

8

Review Essay of Donald B. Redford, *Egypt, Canaan, and Israel in Ancient Times*

Gary A. Rendsburg

One does not have to read far in Donald Redford's *Egypt, Canaan, and Israel* to realize the vast amount of work which went into researching this book.¹ The author is primarily an Egyptologist, but the source materials utilized go far beyond the bounds of the Nile Valley. Archaeological fieldwork in the Levant and the Aegean; literary remains from Hatti, Ugarit, and elsewhere; classical sources such as Herodotus, Strabo, and others; the literature preserved in the Bible; and various and sundry other types of evidence are all brought to bear on the subject at hand. In short, the book is most promising, especially for someone who deals primarily with the West Semitic world. For here is a renowned Egyptologist, with interests in Western Asia as well, synthesizing all of

¹This essay is a review of Donald B. Redford, *Egypt, Canaan, and Israel in Ancient Times* (Princeton, 1992: Princeton University Press). It is based on an oral version presented at the Egyptology and the History and Culture of Ancient Israel Group at the Annual Meeting of the Society of Biblical Literature, November 24, 1992, in San Francisco. I take this opportunity to express my thanks to James K. Hoffmeier for inviting me to participate in the session, and to Kenneth G. Hoglund, Susan T. Hollis, and Alberto Green with whom I shared the podium in a panel discussion devoted to this book. I have changed little of the oral presentation in this written version. Footnotes have been added, but they have been kept to a minimum.

the Egyptian source material in conjunction with the aforementioned types of evidence.

The book is divided into four parts: Part One, "Egypt and the Levant from Prehistoric Times to the Hyksos" (5 chapters = 120 pp.); Part Two, "The Egyptian Empire in Asia" (3 chapters = 113 pp.); Part Three, "The Great Migrations" (2 chapters = 40 pp.); and Part Four, "Egypt and the Hebrew Kingdoms" (5 chapters = 187 pp.). In this review, appearing as it does in a volume devoted to the study of ancient Judaism, I will concentrate on the latter two parts of the book, for they are the sections of deepest interest to students of the Bible. Moreover, they contain the most controversial elements of Redford's analysis.

For while one may quibble with Redford here and there on a particular point in Parts One and Two, and while one even may challenge his reconstruction of major sweeps of history (for example, the Hyksos), no one would deny that Redford has treated the sources with objectivity and with skill. Unfortunately, the same cannot be said about his treatments of Parts Three and Four, especially when he turns his attention to the biblical material.

I shall deal with Redford's interpretation of the biblical evidence below in detail. First, however, I would like to review his treatment of the Sea Peoples, a topic of special interest to biblical scholars attuned to archaeology and devoted to reconstructing early Israelite history. Redford summarizes all the evidence at our disposal concerning the Sea Peoples; and there is little here that one would wish to criticize. Redford's analysis is judicious, even a bit conservative at times.

For example, on the crucial question of exactly what sent the Sea Peoples into motion in the first place, Redford simply presents the various theories without bias. He states plainly, "The ultimate causes of the movement are difficult to assess" (p. 244), after which he presents the main hypotheses: economic weakness in Mycenae, which led the peoples there to strike out after the raw materials needed, instead of relying on importing them; natural causes such as famine and/or bubonic plague; and political factors such as revolts of the western vassals of the Hittites and the southward movement of peoples from Thessaly and Thrace into the region of Mycenae. There is good evidence for the last of these three, the political angle; there is some evidence for the famine hypothesis; and while there is little real evidence for the first of these theories, it is hard to imagine that economics did not play a role in setting the Sea Peoples into motion. Accordingly, we need not choose among these various options. The study of history has advanced to the position today where more than one cause for a single effect is countenanced. So, Redford does not choose one view over its

competitors, and, though he does not say so explicitly, it appears that he is willing to go along with the notion of multiple causation.

On the other hand, there are times when Redford is quite bold in his assessment. Take the following sentence: "There is no reason to doubt that the Egyptian court was at all times during the Mycenaean age in correspondence with the court at Mycenae, although the letters have not as yet been recovered" (pp. 242-243). I agree with Redford, but it is quite striking to see such a statement so boldly proclaimed in print. I can only wonder what kind of attack would be leveled against my colleague Martin Bernal if he were to make such a claim;² but coming from Redford's pen I trust that scholars may be more willing to accept the fact that the two courts of Egypt and Mycenae were in written contact *at all times*.

