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*THE CHARACTER OF THE PROPER NAMES IN
THE PRIESTLY CODE: A REPLY TO PRO-
FESSOR HOMMEL.*

IN his great commentary on the Hexateuch¹ Dillmann defended the antiquity of the personal names in the Priestly Code against Wellhausen, who had found many of them to be late and artificial compounds. Prof. Hommel, in his recent work,² returns to the same subject, and derives from these names the main argument in an attack which, he confidently asserts, has overthrown the Wellhausen school of criticism. I propose in the present article to confine myself to an examination of this particular argument. It plays so large a part in Hommel's attack that it may well receive independent treatment, and the more so because the way in which he has approached the question has seriously obscured the issue. The question is not—Does the Priestly Code contain ancient material? For that, particularly in the case of the names, is inconclusive. The crucial question is—Does it contain nothing but what is in every respect ancient? In other words—Was it *compiled* late or early?

In the preface to the English edition of his book Prof. Hommel refers to a recent work of my own³ in which I also was compelled to consider the historical character of the proper names in the Priestly Code. My conclusion with regard to this—it is far from being, as Prof. Hommel describes it, the main conclusion of my work—was that many of the names were genuine and ancient, but that some were of much more recent formation, and that

¹ See more particularly on Num. i. 5-15.

² *Ancient Hebrew Tradition*, published simultaneously (May, 1897) in England and Germany.

³ *Studies in Hebrew Proper Names*, 1896 (November).

certain *lists* of names (such *e.g.* as we find in Num. i., xiii.) were artificially compiled at a late period, since among the names that composed them, names of certain types—rare, though not in all cases unknown, in early times—occurred in much larger numbers than would have been the case in genuine ancient lists.

Unintentionally no doubt, but none the less actually, Prof. Hommel refers to my book in very misleading terms. I lay no great stress on the fact that he considers that “it indicates in its main conclusion a retrograde movement when compared with Nestle’s work,” for retrogressive in such a connection simply means less in accordance with the writer’s own views. And, again, I am willing to believe that the printer may be partly answerable for attributing to me a sentence I never wrote; certainly the sentence placed in inverted commas in Prof. Hommel’s Preface, as though cited from me, is not mine. But I must warmly protest against the direct statement and a serious implication of the last two sentences of the Preface. Prof. Hommel’s book is not, as he there asserts, a reply to my contention; for it never even approaches the main and crucial part of my argument. Granting—what indeed is very far from being the case—that Prof. Hommel had proved that every single individual name in P was ancient, the unusual proportion of the compounds with *el* to the whole number, and the large proportion of a certain type of these compounds themselves would still demand explanation. My own explanation may or may not be right, but Prof. Hommel has neither shewn that it is wrong nor offered any alternative explanation. The implication of which I complain is that my book was written with a disregard of “material obtained from the inscriptions.” The implication is false. I wrote throughout with constant reference to the valuable comparative data obtainable from these sources.

I have drawn attention to this Preface because it is undesirable that the question of the names in the Priestly Code should be prematurely closed. Whichever view be taken of the names and of the lists which they compose—that they are in every respect ancient or that they are to a greater or less degree modern—they are marked by striking peculiarities which have not yet been completely and satisfactorily explained, and many of which have not even been considered by Prof. Hommel.

With a view then to a further elucidation of these peculiarities, I will discuss in detail the differences between Prof. Hommel's explanation of these names and my own. In the first place our mode of approach is different. "One of the main objects" of Prof. Hommel's book "has been to adduce external evidence (*i.e.* from contemporary inscriptions) to shew that even from the time of Abraham onwards personal names of the characteristically Mosaic type were in actual use among a section of the Semites of Western Asia" (p. x.). And this, be it observed, is all that Prof. Hommel either has done or, with our present resources, can do: he cannot adduce external evidence to prove that the Hebrews in the time of Moses used such names as are attributed to that period in P: for such external evidence does not exist. We have no Hebrew inscriptions of the period. It is precisely the absence of direct external evidence that leaves, and for the present must leave, many matters, matters of inference rather than of fact. My own discussion of these names is, on the other hand, subsidiary. My main purpose was to trace the history of Hebrew names; for this purpose it was necessary to confine one's attention in the first instance to literature that was generally recognised as being approximately contemporary with the persons named—in other words, to exclude Chronicles, a work admittedly of no earlier date than the 3rd century B.C., and of P, whose date was matter of dispute. Then with

the results so obtained the character of the names in Chronicles and P had to be separately compared. The comparison brought to light conspicuous dissimilarities which I explained in the case of Chronicles as being due to a large admixture of late family and personal names, and in the case of P partly to the presence of some late names, but even more to the names (in particular lists) having been selected from various sources to the exclusion of all compounds with *yah*, but with a preference for compounds with *el*.

