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WHITHER BIBLICAL RESEARCH?*

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IT IS not always realized, or kept in mind, that biblical research, no less than any other branch of group activity, is subject to the social forces — the term “social,” of course, represents the longer phrase and concept: social, economic, political, cultural, religious, and the like — at work within the community at large. Thus the kinds of interpretation of the Bible — both as a whole and even of specific passages in it — that prevailed in the last couple of centuries B.C. would not have been possible in any environment but that of Hellenism as it was adopted and adapted in the Jewish communities of the Diaspora and Judea. The earliest specifically Christian exposition of what constituted the Bible differed markedly from that of the Jewish-Christian period and community that preceded it, basically because the social structure of the Roman Empire as a whole and the specific status of the Christian and the Jewish communities within it had changed significantly from those that had obtained in the first three centuries A.D., before Christianity had become in rapid succession a tolerated and then the official religion.

This principle of social forces, rather than the personal whim of a scholar here and there, being the decisive factor in the shaping of a discipline such as ours, applies of course to every epoch in history, be it the Middle Ages, the Renaissance, the Reformation, the demise of feudalism, or the birth of capitalism in Western Europe. But this point need not be belabored here, not because it has been dealt with adequately in various works on the subject — indeed, I do not think that it has been — but because it is chronologically not pertinent enough to the present discussion.¹

* The Presidential Address delivered at the annual meeting of the Society of Biblical Literature on October 26, 1970, at the New Yorker Hotel, New York, N.Y.

¹ The interpretation of the Bible in the light of changing historical circumstances has remained essentially virgin soil for the inquisitive and trained scholar. To describe Philo's or Jerome's or Rashi's or Astruc's or Wellhausen's or S. R. Driver's manner of interpreting the Bible — basic as it is — is only preliminary to the systematic attempt to *account for* their kind of biblical exegesis. It is not easy to improve upon the descriptive approach of Beryl Smalley in her fascinating treatment of *The Study of the Bible in the Middle Ages* (1941; rev. ed., 1952; reprinted in paperback, 1964

During the nineteenth century and the first quarter of our own, i.e., before the consequences of World War I took real effect, biblical research — I shall be using the terms “Bible” and “biblical” sometimes to cover both the Hebrew and the Christian Scriptures and sometimes the Hebrew alone — followed generally the pattern of research in the classical field, which was more solidly and extensively established at the time. Textual and literary criticism and comparative linguistics — in those days involving almost exclusively Hebrew, Aramaic, Arabic, Syriac, and Ethiopic, and what Babylonian-Assyrian was known — were the norm. The standard works were the grammars by König, Gesenius-Kautzsch-Cowley, and Bauer-Leander; the lexicons employed were usually those of Brown-Driver-Briggs and Gesenius-Buhl; and Brockelmann’s two-volume *Grundriss* was the sole claimant to respect in comparative Semitic linguistics.²

This state of affairs is easy to recall, because after all the hectic years since World War I it is still these same works that are standard today — except that Bergsträsser began a notable revision of Gesenius-Kautzsch-Cowley over half a century ago, but no one has followed up his effort after his untimely death in 1933. Koehler published a lexicon (1948–1953), which even Baumgartner’s considerably revised edition is hardly able to improve upon, so that it can seriously compete (in many respects) even with Brown-Driver-Briggs. (There is a good historical reason for this serious lack of progress, and I shall return to the problem below.)

[Univ. of Notre Dame]); what remains to be done is to account for the kind of biblical exegesis practised by the Gilbert Crispins and the Peter Abailards and the Hughs and the Andrews of St. Victor in the light of the historical developments in eleventh-twelfth century England. In more recent times, an inkling of the problem may be gained from a careful reading (sometimes between the lines) of the preface (pp. III–XXI) and addenda (XXV–XXXIX) of Driver’s *Introduction to the Literature of the Old Testament* (rev. ed., 1913), where the learned and careful author has to defend his philosophy of biblical interpretation. An historical analysis of the attitude of the Church of England and its supporters toward Driver’s kind of exegesis would constitute a major contribution to the history of the study of the Bible (e.g., why certain theories are regarded favorably by some groups and rejected in other circles, regardless of the cogency of the argumentation).