As with all treatments of the Sea Peoples, Redford spends a good amount of time attempting to identify the various components, both where they came from and where they wound up. At times I would have liked more information. Consider the following two bits: "Some of the Shekelesh sailed westward to Sicily (to which they gave their name), the Da'anu may have found a haven on Cyprus" (p. 256). Oftentimes I have read the claim that the Shekelesh eventually made it to Sicily (or the opposite, that the Shekelesh originated from Sicily).³ But I am left desiring more data here. Is the whole basis of this theory the similarity of the names Shekelesh and Sicily? If so, this should be stated. Or is there additional evidence which bolsters this view? Not being an expert on Sicilian origins, I simply do not know the answers to these questions, though it is something I have wondered for some time, long before I read Redford's book.⁴

²See M. Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization, Volume I: The Fabrication of Ancient Greece 1785-1985* (London, 1987: Free Association Books); and M. Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization, Volume II: The Archaeological and Documentary Evidence* (London, 1991: Free Association Books); and two more volumes still to appear. For a review of some of the evidence put forward by Bernal, see G.A. Rendsburg, "Black Athena: An Etymological Response," *The Challenge of "Black Athena," Arethusa, Special Issue*, ed. M. M. Levine (1989): 67-82.

³Redford (p. 256, n. 72) disagrees with W.F. Albright who held that it was the Tjekker whose name eventually yielded Sicily; his corrective is in order. Incidentally, greater editorial control should have been exercised in this book. The Tjekker appear as "Tjekru," "Tjakru," "Tjekker," and "Tjakkar" in this book; both "Washosh" and "Weshesh" are used; etc.

⁴See the brief formulation of the problem by R.R. Holloway, "Italy and the Central Mediterranean in the Crisis Years," *The Crisis Years: The Twelfth Century BC*, ed. W.A. Ward and M.S. Joukowsky (Dubuque, Iowa, 1992: Kendall/Hunt), p. 43 (reference courtesy of David I. Owen).

As to Redford's statement about the Da'anu (=Danuna) perhaps winding up on Cyprus, here again one would like more information. As far as I am aware, the only evidence pointing to such a conclusion is the attestation of the term Yadnana in Assyrian records of the eighth and seventh centuries B.C.E. (reigns of Sargon II and Esarhaddon), with reference to Cyprus (and supposedly representing "Ia-Danana," i.e., island of the Danuna). This is rather slim evidence on which to base the claim that "the Da'anu may have found a haven on Cyprus." Furthermore, once having made the statement, I would expect some supporting documentation, yet no reference is made to the Assyrian term Yadnana, nor is there even a footnote directing us to some other source. In the end, it is important to recall the caveat of R.D. Barnett, "no archaeological proof of Danuna settlement on Cyprus has so far been found."⁵

Personally, I accept the view of Cyrus Gordon and Yigael Yadin that the Danuna settled on the coast of Canaan between the Philistines to the south and the Tjeker to the north at Dor (see the Tale of Wenamon), and in time joined the Israelite confederation as the tribe of Dan. Later in the book Redford refers to this 'ingenious' view (p. 296),⁶ but states: "There is no extra-Biblical evidence for the settlement of a large remnant of this community in southern Palestine. Until the time that such evidence is forthcoming, it is better to reserve judgment" (ibid.). However, since clearly there is even less evidence pointing to the arrival of the Danuna on Cyprus, Redford would be wise to reverse his position here. The biblical evidence for the Danuna/Dan connection is both quantitatively and qualitatively more impressive than the rather weak evidence for the Danuna/Cyprus connection.

I turn now to those portions of the book which deal more directly with the Bible. I am forced to state very matter of factly: Redford is a first-class Egyptologist, but his treatment of the biblical material is extremely weak and is encumbered by pure biases and prejudices.

Let us have a look at two of the analyses of biblical items. Redford states that *ssy-r*, the sobriquet of Ramesses II, was transmogrified into the historical hero Sisera of Judges 4-5. Now I am not here to proclaim that beyond doubt Sisera was a Canaanite general whose chariotry terrorized the locals of the region between Hazor and Megiddo, for

⁵R.D. Barnett, "The Sea Peoples," in *Cambridge Ancient History* II/2, 3rd ed. (Cambridge, 1975: Cambridge University Press), p. 377 (reference again courtesy of David I. Owen).