One consequence of this difference in approaching the subject is that it is difficult to be quite sure how far Prof. Hommel and myself are in agreement. Thus, while my proof of the late character of many of the Chronicler's names is independent of my view of the names in P, my judgment with reference to P's names does *in part* depend on my previous conclusions with reference to the Chronicler's, and there is nothing to shew whether Prof. Hommel would accept those conclusions, and, if not, on what grounds he would reject them. All that he has to say on the subject in the present work¹ is that the names in 1 Chronicles contain much ancient material (p. 302 n.)—a conclusion with which I fully agree, and which I had anticipated Prof. Hommel in stating (*Hebr. Prop. Names*, pp. 233 ff.). But the crucial question is—How much and what of this material is ancient? The failure to deal with this question vitiates to no small extent, in my judgment, the method of Prof. Hommel's discussion. For he attempts to defend the antiquity of a certain set of Hebrew names without any adequate reference to the history of Hebrew names in general. His

¹ In earlier works Prof. Hommel has spoken very unfavourably of the general historical value of Chronicles. In *Ed. Glaser's hist. Ergebnisse u.s.w.* (1889) he pronounces the notices peculiar to Chronicles as resting on "halb tendenziöser, halb naiver freier Erfindung," p. 5. Cf. also *Aufsätze u. Abh.* (1892), pp. 3, 49.

recurring mode of argument is this—Such and such a name was in use at a certain period among the South Arabians; ¹ therefore the same name or a name of the same or similar formation or significance was in use at the same time among the Hebrews. Such an inference will, under certain circumstances, possess probability; but when it conflicts with direct Hebrew evidence it is without weight, and when it is contradicted by inferences from the history of Hebrew names it is most unsafe. In other words, due weight must be given to both the historical and the comparative methods, to the inferences suggested by the history of Hebrew names, as well as to the inferences suggested by more or less contemporary non-Hebrew names. It would be disastrous to abandon the former at the call of Prof. Hommel to range ourselves under the banner of “external evidence,” since the existing “external evidence,” valuable as it is, is, so far as the present subject is concerned, indirect. Had Prof. Hommel only given due consideration to the historical method, he would, as I shall show below, have seen that even what he terms “external evidence” offers its own weighty contribution against the view he was propounding.

There are, however, definite points of agreement between us to which it may be well briefly to refer. We are agreed that compounds with *ab* (father), *ah* (brother) are ancient formations which comparatively early (say before the 8th century B.C.) fell into disuse among the Hebrews; and further, that compounds with *am* (a kinsman) belong to the same early period, my main difference here being

¹ The Arabian names cited by Hommel are drawn from two main sources. (a) The South Arabian Inscriptions. According to the highest estimate, these date from 900 B.C., and later, and in the case of one group (the Minæan) according to Glaser, from something prior to 1000 B.C. (b) Babylonian contract tablets from as early as circ. 2000 B.C. Hommel's argument for the S. Arabian character of these names appears to me strong, and, at least for the purposes of this article, will be accepted.

that I consider a single apparent instance (Ammi-Shaddai) open to some suspicion of artificiality. Again, we agree that compounds with *ba'al*, *melek*, and *adon* came into use after the entry into Canaan and fell into disuse before the Exile. Only in our interpretation of the first of this class of names we widely differ, for I notice with great surprise that Prof. Hommel (like Kuenen) takes Baal¹ in these names to be the proper name of a rival deity to Jehovah, and not, as I have concluded with many others² on what I believe to be very convincing evidence, a title applicable to Jehovah as well as other gods.

The points of agreement to which I have just referred have in the main been long established. But I carried the analysis of certain classes, especially of the various formations with *el* and *yah*, much further. This has not been observed by Prof. Hommel, with the result that many of my conclusions with which he disagrees rest on evidence he has not considered.