Formal — but really really perfunctory — surveys of this aspect of biblical research may be found in such Introductions as R. H. Pfeiffer, *Introduction to the Old Testament* (New York: Harper, 1941), pp. 40–49 (Ch. 3: Historical and Critical Interest in the Old Testament); or O. Eissfeldt, *The Old Testament: An Introduction* (New York: Harper and Row, 1965), pp. 1–7 (§ 1: “The Nature of the Undertaking”); cf. the articles on “Biblical Criticism” (by K. Grobel, *IDB*, 1 [1962] 407–13) and “Biblical Criticism, History of” (by S. J. De Vries, *IDB*, 1 [1962] 413–18) and their bibliographies.

² I have discussed some aspects of this in the chapter on “Old Testament Studies” (pp. 51–109) in the volume on *Religion* (ed. P. Ramsey; *Princeton Studies: Humanistic Scholarship in America*, Princeton, 1965).

Finally, the dominant philosophy of history then prevalent was Hegelianism or variations of it, so that the widely accepted reconstruction of biblical Israel's history and literary creativity was largely that of Wellhausen and S. R. Driver, as found in their standard introductions and commentaries, not to mention Wellhausen's *Prolegomena* and *Geschichte*, or Eduard Meyer's several works.

World War I, among other things, opened up western Asia, northeast Africa, and the eastern Mediterranean region generally to the world at large. The Ottoman Turkish Empire gave way to British and French domination, and also to uninhibited archeological and topographical investigation. This discipline gave new direction and emphasis in biblical research to the extent that it is no exaggeration to apply the term "revolutionary" to it. But revolution can be a bad as well as a good thing; and I believe that the negative and harmful consequences of archeology can and ought no longer to be denied or brushed aside.

But good things first. By the end of World War I biblical research had become stabilized, i.e., had gotten into a rut. Excellent as they were, and in many respects still are, the dictionaries, grammars, introductions, and commentaries mentioned above were not being significantly improved upon; no really new insights or breakthroughs were apparent. A major source of new data, the Sumero-Akkadian, had become available; but progress here was only gradual and accumulative. The Documentary Theory, as refined especially by Wellhausen on the Continent and by Driver in Great Britain, reigned supreme. The Pentateuch, as everyone knew, was composite; and the composers were J, E, D, and P. For lack of other approaches and new data, scholars delved even more intensively into these four sources, decomposing the composers into J₁ and J₂, E₁ and E₂, and the like. While sensitive to the frustrations confronting our colleagues of fifty and forty years ago, we regret that so much talent and energy were spent in helping to demonstrate the law of diminishing returns.

With all their secondary disagreements about the limits of J and E, or the character, if not the very existence, of J₂ and E₂,³ scholars generally agreed not only in the matter of the four primary documents, J, E, D, and P, but also in something that was much more important, viz., that none of the four documents was to be treated as reliable ma-

³ I have used the term "secondary (disagreements)" deliberately; already Driver (*Introduction*, Preface, pp. IV-VI and n. * on p. VI) had something trenchant to say about how "language is sometimes used implying that critics are in a state of internecine conflict with one another . . . [so that] the results of the critical study of the Old Testament are often seriously misrepresented. . . ." Many of us today have heard people glibly assert that archeology has "confirmed" the Bible and demolished the Documentary Theory!

terial on which to base a serious reconstruction of biblical Israel's early career. Hence not only could J, E, D, and P be separated as essentially distinct literary creations, and not only could they be dated in their preserved form with some confidence — J and E as the products of the tenth-ninth centuries, D of the seventh (pre-exilic) century, and P of the sixth-fifth (post-exilic) century — but, and this was or should have been regarded as the most important aspect of the Documentary Theory, they were considerably devoid of historical authenticity. Not one of the documents could the sober scholar use, except with the greatest reserve, for the reconstruction of the patriarchal period, or of the Mosaic, or of that of the Judges.

The great and lasting merit of archeology is that it has made it possible, and even necessary, to grant these documents considerable trustworthiness; this constituted a *revolutionary breakthrough*. Pertinent parallels and other data were brought to light so that the Dark Ages of Canaan-Israel in the second millennium (not to mention the blackout of the region during the fourth and third millennia and the prehistory before that) became the relatively well-known Middle and Late Bronze Ages. In this connection, I need only mention in passing such important excavations as that of Albright at Tell Beit Mirsim in the Twenties and such discoveries as those at Nuzu and Ugarit in the Twenties and Thirties. There is hardly an aspect of biblical research that has not benefited directly or indirectly, sometimes to a remarkable degree, from archeology, be it linguistics, lexicography, poetic structure, textual criticism, theology, history, chronology, social and legal institutions, comparative literature, mythology, and so on.