⁶However, Redford cites only Yadin, not Gordon (though it was the latter who first proposed this view). See Cyrus H. Gordon, "The Mediterranean Factor in the Old Testament," *Congress Volume Bonn 1962* (Supplements to *Vetus Testamentum* 9; Leiden, 1963: E.J. Brill), p. 21.

naturally apart from Judges 4-5 we have no evidence about this man. But given the choice between a blanket acceptance of the biblical record and between Redford's attempt at prestidigitation, I much prefer the former. I know of not a single biblical scholar today who would countenance Redford's suggestion,⁷ and yet he maintains it as if it were standard doctrine.

The second example I wish to bring is Redford's understanding of Judges 8:14. As background, I need to point out that according to Redford, the origin of the Israelites is to be found in the Shasu group (more on this below). Thus, have a look at the following sentence: "As there is no reason to believe the Shasu were literate – Judges 8:14 curiously provides a correct reflection of this – there is no reason to expect any contemporary documents from them describing their way of life" (p. 275). Putting aside for the moment that the Israelites were at one time Shasu (again, see below), let us zero in on Judges 8:14 which reads: "[Gideon] took a *na`ar* from among the people of Succoth and requested of him, and he wrote for him [a list of] the officials and elders of Succoth, seventy-seven men."

This passage is the center of a debate as to how literate ancient Canaan was at the end of the second millennium, with positions spanning the spectrum between the maximalist view and the minimalist view. But Redford reads it in a way that no scholar understands it. Presumably, he believes that since Gideon had to ask someone to write down the list of the city elders, Gideon himself was illiterate but the Succoth *na`ar* was literate. And if a leader like Gideon was illiterate, then the same must be true of the general Israelite or Shasu population. But to read illiteracy into Gideon's character on the basis of this passage is to show a wholesale misunderstanding of the material.

As a parallel, let us recall the story of David and Bathsheba. David writes a letter to Joab at the battlefield, but Joab responds by sending a messenger back to David at the palace. Would one conclude from this that David was literate but that Joab was illiterate? Of course not. First of all, one must assume that Joab could read the letter he received from David, and secondly, one must take into account the fact that the general at the front simply does not have the time to write letters; it is simpler to dispatch a messenger.⁸ And so it is with Gideon; he does not have the time to listen to the *na`ar* dictate a list of seventy-seven names and then

⁷And indeed his only quoted source for this view is H. Gauthier, *Le livre des rois d'Egypte*, vol. 3 (Cairo, 1914: L'Institut Français d'Archéologie Orientale), pp. 73-74.

⁸See M. Sternberg, *The Poetics of Biblical Narrative: Ideological Literature and the Drama of Reading* (Bloomington, Indiana, 1985: Indiana University Press), p. 215: "Joab, with the whole war on his hands, has no time for epistolary composition."

to write them down himself. But if Gideon requested such a list in writing, one can only conclude that he could read it as well. In short, the one thing that Judges 8:14 emphatically does not show is that the people involved, be they Shasu or whoever, were illiterate.

Redford devotes far too much space to knocking down the chronology presented in the early books of the Bible. I agree with him that the well-known 400-, 430-, and 480-year figures are not to be taken seriously, nor are the fantastic ages of the patriarchs to be viewed with any validity. It is regrettable that there are scholars who have reconstructed the early history of Israel on the basis of these numbers, but in a work such as *Egypt, Canaan, and Israel* there is no need to deliberate on them to the extent that Redford does (e.g., pp. 258-260). The explanation for Redford's devotion of such space to this issue is not hidden in the book. The author makes his bias clear in his dismissal of "the traditional claims of inerrancy made by conservative Christianity on behalf of the Bible" (p. 258), and in his statement: "Crypto-orthodox tendencies drive some scholars to ludicrous ends" (p. 260, n. 11).

I am in full agreement with him on these scholarly matters. I part company, however, only in questioning how germane the religious perspective of some of the scholars he cites is to the project at hand. The aims of this book lie elsewhere: "to chronicle as empirically as possible the nature and extent of the relationship between Egypt and hither Asia" and "to try to ferret out the causes that might be elicited on the basis of the extant evidence" (p. xxii). That is what the reader expects to find in this volume, and by and large that is what one does find. When Redford deviates from this track with comments such as the above, it is distracting and annoying. My recommendation would be, if Redford is so inclined, that he write a reflective essay on how the personal views of scholars affect their scholarship. Moreover, if we follow the view that there is no such thing as objective history writing, then Redford's personal views presumably inform his scholarship as well. In this volume I would like to see historical issues debated, not ideological stances.