Even when we come to the names in P there is still some common ground between us. Of the five characteristics which I attributed to these names, Prof. Hommel agrees with me in regard to three, and differs in regard

¹ From this it would follow that David, who calls one of his sons Baaliada (=Baal takes knowledge) worshipped Baal as well as Jehovah. Prof. Hommel (p. 304) glides very gently over this point, and makes the statement that the name was altered to Eli-ada "probably during David's lifetime." But the probability hardly rests on anything but the assumption that David cannot have remained a Baal-worshipper all his life. As Prof. Hommel adduces no new evidence for his view, I must differ from him, as I ventured to differ from Kuenen, and decline to admit that David ever recorded in one of his children's names his devotion to another god besides Yahweh. The other passage (pp. 225 f.) in which Prof. Hommel refers to Baal names is exceedingly misleading. No one has ever disputed, so far as I am aware, that there was a great "struggle between the cult of Yahweh and the Canaanite cult of Baal." Again, the very questionable theory of the relation between Yahweh and Yah (that the latter is the original and the former the derivative form) has not a vestige of support in the traditional account.

² *E.g.* Bœthgen, *Beiträge*, pp. 141-4; Schultz, *Old Testament Theology*, I. 148; König, *Hauptprobleme*, pp. 35 f. See further the note in my *Hebr. Prop. Names*, p. 138.

to two. He clearly agrees with me, though no doubt he would express himself in somewhat different language—that (1) “the names—even those *peculiar* to P—are not similar in character to those current *in ordinary life* in the post-exilic period.” (2) “Some of the names peculiar to P do not appear to have been coined by the author, nor by any late writer, nor to have been current after the Exile.” (3) “Some of the names borne by persons mentioned only in P, but also by other persons mentioned elsewhere, are early in character, and a few are not known to have been current late. We differ on two points. I have asserted that (1) “the names in P are not as a whole pre-Davidic in character,” and (2) “that some of the names are late artificial creations.” Prof. Hommel directly denies the second, and indirectly traverses the first. I will consider (2) first. If (2) stands, (1) follows; but even if (2) can be shown to be unwarranted, (1) would still remain almost unaffected, for the main argument in its favour is independent of (2).

The names on which I directly based¹ my conclusion that some of P's names are late artificial creations are (a) six² compounds with either Tsûr or Shaddai; (b) compounds with a preposition or participle—Lael and Shelumiel; and (c) “perhaps certain others,” *e.g.* Pedahel and Nethanel. The question mainly turns on the compounds with Tsûr or Shaddai. Did the ancient Hebrews or did they not employ names of this type? According to P they not only employed them, but employed them with some frequency. Five³ out of a list of twenty-four names of

¹ As a *result* of my general conclusion, I selected seventeen compounds with *el*, which I considered to be probably of late creation (p. 210). The bearing of Prof. Hommel's discussion on these I consider below.

² Pedahtsûr, Elitsûr, Tsuriel, Tsur-Shaddai, Ammi-Shaddai, Shaddai-ur (E.V. Shedur; also z for ts in tsur).

³ Including עֲדַיִאִוִּר (Num. i. 5), which, in spite of the Massoretic pointing, is generally admitted to belong to this class.

tribal princes and their fathers given in Numbers i. (and repeated in ii., vii., x.), and one out of six names in a corresponding list of Levites (ch. iii.), are of this type, *i.e.*, exactly a fifth of the whole number of names in these lists are of a type of which no single trace is found elsewhere in Hebrew literature. This is a striking and significant fact which calls for serious explanation. To say, as Dillmann said, that these names are ancient because they never occur "later" is simply to beg the question. The proper neutral statement is that they never occur *elsewhere*. Thus, *e.g.*, they are absent from the prophetic narrative (JE) of the Mosaic period. Consequently, unless we *assume* that everything in P is ancient, a decisive judgment as to the antiquity of these names must be based on other considerations. Briefly summarized, the considerations on which my own judgment was based were these: (1) Shaddai,¹ although unquestionably an ancient term for God, judged by its usage in Hebrew literature, was not in frequent general use at any time among the Hebrews, but occurs most as an archaism (thirty-one times in Job). (2) Tsûr used absolutely² of God (as in the name Pedahtsûr) occurs in Hebrew literature only in Deuteronomy xxxii. 16 (cf. xxxii. 37) and Habakkuk i. 12, *i.e.*, on generally accepted critical grounds, not earlier than the seventh century. (3) The root פדה found in Pedahtsûr is in names found

¹ In speaking of the name Ammi-Shaddai in connection with the Mosaic age, Hommel says (p. 110): "It contains the subsequently obsolete Divine name Shaddai." This is misleading: as a matter of fact Shaddai was unquestionably in use as late as the Exile (Ezek. i. 29; x. 5; Isa. xiii. 6).