Something too should be said about the fact that the material culture of ancient Israel is now known in vastly greater detail than before. I have in mind not only the walls and houses and household articles (especially pottery) and articles in trade, and the like, but also trade and industry and the crafts in the large. And then there is archeology as a discipline in its own right, regardless of whether it sheds any light on the Bible — and far more often than not it does not. Naturally, archeology in and about the Holy Land is important to biblical scholars “not so much . . . as a branch of science per se but as a handmaid, a tool for the better understanding of the Bible and the Holy Land. Unlike the Sumerologist, Akkadiologist, Hittitologist, Egyptologist, and the like, who have been laying bare the history of their area from the beginning of time to the end of the *floruit* of the civilizations that interest them, the biblical scholar has been interested in archeology mainly for its help in elucidating the Bible.”⁴

⁴Orlinsky, “Old Testament Studies,” p. 66. In this connection, H. J. Cadbury's presidential address to this Society in 1936 is most germane, “Motives of Biblical Scholarship,” *JBL*, 56 (1937), 1-16.

This preoccupation with the biblical aspects of archeology has led to a rather unbalanced view of what archeology has meant for the Bible. Let us recall for a moment the historical background, which many, if not most of us present this evening, lived through, but sometimes tend to overlook in this connection. Ever since World War I, the depression of the early Thirties, the growth of various forms of totalitarianism in Europe and Asia, the horrors of World War II, the cold, hot, lukewarm, and warmed-over wars, domestic and international, of the past two decades, recessions and the fear of them, increasing automation and alienation, and the specter of unemployment — all this and more have convinced many that reason and science, the two major ingredients in the making of the Ages of Reason, Enlightenment, Ideology, Analysis, Science (in short, the Ages of Optimism) — were not able to bring our problems, international, national, group, or individual, significantly closer to solution. And so people began to come back to and seek out once again what had long been regarded as the Word of God, the Bible.

This Word, however, was no longer an isolated phenomenon in the midst of history; no longer was it a static event, independent of time and place. For archeology had changed all that.

So it was that the historical circumstances that had brought archeology into being, and had also brought the Bible once again to the fore of man's attention, led to an extraordinary increase of popular interest in the Bible in the light of archeology. Increasingly during the Forties and Fifties, and there is no sign of any appreciable let-up, people began to seek out the "truths" of the Bible as "proved" by archeology. What had been a bit of a rivulet immediately after the tomb of the late King "Tut" was cleared in 1922 became a veritable torrent of picture books on archeology, on the Bible, and on the Holy Land, a number of them good, some excellent, and many simply commercial potboilers — this apart from the daily press and literary magazines and lecture forums as a popular source of information (and misinformation and half-truths and melodramatic accounts) — of how archeology has "proved" the Bible right; as the title of a best-seller of the middle Fifties had it, *Und die Bibel hat doch Recht*, on which D. R. Ap-Thomas commented with refined British understatement (*British Book List*, 1957, p. 18), "... It will certainly have a large sale, although (perhaps in part because) the scholar would wish for a little more caution at some points . . ."⁵

The emphasis on archeology and the needs of the time made it all too easy for undisciplined journalists and popularizers not only to exaggerate beyond reasonableness the scope of substantiation but to take a

⁵ Cadbury's *caveat* (p. 11), "... As experts we have some responsibility to help curb the morbid tastes of so many superficial lay book readers who prefer to hear from us some new guess than some old fact," certainly applies here.

giant, and utterly unjustified, step beyond that and assert that this substantiation demonstrated the Bible as the revealed word of God! Nothing could be more of a non-sequitur in disciplined reasoning than the juxtaposition of these two completely independent phenomena. This widespread confusion between the Bible as a religious document and the Bible as a historical document is a serious matter, and I shall touch on it below.