Of like regard is Redford's devotion of five pages to the peasant revolt theory of George Mendenhall and Norman Gottwald (pp. 265-269). He calls the former's original proposal an "ingenious notion" (p. 265) and characterizes the latter's work as "like a breath of fresh air" (p. 266). Gratefully, Redford goes on to show the flaws in this whole approach. But it is interesting to contrast his treatment of Mendenhall and Gottwald with his aforementioned words about conservative Christianity and crypto-orthodoxy. Nowhere do the ideological underpinnings of Mendenhall and Gottwald come into play in the discussion, though certainly they are at least as strong as the ideology which informs those who wish to

accept, in whole or in part, the chronology presented in the Bible. I do not shy away from calling Mendenhall's position anti-Semitic; by transforming Israel into a group of Canaanites, the idea of a Jewish nation dissipates.⁹ Gottwald's agenda is based on his avowed Marxist view of world history: nations come into existence only through economic struggle, for example, peasants overthrowing their repressive overlords. None of this is indicated in Redford's treatment of the peasant revolt model. On the contrary, though he knows the primary material well enough to see the flaws of the model, in large part his words (some of which are quoted above) are sympathetic to Mendenhall and Gottwald. There can be only one reason for Redford's contrasting treatments of Mendenhall and Gottwald, on the one hand, and the crypto-orthodox (I content myself with Redford's term), on the other. No matter how misguided the former may be, as they seek to overthrow what is stated in the Bible (i.e., the conquest model), their view is worthy of serious consideration, not derisive comment.

One specific point in this regard: In his desire to be praiseworthy to the peasant revolt model, Redford overstates the case. He writes that "the cultural continuum between the Canaanite LB II and Iron I is a fact" (p. 268). Obviously, there has been much debate on this issue. To state the view of the one side, without presenting the evidence, especially the goodly amount of evidence that runs counter to Redford's view, is unbalanced. I can summarize the other side of the argument with one sentence written by Israel Finkelstein: "the material culture of indubitably Israelite sites, those in the central hill country, is completely different from that of the Canaanite centers."¹⁰ A prime example of distinctive Israelite material culture is the elliptical layout of Israelite Iron Age I sites, in contrast to the traditional cities of Canaanite Late Bronze Age sites.

I come now to Redford's main point in this discussion: The Israelites are Shasu who settled down. Redford summarizes the references to the Shasu in Egyptian texts from the New Kingdom, and concludes that they were seasonal nomads whose homeland was in Moab and Edom. He accepts the reading 'Yhw in the land of the Shasu' from the Soleb and Amarah lists as a reference to Yahweh, and correlates with it the biblical references to Yahweh coming forth from Seir/Edom. "The only reasonable conclusion is that one major component in the later amalgam

⁹See B. Halpern, "Sociological Comparativism and the Theological Imagination: The Case of the Conquest," *Sha'arei Talmon: Studies in the Bible, Qumran, and the Ancient Near East Presented to Shemaryahu Talmon*, ed. M. Fishbane and E. Tov (Winona Lake, Indiana, 1992: Eisenbrauns), pp. 53-67, especially pp. 61-62.

¹⁰I. Finkelstein, *The Archaeology of the Israelite Settlement* (Jerusalem, 1988: Israel Exploration Society), p. 338.

that constituted Israel, and the one with whom the worship of Yahweh originated, must be looked for among the Shasu of Edom already at the end of the fifteenth century BC" (p. 273). The argument is sound, though naturally it neither can be proved nor disproved to the extent that would satisfy any historian.

This being the case, what does one do with the main narrative in the Bible? The story, of course, locates early Israel in Egypt doing corvée labor, leaving at a point conducive to their escape, wandering through the desert (including Edom), and arriving in Canaan from the east. Redford attaches no historical value to the biblical account whatsoever. Instead, he concludes that the biblical record is based not in Israelite (i.e., Shasu) history, but in the *mémoire collective* of the Hyksos expulsion from Egypt among all peoples in Canaan. In Redford's words: "There is only one chain of historical events that can accommodate this late tradition, and that is the Hyksos descent and occupation of Egypt...The memory of this major event in the history of the Levant survived not only in Egyptian sources. It would be strange indeed if the West Semitic speaking population of Palestine, whence the invaders had come in MB IIB, had not also preserved in their folk memory this great moment of (for them) glory. And in fact it is in the Exodus account that we are confronted with the 'Canaanite' version of this event" (p. 412). That is to say, the Shasu settled down in the central hill country of Eretz-Israel, and in time fell heirs to the great folk traditions of all peoples in the area. The greatest of those folk traditions harked back to the Hyksos, and the story of their descent to Egypt and return to Canaan was adopted as the foundational statement of Israelite history.