² On further consideration I question whether Tsur is ever used *absolutely* of God in O. T. In Deut. xxxii. 18 and 37, where it occurs without the article, it is virtually defined by the following relative clause. In vv. 15, 30, 31 of the same chapter the definition is still more manifest. In none of these cases is the usage really like that in פדהצור, which is paralleled, if at all in the O. T., only in Hab. i. 12—a passage in which the text, especially as regards the word צור, is uncertain; cf. the *Beilagen* to Kautzsch's Bible. In Deut. xxxii. 4, הצור may perhaps be considered to be *on the way* to become personal; cf. הצור in Job.

only in those which are unquestionably late, or attested only by documents of late or uncertain date. (4) The *prefixing* of the perfect tense (as in *Pedahtsûr*) is one of the commonest formations of the later periods, but almost if not entirely unknown to the earliest;¹ thus in the earliest pre-Davidic period among compounds with *yah* or *el* (attested by writings other than P or Chronicles), the perfect is post-fixed in five names, but it is prefixed in *none*; on the other hand, in names which can be first traced in post-exilic times, the perfect is post-fixed in *no* case, but it is prefixed in twenty-six.

Of these various considerations Prof. Hommel has taken no account. He has, however, brought forward other considerations worthy of attention. He claims to have found parallels (hitherto uncited) for the use of both *Tsûr* and *Shaddai* in compound names. The case with regard to *Tsûr* is clear. It occurs in *Tsûri-'addana* (cf. Hebr. *Yeho-'addan*), a name found on a South Arabian inscription not later than the 8th century B.C. Prof. Hommel also (and probably enough correctly) detects *Tsûr* as a Divine term in *Bir-* (or *Bar-*) *Tsûr*, a North Syrian name of the 8th century (*Zinjerli* inscription). By an *inference* he then refers the usage of *Tsûr* as a Divine name in Midian to a much earlier period, and considers this "of decisive importance in determining the antiquity of Hebrew names compounded with *Tsûr*" (p. 321). Prof. Hommel rather confuses inferences and facts. It is important to distinguish them. The fact is that one certainly, and perhaps two² or three other Semitic compounds with *Tsûr* can be traced to the

¹ The comparative data employed by Hommel support, as I shall show further on, the validity of this conclusion.

² I have drawn attention (*Hebr. Prop. Names*, p. 195 n. 1) to the possibility of *Tsûr* in *Beth Tsûr* being a Divine name. We cannot be *sure* of the antiquity of this name, since it is only mentioned by P (*Jos. xv. 58*) and unquestionably late writings—*Neh.*, *Chron.*, and *Macc.*; but, being a place name, the probability that it is fairly ancient may be admitted.

8th century B.C. The inferences are: (1) that such compounds were also in use five or six centuries earlier; and (2) that they were used by the Hebrews as well as by North Syrians and South Arabians.

As regards Shaddai, Prof. Hommel adduces no new names, but offers a fresh explanation of a now familiar name, which, if accepted, furnishes us with a very close Arabic parallel to Ammi-Shaddai. The name in question is Ammi-satana, borne by a Babylonian king of the Khammuri Dynasty (which was of Arabic origin), who lived not later than about 1820 B.C.; and is interpreted "My uncle is our mountain," *sata* being treated as = Ar. *saddu*, and the final *-na* as the Arabic form of 1st pers. plur. pronominal suffix. But *saddu* also = Hebr. Shadd(ai); and thus the only difference between Ammi-satana and Ammi-Shaddai is that in the one the suffix is plural and in the other singular, just as at a later date (8th century) we have the Assyrian names Marduk-shadûa = Marduk is my mountain, and Sin-Shadûni = Sin is our Mountain. But all this is mere possibility—a theory based on one unproved conjecture after another. I draw attention briefly to these points: (1) The transliteration of the second element in Ammi-satana is uncertain. Maspero¹ adopts Ammi-ditana. (2) Granted *satana* be correct, Prof. Hommel's interpretation of it has not yet been accepted by Assyriologists. The possibility of its being 3rd perf. of a verb remains. (3) In view of the root meaning "to stop up," and the fact that derivative nouns signify a barrier, or dam, the Arabic *saddu* appears to designate a mountain as an *obstruction*, not as a lofty height.² (4) Shaddai as a Divine name did not convey to the Hebrews the meaning "my mountain," since in

¹ *Histoire Ancienne*, Tome II. p. 45 n. 2.