The rise of biblical archeology since World War I not only coincided with but has in part been responsible — to be sure, unwittingly — for the decline in biblical philology and textual criticism. In the general educational pattern of the United States and Canada, the humanities began to give way to the pure, the applied, and the social sciences. The number of students studying Greek and Latin in high school and college decreased considerably in the past two or three decades, and these subjects are generally not required for ordination even in theological seminaries; so most students, by the time they have acquired the B.A. or B.D. degree and decide to specialize in Bible, must *begin* the study of Greek and Latin, of Hebrew, Aramaic, Canaanite, Syriac, Arabic, Akkadian, or Egyptian. And since it is much easier to do original work in connection with such expanding disciplines as archeology and Akkadian and Northwest Semitic-Canaanite, it is these areas — especially in the form of parallels between them and between passages and phrases in the Bible — that have been attracting the research efforts of so many younger scholars who otherwise would have tended toward biblical philology. As a result, in 1947, E. C. Colwell, in his presidential address to this Society, was able to begin right off with the assertion, “Biblical criticism today is not the most robust of academic disciplines . . . [it] is relatively sterile today . . .”⁶

This widespread inadequacy in the most basic of disciplines in any field of scholarly research, that of being able to handle a text, showed up especially in the study of the biblical portions of the Dead Sea Scrolls. It is no exaggeration, as it is no pleasure, to assert that all too many of the textual studies of these biblical documents hardly rated a passing grade. The Wellhausens and the S. R. Drivers, the George Foote Moores and the Max Margolis's, and the James Alan Montgomerys would have known how to deal with biblical texts and quotations, whether copied from a *Vorlage* or written down from memory or from dictation. Instead, that gold mine of misinformation and half-truths and of errors of omission and commission, and the like, viz., the so-called critical apparatus in Kittel's *Biblia Hebraica*³, constituted the pay dirt for so many who used it when referring to or when basing arguments on the Septuagint or Targum or Syriac or Vulgate, etc., but who

⁶ “Biblical Criticism: Lower and Higher,” *JBL*, 67 (1948), 1–12.

never saw these primary versions directly, or never realized the inner problems that not infrequently beset the primary versions. It will suffice here to reproduce the following statement from the survey article by Peter Katz(-Walters) in 1956, "Septuagintal Studies in the Mid-Century,"⁷ ". . . Contrary to Lagarde's intentions they [Duhm and his school] confined their interest in the LXX to those passages which seemed hopeless in the Hebrew. One may say with truth: Never was the LXX more used and less studied! Unfortunately much of this misuse survives in BH³. I have long given up collecting instances. Ziegler, after ten pages of corrections from the Minor Prophets alone, rightly states that all the references to G must be rechecked. H. M. Orlinsky who comes back to this point time and again is not very far from the truth when he says that not a single line in the apparatus of BH³ is free from mistakes regarding G " (p. 198).⁸

So far as the biblical texts among the Dead Sea Scrolls are concerned, it must be said that whatever be the consensus of scholarly opinion about their value for the textual criticism of the Bible, that consensus would have very little *a priori* standing in a court of law in which competent textual critics were the judge and jury. The consensus, whatever it be, would have to undergo the most detailed and searching methodological cross examination before it could hope to be cleared by the court. The fact that the biblical scrolls have come to enjoy a fairly widespread popularity among members of our scholarly guild makes that no more authoritative and useful than the fact that for decades the critical apparatus in BH³ also enjoyed that very status; the latter is a woefully weak link in the chain of the former.⁹

Another aspect of biblical research that the fruits of archeology have unfortunately helped to bring to the fore is the current vogue to equate

⁷ Subtitled "Their Links with the Past and Their Present Tendencies," *The Background of the New Testament and its Eschatology In Honour of Charles Harold Dodd* (eds. W. D. Davies and D. Daube; Cambridge: University Press), pp. 176–208.

⁸ The reference is to Part I ("Kritische Bemerkungen zur Verwendung der Septuaginta im Zwölfprophetenbuch der Biblia Hebraica von Kittel," pp. 107–120) of J. Ziegler, "Studien zur Verwertung der Septuaginta im Zwölfprophetenbuch," *ZAW*, 60 (1944), 107–131. There the concluding sentence reads, "Bei einer Neuauflage der Biblia Hebraica des Dodekapropheten muss das gesamte G —Material, wie es die eben erschienene Göttinger Septuaginta-Ausgabe vorlegt, neu bearbeitet werden" (p. 120).