There is no need to enter into a detailed critique of this view. To be kind, one could say that this reconstruction of events is plausible, but again, it cannot be proved nor disproved to any satisfactory extent. To be unkind, one could say about Redford what he himself says about others with whom he disagrees: "This, however, has become such an exercise in ingenuity and a travesty of methodology that it ceases to amuse" (p. 354, n. 165). For as even as great a skeptic as Gottwald has written, "There is an indeterminate measure of historical plausibility in the biblical report that Israelites migrated from Egypt to Canaan,"¹¹ and of courses others such as Raphael Giveon have been more forceful in arguing for the Exodus as "an historical event, although there is little to prove it outside the literary tradition of the Bible."¹²

¹¹N. Gottwald, "The Israelite Settlement as a Social Revolutionary Movement," in *Biblical Archaeology Today* (Jerusalem, 1985: Israel Exploration Society), p. 36.

¹²R. Giveon, "Archaeological Evidence for the Exodus," *Bulletin of the Anglo-Israel Archaeological Society* (1983-84): pp. 42-44, in particular p. 43.

The argument for a historical Eisodus and Exodus is not a case of special pleading (so Redford would have it). On the contrary, there is quite a bit of evidence, indirect though it may be, pointing in this direction. But before discussing it, I need to state very clearly my view of the biblical record. It is first and foremost an idealized history of Israel. Nations simply do not descend from the offspring of one man, and all Israel did not experience the Eisodus and Exodus as the Bible portrays. I already have stated that the Danites were a segment of the Sea Peoples who in time came to join the Israelite tribal league. Similarly, if we assume that *isr* in Papyrus Anastasi I refers to Asher (this is the most likely possibility), then here is another example of an Israelite group with no experience in Egypt. But to move from these points to a denial of the entire story as a historical reality is too great a jump.

I list here a few items to consider, all of them well known:

1. Papyrus Anastasi VI includes the report of a frontier official from the reign of Merneptah: "We have just finished letting the Shasu tribes of Edom pass the fortress of Merneptah Hotepirmaat l.p.h. which is in Tjeku, to the pools of Per-Atum of Merneptah Hotepirmaat, which are in Tjeku, to keep them alive and to keep their cattle alive." If Redford is correct that the Shasu in time evolved as Israel, then here is a striking parallel between the Shasu of historical record and the Israelites of the Bible. If the Shasu and the Israelites are to be disassociated, then in the least we have a general parallel between the historical record and the biblical narrative. The picture emerges that Egypt did not object to opening its doors to Semites in search of food and grazing area. And of course both the Egyptian text and the Bible places these Semites in Per-Atum = Pithom.
2. Papyrus Leiden 348, dated to the reign of Rameses II, includes the instructions: "Issue grain to the men of the army and to the 'Apiru who are drawing stone for the great pylon of Rameses II."¹³ We cannot go into the whole 'Apiru/Habiru/Hebrew problem here, but in view of a passage such as this one, I am inclined to see some ultimate connection between the Hebrews of the Bible (see Exod. 1:11 in particular) and the 'Apiru of at least this Egyptian text.
3. The Merneptah Stele refers to an entity named Israel as a group of people without a land. While many have

¹³See M. Greenberg, *The Hab/piru* (New Haven, 1955: American Oriental Society), p. 56.

interpreted this reference to an Israel wandering through the desert or settling down in the land of Canaan, I prefer to interpret Israel in this inscription, with a determinative different from that of the others in the list of Canaanite peoples, as an Israel enslaved in Egypt.¹⁴ The scribe knew that the Israelites originated in Canaan, and thus while proclaiming Merneptah's victories in the Levant, he included Israel in the list of vanquished peoples.¹⁵ Redford, of course, believes that the Merneptah Stele is to be discounted: "The sort of triumphal sweep of arms which the above snippet of poetry conjures up is quite unhistorical, and the passage must be rejected as a reliable source."¹⁶ But the evidence put forward by Ithamar Singer and others shows that the Merneptah Stele can be trusted as a historical document.¹⁷