² Others who, like Fried. Delitzsch (*Hebrew Language in the Light of Assyrian Research*, p. 48), have previously given to Shaddai the meaning mountain (not *my* mountain) have based the meaning on the Babylonian *sâdû* = lofty height, mountain.

that case Yahweh would be made to say, "I am El, my mountain!" (Gen. xvii. 1). In other words, in the narrative of P Shaddai standing by itself cannot have had the sense which Prof. Hommel would attribute to it in the names Ammi-Shaddai, etc.

Assuming however for the moment the correctness of the proposed view of Ammi-satana, what ought to be our conclusion with regard to the six names compounded with Tsûr or Shaddai? Ammi-Shaddai would probably enough be an ancient Semitic name; the other two compounds with Shaddai (Shaddai-ur and Tsûri-Shaddai) might reasonably on analogy be judged to be genuine Semitic personal names. Similarly, in view of Tsûri-addana, the compounds with Tsûr may be accepted as actual personal names. But several of the considerations derived from a study of the history of Hebrew names remain unaffected. The comparative frequency of the names in P's lists still stands in striking contrast to their entire absence from all other Hebrew sources and their extreme rarity in other Semitic sources. The use of פִּרְיָה and the prefixing of the perfect in one of the compounds remain as before, suggestive of late date. It still seems to me, therefore, that the hypothesis that P's lists are late artificial compilations from names of various sources and periods alone accounts, even in the case of this particular group of names, for all the facts—those derived from the Hebrew as well as from the inscriptional sources.

Lael, so far as I have observed, is not discussed by Prof. Hommel. Shelumiel he does not regard as compounded with a participle. I will not therefore discuss these names afresh. Interesting as they are in themselves, they are too isolated and uncertain to form by themselves any strong argument for artificiality or lateness of formation.

Turning now to the general complexion of P's names, I have first to repeat that Prof. Hommel never once consi-

ders this important aspect of the question. Briefly to summarize my argument on this point: Of twenty-nine compounds with *el* peculiar to P, *el* forms the last element in twenty-five, the first in only four. But in pre-Davidic names attested by Hebrew literature (exclusive of P and Chr.), the Divine name (*el* or *yah*) stands as frequently first as last;¹ an increasing tendency can be traced through the history to post-fix the Divine name till in post-exilic (as in P's) names the post-fixing occurs many times more frequently than the pre-fixing. Again, P contains two or three names in which *el* is post-fixed to a perfect—a formation which was, as I have shown, frequent in post-exilic, but unknown in pre-Davidic names. Then when we turn to some of the lists in P we find—(1) The number of compound names much larger than in other early lists, but in approximately the same proportions as in later lists; and (2) a striking and unusual proportion of compounds with a Divine name (especially *el*). Further, in several respects,² there is a close resemblance between P's compounds with *el* and those found in the list of angelic names in Enoch c. vi.

Here then is a whole series of similarities between the general complexion of the names in P and the general complexion of post-exilic names; the one striking dissimilarity is the absence of compounds with *yah*—for which, if the lists be artificially selected, there would be abundant reason (cf. Exod. vi. 3), and to which we have a parallel in the list of angels' names in Enoch.

As Prof. Hommel has left all this unnoticed, there are no

¹ This is stating the case most favourably. In further detail, cf. *Hebr. Prop. Names*, pp. 159, 166 (Table 1), with special reference to the footnote on p. 166.