For my own strictures against Kittel's *apparatus criticus*, see §§ I-II (pp. 140–152) of "The Textual Criticism of the Old Testament," *The Bible and the Ancient Near East* (Fest. W. F. Albright; ed. G. E. Wright; Garden City: Doubleday, 1965; paperback reprint, 1961, pp. 113–121), with considerable bibliography. Note especially the reference to the vain attempt of E. Würthwein to suppress the sharp criticism of Kittel's BH³.

⁹ See §§ II–III (pp. 145–157) of "The Textual Criticism of the Old Testament" (cited in n. 8).

“parallelism” with “proof,” to substitute the citation of parallels for reasoned argument. I suppose that it is inevitable in the nature of things for anyone, as well as anything, to seek the level of least resistance. When the cuneiform texts of the second and first millennia B.C. were uncovered earlier in the twentieth century, what was more natural than for scholars to jump on the Hittite and other bandwagons and find parallels in the most unlikely as well as likely places? One may readily recall the Pan-Babylonian-Hittite school, and the obsession of Hugo Winckler; or the tracing back of almost every detail in the biblical version of creation to the so-called Babylonian Genesis, Enuma Elish; or the connecting of nearly every clause in the pentateuchal laws associated with Moses to the laws of Hammurabi. It is true that, by and large, we have subsequently learned differently. We dismiss good-naturedly Winckler’s Pan-Babylonianism; and probably most scholars would now agree, e.g., with T. J. Meek’s statement of twenty years ago (*Hebrew Origins*², pp. 68–69), “There is no doubt but that there is great similarity between the Hebrew and Babylonian codes . . . , but the connection is not such as to indicate direct borrowing. No one today argues that. Whatever borrowing there was came indirectly, either through common inheritance or through Canaanite influence, or much more likely through both ways.”

I think, however, that we must go into the matter more deeply than that, for the problem constitutes the very heart of the question posed in our title: Whither Biblical Research? Bluntly put, it is a fact, one that is generally not recognized, that virtually none of those who are engaged in serious work in our field has been trained to do research in history, that is, to seek to account for the important changes, or for the serious, even unsuccessful attempts at changes, or for the failure to attempt any serious changes, in the structure of a given society. And without being able to comprehend historical forces at work, it is simply impossible to understand how a social structure functions, why it comes into being, why it is maintained, why it is changed, sometimes radically.

Let us assume that some time in the future, out of the ground and rubble of civilization, several documents, none of them intact or complete, are excavated: they are what we today recognize as the Constitution of the United States, the Charter of the League of Nations, the Yalta Agreement, and the Charter of the United Nations. And let us assume further that very little is known in any detail about the events that brought these notable documents into being, or of the social forces that brought on those events; more specifically, we know the background of these documents no better than we know, say, the two centuries preceding 586 (or is it 587?) B.C. or the two centuries following the momentous event of that date.

The scholars of that future date begin to study the numerous fragments of those four documents, trying to fit the many pieces together. They devote years to the study of the terribly fragmented texts and contexts. They recognize word formations, phrases, meanings, and the like, which have association — whether directly or indirectly, they are not always sure — with what they know of Latin and Greek, and with the languages and dialects of countries that once in the long ago had been France, Germany, Italy, the United States, Canada, England, Russia, and other such countries. The scholars have considerable difficulty in determining the precise nuance of numerous expressions; and some even suggest that it would be worthwhile compiling special glossaries of legal terms, economic terms, and political terms. Of course, a number of scholars will be busy working on the Form Criticism of these fragmented documents, for their *Sitz im Leben*. Special groups will be formed for this study, and foundations will be approached to help finance these studies. Monographs will be published on the grammatical forms employed in these documents, whether it is, say, the third or the second person that is employed, and on whether the clauses are apodictically or casuistically formulated (“you shall” as against “if one does . . . , then”) — for then the documents may be traced back to a British, or Russian, or American, or French, or other prototype, or perhaps to a common ancestor for all four documents. In that case, it might become possible to date these four documents relatively (i.e., typologically), if not absolutely.