4. Papyrus Anastasi V, dated to the reign of Seti II, includes the report of a frontier official concerning the pursuit of two escaped slaves who passed in the vicinity of Migdol. Again, this is strikingly parallel to what we read in the Bible: an Egyptian force is dispatched to pursue a group of escaped (freed?) slaves passing in the vicinity of Migdol (see especially Ex. 14:1).¹⁸

Items 1 and 4 most likely are not related directly to the Israelites. Items 2 and 3 most likely are related directly to what is stated in the Bible. But whether of direct or indirect relationship, taken together these four items show that the basic story line outlined at the end of Genesis and at the beginning of Exodus is based on the historical reality of the

¹⁴As H. Engel, "Die Siegestele des Merneptah," *Biblica* 60 (1979): 346-347, pointed out, this opinion was held by some scholars in the years immediately following W.M.F. Petrie's discovery of the stele. It is mentioned again, as one of several options, in the recent work of H. Sourouzian, *Monuments du Roi Merneptah* (Mainz, 1989: Von Zabern), p. 215 (reference courtesy of John A. Emerton; I have not been able to consult this work).

¹⁵For more details, see G.A. Rendsburg, "The Date of the Exodus and the Conquest/Settlement: The Case for the 1100s," *Vetus Testamentum* (forthcoming).

¹⁶D.B. Redford, "The Ashkelon Relief at Karnak and the Israel Stela," *Israel Exploration Journal* 36 (1986): 199.

¹⁷I. Singer, "Merneptah's Campaign to Canaan and the Egyptian Occupation of the Southern Coastal Plain of Palestine in the Ramesside Period," *Bulletin of the American Schools for Oriental Research* 269 (1988): 1-10; and G.A. Rendsburg, "Merneptah in Canaan," *Journal of the Society for the Study of Egyptian Antiquities* 11 (1981): 171-172, with corrigenda published as Supplement to *JSSEA* 12 (1982).

¹⁸If Tjeku in the inscription is the same as Succoth (Ex. 12:37, 13:20), then there is another toponym shared by the Egyptian document and the biblical description.

New Kingdom period. Problems remain, some of which are addressed in my forthcoming article on the date of the Exodus (cited above, n. 15). But this approach to the subject at least is based on real evidence, as opposed to Redford's totally invented view of the biblical account as a story based on amorphous traditional elements related to the Hyksos.

Lest one think that Redford places no credence only in the biblical traditions about early Israel, it is important to state that the remaining portions of the Bible fare no better in his estimation. For Redford, the biblical sources for the reigns of David and Solomon (the Succession Narrative, the deeds of Solomon, etc.) also have no value for historical reconstruction. The following statement will suffice as demonstration: "Some day evidence may be produced on Solomon's trade in horses or on his marriage to Pharaoh's daughter. Until then these must remain themes for midrash or fictional treatment" (p. 311).

One final illustration: In a discussion of the Twenty-fifth Dynasty monarch Taharqa (690-664 B.C.E.), Redford states that this king "is wrongly identified in the Biblical narrative (2 Kings 19:9)" (p. 258; see also pp. 353-354, nn. 163, 165). First of all, Redford overstates the case.¹⁹ Secondly, to jump from this point to the conclusion that "Biblical writers of the seventh and sixth centuries BC lacked precise knowledge of Egypt as recent as a few generations before their own time" (ibid.) is absurd. On the contrary, the great majority of historians of the period has been impressed with just the opposite of what Redford claims: the picture which emerges from an objective reading of 2 Kings, Jeremiah, and other books is one of remarkable accuracy.²⁰ Redford's negativism on this point is most regrettable.

In light of Redford's evaluation of the biblical material, it is rather shocking to read that "Aramean enclaves...in the tenth century were in

¹⁹To some extent Redford's statement is correct, for at the time of Sennacherib's invasion in 701 B.C.E. (the setting of 2 Kings 19), Taharqa (Tirhakah in English Bibles) was not yet king of Egypt. However, note that the Bible does not claim he was *melek misrayim*, rather he is called *melek kûš*. Moreover, solutions are forthcoming; see K.A. Kitchen, *The Third Intermediate Period in Egypt (1100-650 BC)*, 2nd ed. (Warminster: Aris & Phillips, 1986), pp. 154-161, 383-387; N. Na'aman, "Sennacherib's Campaign to Judah, and the Date of the *Imk* Stamps," *Vetus Testamentum* 29 (1979): 61-86; and M. Cogan and H. Tadmor, *II Kings* (New York, 1988: Doubleday), p. 234.