² I may add as a *possible* additional resemblance in a matter of detail to those more general resemblances mentioned in my book the name $\tau\upsilon\rho\mu\epsilon\lambda$. Is the first part of this word the Aramaic צור =mountain? If so, we have a curious parallel to צור־אל —one of the names peculiar to P.

criticisms of his to reply to. I will, therefore, only point out how the comparative evidence to which he attributes primary and almost exclusive value supports my view as against his own. He has not furnished us with exact statistics of the different formations of South Arabian names, nor have I yet—as I hope, unless anticipated, to do at some future time—been able to compile them for myself. But the large number of South Arabian names cited by Prof. Hommel, into which either *ilu* (=el), or an equivalent expression (*e.g.* *abi*) enters, may presumably for present purposes be taken as sufficiently typical; *i.e.*, there is no reason to suppose that Prof. Hommel has cited a larger proportionate number in which the divine element is the first than in which it is the second element, especially since he was aware that suspicion rests on certain Hebrew names on account of the post-fixing of the divine name. On pp. 83f. Prof. Hommel cites nineteen names in which *ilî* precedes a perfect, five in which it follows an imperfect; three (+ three possible, but, in Prof. Hommel's judgment, unlikely) instances in which it follows a perfect; *i.e.*, in all nineteen names in which *ilî* is the first, eleven in which it is the second element. Again, on pp. 85f. he cites forty-seven names in which *abi* or a similar element *precedes*, three in which it *follows* a perfect.¹ The same preference for prefixing the divine element, which we find in early South Arabian names and in Hebrew names referred to the earliest periods by writings other than P and Chronicles, prevailed in Babylonia. Speaking of early Babylonian names, and having just cited a number in which the divine element stands second, Prof. Hommel proceeds:—

¹ Cf. p. 81 f. "Now, it is interesting to observe that it is not till we come to neo-Sabæan inscriptions [200 B.C.—600 A.D.] that Shamsun, Aum, Athtar, and other names of deities . . . appear as the second element in personal names, and even then they do not occur nearly as often as *ilu* = God, which, moreover, appears frequently as a first element."

“Even more numerous are those names compounded of two elements in which the *first* element consists of the name of a deity. From an examination of instances it is clear that a much greater freedom was allowed in the choice of verbal forms which might be tacked on after the name of a god. While verbs and participles are of comparatively rare occurrence as first elements, they are quite common as second elements” (p. 74).

To sum up, then, the compounds with *el* peculiar to P regarded all together, present in respect of formation a striking dissimilarity to early Hebrew compounds with a divine name (as attested by Hebrew literature, exclusive of P and Chronicles), to early South Arabian, and to early Babylonian compounds with a divine name and an equally striking similarity to late Jewish, and, apparently, to late South Arabian compounds.

Of the seventeen compounds¹ with *el* peculiar to P, which I considered likely to be individually and separately of late origin in Hebrew, I have already discussed two—Tsurial and Elitsûr—in the light of what Prof. Hommel has to say. The reason for regarding them as *artificial* is lessened, and so far the reason for regarding them as late. So far as the form goes, they might be of any period. Of the remaining fifteen, Prof. Hommel has little to say. Gaddiel, if correctly interpreted by him, “My grandfather is god” (cf. Arab. *ġadd* = grandfather) would almost certainly be early. Unfortunately, we need to be sure of the period in which it originated in order to estimate the relative probabilities of two equally possible interpretations of the first element—grandfather or fortune. If the name be early, Prof. Hommel is most probably right, and the name would form an interesting, though isolated, instance of another group of names compounded with a term of kinship.

Prof. Hommel holds that the Hebrews of the Mosaic period spoke Arabic, and that their names were Arabic.

¹ *Heb. Prop. Names*, p. 210 (where לוריאל is a misprint for לוריאל).

Does the list in Numbers i. on this hypothesis satisfy the conditions of antiquity? It certainly contains, as I had myself pointed out, names (Ochran, Enan) ending in *ân*, which is an Arabic termination. But in the same list we have Naḥshon and Helon, with the corresponding Hebrew termination *-ôn*. Prof. Hommel accounts for this by the suggestion that, owing to the intercourse between Canaan and Egypt, some Canaanite forms would find currency among the Hebrews (p. 300 n. 2, 301). But are not the very forms with Shaddai purely Hebraic (or Canaanite), and non-Arabic? The Arabic form would begin with the simple, not the aspirated sibilant. Again, a name like Nethanel is not Arabic, since that language does not use *nathan* (=to give); the corresponding Arabic name is Wahaba-ilu (cf. Hommel, p. 84). Many of the names are indecisive, but the decisively Hebræo-Canaanite considerably outnumber the decisively Arabic forms. Once again, then, judged from this standpoint, the names are mixed, not homogeneous, in character.

