

Shifting Sands

*The Rise and Fall of
Biblical Archaeology*

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Preface

The current generation has witnessed great changes in the archaeology of Palestine. Before the 1970s, biblical archaeology was the dominant research paradigm. Today, biblical archaeology has been “weighed in the balance and found wanting.” Although not all American archaeologists in Syria/Palestine rejected the earlier terminology (Lance 1982), most now prefer “Syro-Palestinian archaeology,” or a similar, specific political/geographic term (Dever 2003). This is not just a nominal shift, but reflects a major theoretical and methodological change that has been labeled a revolution (Dever 1981). A new consensus has formed around principles articulated by the anthropological archaeologists working in the United States. The clearest sign of the change in Palestine is in the current research designs, field projects, and preliminary publications of American archaeologists in Palestine. A thorough evaluation of the new theory and method will occur when all the new projects are published.

Why, then, look at the old? No revolution is ever complete, particularly a scholarly one. The new paradigm of Syro-Palestinian archaeology carries the stamp of its parent, biblical archaeology. The senior figures in the field, those who brought about the change in paradigm, are all products of biblical archaeological training. In the popular mind, the biblical archaeology paradigm is still alive and well, as witnessed by the success of the *Biblical Archaeology Review*. I believe the reason for this dichotomy is a failure to recognize the

changing nature of biblical archaeology through time. This study elucidates the changes that did occur during the lifetime of biblical archaeology, following a chronological framework.

This study traces the interaction of biblical studies and archaeology in Palestine. Archaeology carries the connotation of fieldwork, and this study highlights the field aspect of biblical archaeology. An immense amount of data was gathered under the paradigm of biblical archaeology. This study enables that data to be more useful for current research by clarifying the theoretical and methodological framework of the original excavators. Until the 1920s, biblical scholars remained on the sidelines, although they were actively supporting archaeology and using the data gained from excavation. William Foxwell Albright brought biblical archaeology into the mainstream by conducting field research to ultimately aid biblical scholars. Biblical archaeology gained its prominence in Palestinian archaeology due to Albright's brilliant breakthrough in field methodology. Ironically, Albright's student, George Ernest Wright, would bring about the demise of classic biblical archaeology by continuing the tradition of methodological experimentation.

Biblical archaeology was, in simplest terms, a search for realia. It was an attempt to ground the historical witness of the Bible in demonstrable historical reality. Throughout its history, it was linked to this aim. Only when the archaeological data themselves became recognized as dependent on interpretation for their meaning (in other words, no longer seen as purely objective data) did biblical archaeology lose its positivist foundation, and collapse.

The history of classic biblical archaeology is ultimately a history of an aspect of biblical studies, not archaeology. As will be shown below, the theoretical base for the archaeology lay in the field of theology. This is why biblical archaeology was almost exclusively an American endeavor. American Protestantism strongly resisted the inroads of continental biblical criticism, and research in the ancient Near East became a potential source of support for the conservative opponents of critical study. Biblical archaeology became a weapon in theological debate, ultimately being very closely linked to the biblical theology movement by George Ernest Wright. The practitioners of biblical archaeology believed, albeit in different ways, that biblical faith, both Christianity and Judaism, depends on the historical reality of the events that displayed the Hand of God. If the events that the Bible interprets as the intervention of the divine have no basis in reality, then there is no basis for believing in the biblical witness. Thus, any evidence that might help to buttress the hope of faith is welcome. Here is the ultimate drive for realia. The archaeology of Palestine, the Land of the Bible, became biblical archaeology.

Biblical archeology still has validity as a name for the sphere of interaction of archaeology and the Bible. The new biblical archaeology is currently racked by fierce polemics (e.g., Dever 2001; I. Finkelstein and Silberman 2001). Ironically, archaeology is once again a weapon to be used to further particular biblical perspectives. The “maximalists” accept a certain level of validity to the historical witness of the Hebrew Bible; the “minimalists” reject any historicity associated with the Hebrew Bible and consider it to be a product of later Judaism. As will be explored below, both sides in the debate employ archaeology in the same way, as did Albright and Wright, as a source of objective data.