²⁰K.A. Kitchen, "Late Egyptian Chronology and the Hebrew Monarchy," *Journal of the Ancient Near Eastern Society* 5 (1973): 225-231; A. Malamat, "The Twilight of Judah: In the Egyptian-Babylonian Maelstrom," *Congress Volume Edinburgh 1974* (Supplements to *Vetus Testamentum* 28; Leiden, 1975: E.J. Brill), pp. 123-145; J.K. Hoffmeier, "A New Insight on Pharaoh Apries from Herodotus, Diodorus and Jeremiah 46:17," *Journal of the Society for the Study of Egyptian Antiquities* 11 (1981): 165-170; etc.

the process of founding powerful states at Hamath, Geshur (in north Palestine), and especially at Damascus" (p. 298). Now, outside of the Bible there exists no evidence for the Aramean state of Geshur. The references in 2 Samuel 3:3, 13:37-38, 15:8, etc., are the sole basis for positing the existence of such an entity.²¹ One wonders on what basis Redford affirms the presence of an Aramean state in Geshur in the early first millennium B.C.E. I hope it is not the one-word Aramaic inscription *lšqy'* found at Ein Gev in a ninth-century B.C.E. context.²² If it is the Bible, then we are left with a quandry, for in Redford's words: "Use of the Succession Document as historical source at face value renders any investigation, no matter how erudite, a mere exercise, devoid of substance" (p. 306, n. 100). To carry this line of argument to an absurd end, Redford would have us believe that 2 Samuel and 1 Kings inform us more about the historical reality of the Aramean state of Geshur than about the historical reality of the kingdom of Israel under David and Solomon.

My final point is again a disturbing one, but I refrain from any attempt at detailed analysis. I trust that a mere presentation of Redford's words will point the way. One can only imagine what lies behind the author's statement: "The rules he [Yahweh] laid down...were Draconian in the extreme, and the deity's will utterly barbaric" (p. 276); or the reference to "the brutal nature of Yahweh" (p. 277); or the final summation: "An honest reading of the account of Exodus and Numbers cannot help but reveal that the tyranny Israel was freed from, namely that of Pharaoh, was mild indeed in comparison to the tyranny of Yahweh to which they were about to submit themselves. As a story of freedom the Exodus is distasteful in the extreme...and in an age when thinking men are prepared to shape their prejudice on the basis of 3,000-year-old precedent, it is highly dangerous" (p. 422).

Exodus, Canaan, and Israel is a book of great potential. Portions of it (mostly Parts One and Two) fill a real need for the scholar devoted to the history of Western Asia and Egypt. Unfortunately, the biases brought to Parts Three and Four, specifically in the author's treatment of the biblical material, mar the book beyond repair. In the final analysis, it cannot be recommended for the scholar desiring information on the history of Israel or on the traditions of the Bible.

²¹El Amarna letter 256 refers to the land of *Ga-ri*, which may be an error for *Ga-su-ri*, but otherwise no extrabiblical source attests to this toponym. See S.E. Loewenstamm, "Geshur, Geshuri," *Enšiqlopediya Miqra'it* 2 (1954): 568.

²²B. Mazar, A. Biran, M. Dothan, and I. Dunayevsky, "Ein Gev Excavations in 1961," *Israel Exploration Journal* 14 (1964): 27-29. After this essay was completed, I learned of a new inscription found recently at Tel Hadar, but it, too, makes no mention specifically of "Geshur."

SOUTH FLORIDA STUDIES IN THE HISTORY OF JUDAISM

Edited by
Jacob Neusner
William Scott Green, James F. Strange

APPROACHES TO ANCIENT JUDAISM

edited by
William Scott Green

Editorial Board
Alan J. Avery-Peck
Paul V. McCracken Flesher
Jacob Neusner
Gary G. Porton
James F. Strange

Number 110
Approaches to Ancient Judaism
New Series

Volume Seven

edited by
Jacob Neusner

APPROACHES TO ANCIENT JUDAISM

New Series

Volume Seven

edited by

Jacob Neusner

Scholars Press
Atlanta, Georgia

1995