Naturally scholars will disagree in the matter of the relative, as well as of the absolute, dates of the documents. Old words and phrases will be found in all four documents, and so some scholars will jump to the conclusion that the older the phrase the older must be the fragment in which it was preserved. It will also become apparent that those scholars who ultimately derive from, or have an affinity to, the region or people or culture of what had once been, say, Great Britain, will tend to trace the origin and essential nature, and even the extraordinary worthwhileness of the documents — or of the Ur-Document — to that sphere, as against those who will hold out for the North American, or Central European, or Russian spheres, depending on the sphere to which they traced back their own cultural or physical ancestry.

It is obvious that one could go on in this vein, for there are many more areas and sub-areas of study in higher and lower criticism, in linguistics, in literary structure and analysis, and the like, that could be listed. But I have had something more in mind than a purely hypothetical situation in the future. What I have been leading up to is the fact that there is hardly a member of our Society who would be content with the kind of studies that I have indicated — no matter how scientifically they were done on these documents; and they would, of

course, be right. After all, is the significance of these documents to be found in their linguistic history and character? Or in their literary structure? Their primary importance, when all is said and done, their major *raison d'être* for scholars, as well as for laymen, lies in their historical value, in the use to which they are put for the explanation of not only what happened but *why* it happened. *Why* were these documents drawn up in the first place? Who had them drawn up, not merely the names of the countries but the powerful groups within each country? What motivated each of the signatories? Why did certain major powers decline to become signatories — for surely the reason that the United States did not become a signatory of the League of Nations will not be determined through literary, or linguistic, or archeological, or theological analysis.

It is for historical matters that these documents have significance, for it is about these matters that the welfare — sometimes the very existence — of the government and people of the signatory countries, and even of a number of non-signatory countries, revolves. One can just imagine how the scholarly and lay world, where it did not simply ignore, would hoot derisively at the virtually exclusively philological, literary, linguistic, archeological, theological, and similar studies of these documents; the silence that would greet these studies would truly be golden compared to the scorn with which they would be laughed at. And the world would be right: Is that all that these documents are useful for? Is that their true significance? Yet this is precisely what we members of our biblical guild have been doing since archeology began to provide us with a breakthrough in our field half-a-century ago. Literary patterns and — what is much worse, lexical and literary *parallels* — are what have been occupying the energies of so many of us and have been filling so many of the pages of our learned journals and books.

In the past decade especially, hardly an issue of our journal and of others in the field has appeared without an article or two and a book review or two, or more, that does not deal in part or in whole with a parallel, or an alleged parallel, between a biblical phrase or section on the one hand and an extrabiblical correspondent on the other. A decade ago the search for parallels in “the areas of rabbinic literature and the gospels, Philo and Paul, and [by then, especially] the Dead Sea Scrolls and the NT” had reached such proportions that the presidential address to this Society in 1962 dealt with “Parallelomania.” That was actually the title of the address; and the plea was made that “biblical scholarship should recognize parallelomania for the disease that it is . . . and which the scrolls have made an imminent and omnipresent one.”¹⁰

¹⁰ S. Sandmel, “Parallelomania,” *JBL*, 81 (1962), 1–13.

I have alluded already to the handling of the biblical texts among the Dead Sea Scrolls both *per se* and in relation to the received Hebrew text and the Septuagint; this is a chapter in itself, not a very happy one. But I do wish to make specific reference to the current vogue, viz., the limitless and uncritical search for extrabiblical parallels to the concept and institution of covenant in the Bible. There is hardly a treaty or contract in any part of the Near East of the second or first millennium B.C. that has not been cited as a prototype of the biblical notion of covenant. Yet I am not aware of a single study of the concept and institution of the covenant in the Bible that a historian *qua* historian could accept methodologically. True, there is the basic factor, beyond the historian's immediate control, of being unable to date most of the biblical material. Imagine working on the Constitution of the United States, the Charters of the League of Nations and the United Nations, and the Yalta Agreement, and trying to reconstruct from them the history of their signatories without being able to date these documents relatively or absolutely. Yet that is exactly what we have been doing and tolerating, even accepting, in our field. All kinds of Sumerian, Assyrian, Babylonian, Hittite, and Northwest Semitic texts of all historical climes and periods are cited indiscriminately to prove that Israel and God had agreed to a vassal treaty. I am not really being facetious when I wonder out loud where the various historians, prophets, psalmists, and chroniclers — not to mention the glossators and redactors — who composed the Bible found the time to compose what they did when they were so busy reading and keeping up with and making use of the suzerain-vassal treaties that the Hittites and Babylonians and Assyrians and Northwest Semites were signing and, so often, breaking. In point of fact, I am not sure that any scholar has ever proved — worse, I am not sure that any scholar has recently even thought of trying to prove — that the contractual relationship between Israel and God as presented in the Bible is actually one that involves an inferior and a superior in the manner of a vassal and a suzerain. My own impression is that the biblical concept of the contractual relationship between Israel and God, a relationship into which both parties entered freely and in which both are legally equals, derives ultimately — since God by the very concept of Him to begin with is the Lord, and Israel the servant — from the lord-servant (*ʿādōn-ʿebed*) relationship that characterized Israel's (and much of Western Asia's) economy at the time. And while biblical expressions may be clarified with the aid of extrabiblical texts, I do not see how this can prove that Israel's covenant with God derived from vassal-suzerain treaties. As a matter of fact, it may well be that the more numerous the "covenant" parallels between Israel and her Asiatic neighbors during the second and first millennia B.C. become, the greater becomes the probability that the biblical con-