This book is a revised version of my Ph.D. dissertation at the University of Arizona, first prepared in 1987. I give my very special thanks to Dr. William G. Dever, teacher, advisor, and friend, for without his belief in this topic the study would not have been completed. Thanks are also due to the late Professor James A. Sauer, formerly of the University of Pennsylvania, for permission to use the library and archives of the University Museum in Philadelphia; Douglas M. Haller, chief archivist of the Museum archives, and his assistant, Georgianna Grentzenberg, for their aid in researching C. S. Fisher; and the Committee of the Palestine Exploration Fund and the secretary of the Fund, Dr. Rupert Chapman, for their permission to examine and use the archives of the Fund. Many scholars generously gave of their time to read and review various stages of this manuscript: Dr. Steven Falconer of Arizona State University, Dr. Peter Machinist of Harvard University, Dr. Bonnie Magness-Gardiner of the U.S. Department of State, Dr. Michael Schiffer of the University of Arizona, and my first editors, Chris and Linda Hulin. A special thank you is given to Dr. James K. Hoffmeier of Trinity International University, who reawakened my interest in the topic when he invited me to join him on an excavation in Egypt, after I had been a long time away from the Levant. My deep personal thanks to my brother, Dr. Edward B. Davis, for his practical suggestions and encouragement in the dissertation process; my parents, for their belief in my ability; and most of all to my wife, for her unending patience, love, and support.

Winston Churchill, while paying tribute to his predecessor, Neville Chamberlain, described the perspective that history can bring to an endeavor:

It is not given to human beings, happily for them, for otherwise life would be intolerable, to foresee or to predict to any large extent the unfolding course of events. In one phase men seem to have been right, in another they seem to have been wrong. Then again, a few years later, when the perspective of time has lengthened, all stands in a different setting. There is a new proportion. There is another

scale of values. History with its flickering lamp stumbles along the trail of the past, trying to reconstruct its scenes, to revive its echoes, and kindle with pale gleams the passion of former days. What is the worth of all this? The only guide to a man is his conscience; the only shield to his memory is the rectitude and sincerity of his actions. It is very imprudent to walk through life without this shield, because we are so often mocked by the failure of our hopes and the upsetting of our calculations; but with this shield, however the Fates may play, we march always in the ranks of honour. (1949: 550)

Biblical archaeology as understood by Albright and Wright is no more. Today, the field and its practitioners are often vilified. I hope that this study will allow a more sober, reasoned judgment of the achievements and the failures of biblical archaeology to be made. It is not the final word on the subject, for we are still too close for such surety. For biblical archaeology, the hopes failed, the calculations were upset, the realia were lost, but surely, it belongs in “the ranks of honour.”

Contents

1. The Beginnings, 3
2. The Albright Watershed, 47
3. Biblical Archaeology Triumphant, 95
4. The Collapse of the Paradigm, 123
5. The Legacy of Biblical Archaeology, 145

Bibliography, 157

Index, 171

4

The Collapse of the Paradigm

The dominance of the Albright-Wright paradigm did not go unopposed: Challenges to the theoretical outlook of biblical archaeology came from within its own ranks as well as from without. When Wright went to Harvard in 1959, Albright was on the point of retirement and, as a result, Wright's program came to be the most popular choice for prospective students. The fact that he was also actively excavating provided an additional incentive to study at Harvard. Ironically, some of the most vigorous challenges to biblical archaeology came from among Wright's own students.

External Attacks

The publication of Wright's *Biblical Archaeology* (1962a) provoked a storm of criticism in Germany directed against the entire methodology of biblical archaeology (Elliger 1959; Noth 1960). Albright (1939) had previously attacked Martin Noth's negative assessment of biblical historicity in the late 1930s, and as Dever (1980a: 3) has rightly observed, the virulent response to Wright may be seen as a belated reaction to Albright as much as opposition to Wright himself.

Karl Elliger (1959), a biblical scholar, reviewed the German edition of Wright's book. The review attacked both his methodology and his results and reproved him for his ignorance of critical issues (95). The heart of Elliger's critique was an attack on the treatment of archaeology

by the “Albright Schule” (96–98), and he was clearly bothered by the positivist nature of Wright’s treatment. He challenged Wright’s acceptance of archaeology as external objective evidence, which the “Alt Schule” had rejected. Elliger reacted more against Wright’s conservative biblical understanding than against his treatment of the archaeological data. It was a negative assessment of Wright the theologian, not Wright the archaeologist.