In conclusion, I will briefly refer to two points having a more general and indirect connection with the subject of this article. On p. 299, in footnote 2, Prof. Hommel draws attention to a distinction made by Wellhausen between the antiquity of tribal names compounded with *el* and personal names of the same kind. The distinction is, I believe, valuable and valid. It is an inference, no doubt, but, as it appears to me, a well-founded inference, that the formation of tribal names compounded with *el* preceded the formation of similar personal names. Still, I made no use of this point in my discussion of P's names. On the contrary, I distinctly stated (herein differing from Wellhausen as cited by Hommel) that compounds with *el* of certain types were used as personal names in all periods of the history of Israel, and that the restriction of these to tribal names existed, if at all, prior to the earliest period of

Hebrew history. What I have maintained, however, is that a *particular formation* with *el*—that, namely, in which an imperfect precedes—was for long confined to tribal names, and only from about the 8th century began to be employed by the *Hebrews* for personal names. The data, which will be found on pp. 215ff. of my book, still appear to me to give to the inference great probability. At the same time, Prof. Hommel would probably challenge it. For formations of this kind occur early as personal names in South Arabia (Hommel, pp. 83, 112), and, as I had stated, in Babylonian contract tablets of about 2000 B.C. The right conclusion from all the data appears to be that a formation in early use among certain Semites only came into use among the Hebrews and certain other Semites at a relatively modern date. It is precisely one of the cases where we have to recognise difference and not similarity among related races. Let me only add that the critical use I have made of this inference is small. I have used it occasionally as a test of the antiquity of the names in 1 Chronicles i.-ix.; but in most cases it could be dispensed with. Thus the evidence against the antiquity of 1 Chronicles iv. 34-41 remains overwhelming without it. But the very fact that the names so frequently occur in sections and among names which on other grounds must be adjudged modern, lends additional strength to my view of the Hebrew usage. With regard to the significance of names of this type, I note with pleasure that Prof. Hommel inclines to take the same view as myself, and to attribute to the imperfect an optative sense.

From the fact that in the early South Arabian names a preference is shown for the use of *ilu*, or some other general term, to the almost total exclusion of proper names of deities, Prof. Hommel infers that the religion was "a very advanced type of monotheism, not unworthy to rank with the religion of Abraham as presented in the Biblical

narrative" (p. 88); and similarly, from the sparing reference to deities by their proper names in Assyrian compounds of the Khammurabi period, he infers that "these names express no sentiment which is inconsistent with the highest and purest monotheism" (p. 76). Prof. Hommel is here really putting forward a modification of Renan's theory¹ of the original monotheism of the Semites.

Prof. Hommel is probably right in inferring, as I think he does, that compounds with a generic term (subsequently at least applied to a deity or deities) were earlier than compounds with the proper name of a deity, but the fundamental objection to the inference he draws from the use of these generic terms is that it is based on a too isolated study of names, and a failure to distinguish between the significance of an actually generic term and a generic term which has in course of time become virtually specific. The post-exilic Jewish name Mehetabel (God does good) no doubt reflects monotheistic belief; but we can assume this only because we know *on other grounds* that the Jews of the period believed that only one *el* existed. Such a name occurring at another period or among another people who believed in the existence of more than one God would possess an entirely different significance. Now both in South Arabia and in Babylon we can, even from the names and still more from the general tenour of the inscriptions, see that more than one God was believed to exist. Under these circumstances we must believe that the generic term retains its full and original sense, and has not become virtually specific; that such a name, for instance, as *ilu-wahaba* means "the member of the class *el* (or God) whom I worship has given," and not "the one supreme being who alone constitutes the class *el* (or God) has

¹ See e.g. *Hist. Comparatif des Langues Sémitiques*, pp. 5 f., and later, with special reference to Semitic names, *Revue des Études Juives*, v. 161 ff.

given." In any case, Prof. Hommel draws an entirely unreal and unjustifiable distinction between the Arabian and Canaanite religions as illustrated by the proper names (p. 225). *Ba'al* and *Ādōn* are just as much generic terms as *ilu* or *abi* or the like, and we have just as much reason for inferring a virtual monotheism from the prevalence of *Ba'al* and *Adon* in the one set of names as from the use of *ilu*, *abi*, etc., in the other.

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