cept of the Israel-God covenant developed quite independently. So that, with Gertrude Stein, a parallel is a parallel . . . The pity of it is that in pursuing and collecting parallels, scholars think that they are writing history.¹¹

The net result is this: when the overwhelming majority of us are not trained textually and are unable for the most part to handle a text properly, and when even fewer of us have been trained to get at the underlying forces that shape the structure of society, to comprehend the social process, that can mean only one thing — that our work is rarely taken seriously by historians in the classical, or medieval, or modern periods of research. The most frequently used history of biblical Israel, virtually our standard textbook, is described by its author as having “been prepared with the particular needs of the undergraduate theological student in mind”; and the author of a standard textbook in biblical archeology states frankly in his preface that “only readers concerned with the religious value of the Bible will find anything of interest in these pages. The volume has been written with a frankly and definitely religious interest. It has also, of course, been written from a particular point of view, that of a liberal Protestant Christian.” Whatever else it may be that we are writing, it is not *history*.

Let us understand each other correctly. I am not opposed to Form Criticism, or to linguistic study, or to excavations, or to the seeking out of similarities — as well as points of difference — between Israel and her neighbors. Quite the contrary! I am all in favor of it, and more. But these disciplines, while each of them must be studied *per se* and not treated as but a handmaid to something else, cannot be regarded as ends in themselves for the real comprehension of ancient Israel. A historical analysis of, say, the concept of covenant in biblical Israel's career will go quite beyond the citation and compilation of parallels between biblical and extrabiblical phrases; it will, instead, ask — and attempt to answer — such questions as, Why did the concept of covenant mean one thing to Jeremiah and something else to his opponents in the matter of pacts with Babylonia and Egypt? What did “covenant” really mean when Uriah “prophesied in the name of the Lord” against the policy of King Jehoiakim, had to flee for his life to Egypt, was brought back, executed, and denied proper burial (Jer 26:20–23)? Why did “covenant” mean one thing to King Josiah in his attempted “reformation,” and the opposite to those who championed the cause of the legitimate and non-idolatrous shrines all over the country (for we fall into a trap when we follow tradition mechanically and brand the *bāmôt*

¹¹ The “covenant” parallels may turn out to be very little different from the “Hammurabi” parallels, viz., essentially parallels.

as idolatrous "high places")? Was it a question of conflicting economic and political interests couched in religious terminology — a phenomenon common to historians, especially to those who study the Middle Ages. Only when all the data achieved by Form Criticism, archeology, textual criticism, the determination of parallels, and the like, are brought into proper focus and play by the trained historian do they acquire life, worthwhileness, meaning.

In fine, as a consequence of a resurgence of textual criticism and philology in the broadest sense and by the introduction of the methodology and outlook of the trained historian, we shall not have to worry about a question like "Whither Biblical Research?" and preclude the withering of meaningful biblical research.

The full title of the presidential address had been "Whither Biblical Research: The Problem of 'Sin' as a Case in Point." Since time did not permit, the latter part of the title was not discussed on the podium of the Society's banquet. Here I shall but touch on the problem of the concept "sin" in the Bible, as I see it.