Wright added to the debate in an article entitled “Archaeology and Old Testament Studies” (1958), contending that archaeology’s main interests in Palestine were secular, an effort to answer historical and cultural questions. He reiterated biblical archaeology’s theoretical understanding of archaeology as objective and biblical criticism as subjective. He attacked the “Alt school” as “nihilistic” and “refusing completely to use archaeological data” (46, 47). He believed that Noth and his colleagues did not truly understand the revolution Albright had wrought in the usefulness of archaeology. A generation before, Albright (1939) had contended that the Germans were still locked into the pre-World War I rejection of archaeology on the grounds of its lack of precision. Wright concluded by stressing that even if one decides that a biblical account is in the form of a tradition, that is not equivalent to a negative assessment of its historical value.

Noth’s (1960) response to Wright was more substantial than Elliger’s. He strongly disagreed with the Albright-Wright assessment of Alt’s work (and, by extension, his own) as “nihilistic” (263 n. 1). He contended that the method of the German scholars was scientific, so it could not be labelled “nihilistic,” which is a description of attitude. Nihilism carried strong political overtones in Germany, particularly for a scholar who had lived through the Third Reich. Albright’s (1939) naming of the German approach as nihilistic was as loaded with meaning as the labeling of Albright as a fundamentalist. Albright reacted quite harshly to his own “supposed tendency to fundamentalism” (1934a), and Noth’s strong dismissal of the charge of nihilism carried the same air of defensiveness. Elliger’s implied accusation of fundamentalism against Wright was part of the German reaction against this charge. It is likely that Albright’s original contention reflected the political climate of the late 1930s as well as the nature of German methodology in biblical studies.

Noth (1960: 272 n. 2) complained about Wright’s accusation that the Germans refused to use archaeology in their studies. He asserted that they had, but without a facile acceptance of the Bible to be found in biblical archaeology. He specifically attacked the equation of Abraham with the MBI period and the use of Nuzi parallels in the Patriarchal reconstruction, noting that Glueck and Albright had put Abraham into MBI because of the Negev settlements, yet they used texts that were centuries later to point out parallels that then functioned

as added “proof” of the historicity of the Patriarchs. Noth dismissed Albright’s evidence as too divergent, and concluded that in spite of the efforts of Albright and Wright, archaeology shed little light on the issue of the Patriarchs (265–70).

The most damaging question Noth (1960: 271 n. 1) raised dealt with the nature of archaeological evidence: Is it truly external and objective? The fundamental datum of biblical archaeology, the belief in archaeology as *realia*, was under attack. Noth wondered if the interpretations of the data that the Albright school accepted were based on a particular biblical understanding, which would make them internal, not external to biblical study.

Proving the Bible?

Nelson Glueck’s *Rivers in the Desert* (1959) provoked an enlightening exchange on the purpose of biblical archaeology between J. J. Finkelstein (1959) and Wright. Glueck’s bald statement regarding the reliability of the Bible and archaeology—“It may be stated categorically that no archaeological discovery has ever controverted a biblical reference” (31)—provided the base from which Finkelstein attacked biblical archaeology in general. He accused Glueck of championing a new thesis that allegedly demonstrated the historicity of the Bible and countered Glueck’s statement with a presentation of the Jericho evidence. In the process of discussing this site, he went on to attack Wright’s treatment in *Biblical Archaeology* of the Jericho evidence as well (344), arguing that when Wright used the phrase “virtually nothing” in discussing the evidence for Late Bronze occupation at Jericho, he meant “nothing,” and the “virtually” was only a “scholarly hedge.” Finkelstein added that Wright’s use of “discouraging” in reference to the Jericho evidence “speaks volumes on the subject of scholarly detachment in the area of Biblical Studies” (344).

Wright (1959) responded by saying that the “historicity” school may have overstated its case, but that this thrust was only a small part of the growth of the discipline of archaeology in Palestine. Mentioning the work of Harvard, Yale, Penn, and the Oriental Institute, Wright maintained that “fundamentalist” money was never a major support of archaeological research (103). Clearly sensitive to the charge of fundamentalist domination of the field, he had written against such an understanding elsewhere (Wright 1958). He did not admit that the biblical connection began archaeology in Palestine, that ASOR had been supported in part by religious institutions, or that Tell Beit Mirsim was almost fully funded by a fundamentalist, Melvin Grove Kyle. But he was indulging in something of an overreaction; moreover, he ignored his own theological agenda, which he carried with him to Shechem.

