1 and 2), but a thorough analysis of the phenomenon has not yet appeared, at least not in a Middle Eastern context.

the "Hebrew" slave fits a milieu perfectly in which both עברי and חפשי were social designations. Neither עברי nor חפשי appears in the OT in an incidental way. חפשי is mostly directly connected with עברי and the majority of examples of חפשי are to be found in the three interrelated texts, Ex 21,2; Dtn 15,12-18 and Jer 34,8-20³⁵. Therefore it is very unsatisfactory that LORETZ dissolves this basic relationship between the two designations, because the consequence of this is that the authors of Ex 21,2 etc. must have chosen both designations at random - but both עברי and חפשי derive from specific sociological terms - at least before the emergence of historical Israel. To introduce a free citation from Oscar WILDE we may say that one accidental occurrence might be permissible, but two sound like gross negligence: The application of one of the two designations, עברי or חפשי, might be explained away, but together this application is not likely to be fortuitous.

There may never be a sure proof of this thesis as it remains a mere hypothesis that the law of the "Hebrew" slave in Ex 21,2ff. could form part of the inheritance dating from the era before the emergence of the Israelite state c.1000 B.C., or else it perhaps reflects a social usage during the early Israelite monarchy. This law was meaningless later on and it was accordingly necessary to explain who the "Hebrew" was in case the law in Ex 21,2 was cited.

Finally, it is necessary to emphasize that this interpretation which explains the choice of the designation "Hebrew" in such late texts as the Joseph Novella and the "Passover Legend", does not presuppose that the Israelites were ever Hebrews in Egypt. Thus the development from a sociological meaning of עברי to an ethnic one, a development not totally brought to a conclusion in the late contexts in which עברי appears in the OT, does not presuppose the historicity of the Mosaic tradition in the OT. If LORETZ had not misread my article from 1979 he would have acknowledged that I nowhere speak of a historical sojourn of early Israelites in Egypt. I only mention the fact that such information is contained in the later Israelite tradition. Thus this tradition must be evaluated as a

35 Ex 21,2; Dtn 15,12.13.18; Jer 34,9.10.11.14.16; and outside this context only Job 3,19; Is 58,6 and of course 1 Sam 17,25.

literary survival and not as a historical recollection³⁶.

On the other hand a traditio-historical analysis of the fate of the designation "Hebrew" in an Israelite milieu might contribute to an uncovering of a social process which took place in Palestine between the dissolution of the city-state system of the Late Bronze Age and the appearance of the Israelite monarchy in the Iron Age. In this historical context we have no need of any national interpretation of *habiru* - such an understanding of the term rather obscures our understanding of the process in question, because it introduces quite extraneous ideas: the question of national identity and ethnic "purity". Quite to the contrary nothing prevents a social group, i.e. a society of *habiru*, from developing a "national" ideology and identity as time goes by in response to a collective social experience, i.e. a communal pre-history in which all members of the society participated³⁷. The interesting point is that this model does not presuppose an early mixture of biblical and oriental studies. The model works well without adducing the biblical tradition of the pre-history of Israel, and it is only dependent on a correct interpretation of the results of the archeological excavations and of the available inscriptional materials which inform us about conditions in Palestine and the neighbouring countries. Of course such a model also presupposes that the scholar in question is open to the possibilities of applying dynamic social theories.

36 LORETZ, *Habiru-Hebräer*, p.167, with a reference to LEMCHE, *StTh* 33, p.15. Here I write: "In all those examples <i.e. in the Book of Exodus> the Israelites are Hebrews because they are confronted with the Egyptians among whom they lived as fugitives (foreigners) and were treated like slaves, two fixed ideas in the later Israelite understanding of the origin of the nation".

37 Cf. my reconstruction of the early history of Israel in LEMCHE, *Det gamle Israel*, pp.66-102. 112-121. This explanation follows a model which clearly differs from the usual immigration hypothesis but also from the revolution hypothesis as correctly identified by E.OTTO, though OTTO of course does not agree with my model, cf. his *Historisches Geschehen - Überlieferung - Erklärungsmodell*, BN 23 (1984), pp.63-80, p.69.