Discussions of "sin" in the Bible are almost as numerous as occurrences of sin; see, e.g., the recent study by R. Knierim, *Die Hauptbegriffe für Sünde im Alten Testament* (1965; 280 pp., with bibliography). It seems to me that without significant exception, the opening paragraphs of the article on "Sin" in *The Interpreter's Dictionary of the Bible* (IV, 361a-376a) represent very well the manner in which our guild of scholars understands the concept. They read:

The Bible takes sin in dead seriousness. Unlike many modern religionists, who seek to find excuses for sin and to explain away its seriousness, most of the writers of the Bible had a keen awareness of its heinousness, culpability, and tragedy. They looked upon it as no less than a condition of dreadful estrangement from God, the sole source of well-being. They knew that apart from God, man is a lost sinner, unable to save himself or find true happiness.

It is not difficult to find biblical passages referring to sin; as a matter of fact, there are few chapters which do not contain some references to what sin is or does. It might even be said that in the Bible man has only two theological concerns involving himself: his sin and his salvation. Man finds himself in sin and suffers its painful effects; God graciously offers salvation from it. This is, in essence, what the whole Bible is about.

It is my contention that this is precisely what the Bible is not about, and that the only way that one can begin to understand what sin, as well as the Bible as a whole, is all about is to try to comprehend it — naturally to the extent that our sources permit — in the light of the specific historical circumstances that prevailed at any given time. For instance, if one reads the book of Ezekiel, one gathers that the government and the people of Judah were on the greatest sinning binge in the history of Judah and Israel, if not in all of history. If only ten just

men had been found living in Sodom and Gomorrah, those legendary centers of sin and all their sinful inhabitants would have been spared. But so great was the sin of Jerusalem and Judah that, even if those very models of justice, Noah, Job, and Daniel, were living there, they alone would have been spared; but all the other inhabitants would have been destroyed, along with the Temple, the great city itself, and the country as a whole.

Ezekiel, as is well known, has provided us with a most detailed description of sinful acts, some of them so perverse and striking that more than one person has been led to believe that much of the detail was due to "Ezekiel's Abnormal Personality."¹² But whether the acts of sin did or did not take place, no serious historian would permit himself to be drawn into a debate as to whether the sin of Jerusalem and Judah was greater than that of Sodom and Gomorrah, or whether such sin as Ezekiel described, regardless of its alleged quantity and quality, was responsible for King Nebuchadnezzar's decision to wage a military campaign against — *inter alia* — Judah. Rather, the modern historian would "seek — behind the religious terminology — the same kind of documented human story, with an examination of its underlying dynamics, that would be his proper objective in any other field. Otherwise he would achieve no more than a compilation of myths, chronicles, annals, oracles, autobiographies, court histories, personal apologia."¹³ In dealing with the book of Ezekiel, the historian now has good reason to regard the Book as a whole as essentially reliable — unlike the situation in the late Twenties and early Thirties — thanks to the excavation of such sites as Tell Beit Mirsim, Beth Shemesh, and Lachish, and the publication in 1939 of the long-excavated and lost Babylonian texts of Nebuchadnezzar and Evil-merodach. But in his analysis of the momentous events that befell Judah at the turn of the sixth century B.C., the historian will go seeking behind such terms as "sin" and "covenant" for the fundamental economic, political, and social forces that determined the use and content — and, so frequently, the utter disregard — of these terms.¹⁴ There is a great future for biblical research and the trained historian who devotes himself to it.

¹² E. J. Broome, Jr., *JBL*, 65 (1940), 277–92.

¹³ From the writer's *Ancient Israel* (Ithaca: Cornell, 1954), p. 9 (p. 7 in the 2nd paperback edition, 1960).

¹⁴ The reader will do well to study carefully the methodology employed by M. A. Cohen in his discussion of "The Role of the Shilonite Priesthood in the United Monarchy of Ancient Israel" (*HUCA*, 36 [1965], 59–98) and in his analysis of "The Rebellions during the Reign of David: An Inquiry into the Social Dynamics of Ancient Israel," in the forthcoming volume of *Studies in Jewish Bibliography, History, and Literature in Honor of I. Edward Kiev* (ed. C. Berlin, New York: KTAV, 1971).