Regarding the statement of Glueck's that touched off the furor, Wright admitted it was extreme, but pointed out the qualifications that Glueck used, such as the existence of legend in the Bible, concluding that "the total context is not as extreme" (1959: 106). He stressed that *Rivers in the Desert* was not a scholarly work, but was meant for a popular audience. Moreover, Glueck believed deeply in his faith and was an emotional man; the implication was that he allowed himself to get carried away. Wright did not agree with the straightforward apologetical overtones of Glueck's statement, although his own views at this time were almost as positivistic.

It should be noted that Finkelstein seemed to equate scholarly detachment with a lack of any a priori framework for interpretation, and while accusing Wright of operating within a preexisting framework, he himself interpreted his data to fit his own preconceived ideas. A preconceived notion is not in itself a bad position to start from; the key lies in recognizing it for what it is and being willing to allow it to be challenged. Certainly Wright was willing to test his ideas about the historicity of the Bible; it is why he dug at Shechem. He was very forthright about his own a priori assumptions as early as the 1940s (Wright 1946). Nonetheless, Finkelstein implied that anyone accepting the theology of the Bible was incapable of "scholarly detachment." For him, such a presupposition was unacceptable; by implication, the theological rejection of the Bible was the only valid position for a detached study of it. Clearly, this too must be rejected: Both positions are presuppositions. If they are admitted to be such and their potential bias recognized, then a holder of either position can be "detached." Theological beliefs do influence archaeological interpretation, and a negative theological position has just as much potential for bias as a positive one.

Postwar Fundamentalist Archaeology

Wright was much more liberal theologically than the very conservative scholars who continued to be involved in archaeology in Palestine after World War II. Joseph P. Free became a leading spokesman for the Christian fundamentalist position in archaeology, who did set out to "prove" the Bible (figure 4.1). Free had a Ph.D. in French, but became enamored with archaeology. Unlike many conservative writers using archaeology in their studies, he actually worked in the field. He was influential in conservative circles, because he produced students with some knowledge of archaeology, not just the Bible. He ran a program in Biblical Archaeology at Wheaton College (Illinois), founding it in 1940. Free wrote his major textbook, *Archaeology and Bible History*, to answer "the

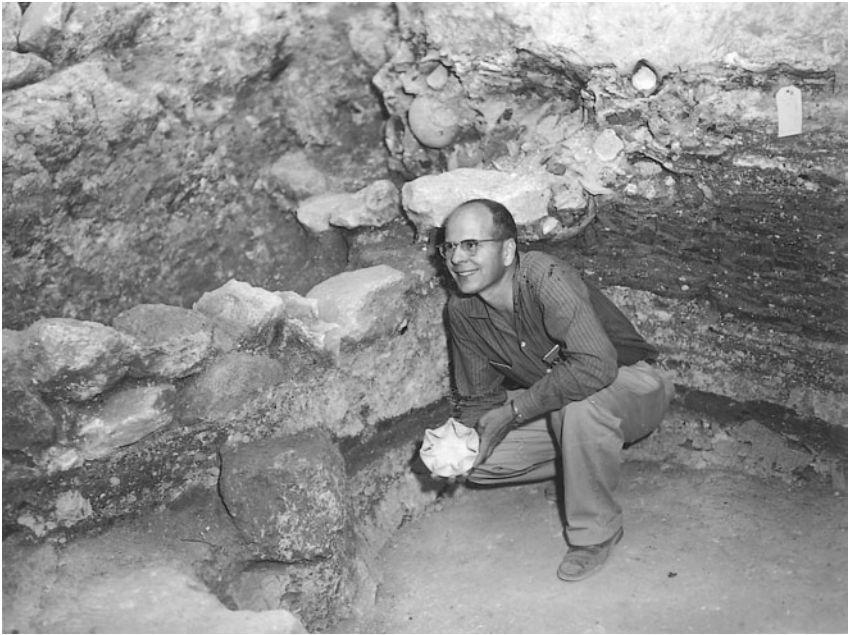


FIGURE 4.1 Joseph P. Free at Dothan. Courtesy of Wheaton College

need for a book true to the Scriptures and at the same time sufficiently documented” (1950: vii). He made no attempt to hide his bias, flatly stating, “The writer holds a very conservative position” (ix). Free had two uses for archaeology in his schema: to illuminate the world of the Bible, and as “a valuable part of . . . apologetics” (1). He called himself a fundamentalist and a “Bible Believer” (350). He accepted the idea of verbal inspiration, believing the Bible to be accurate in all respects (3). From that position, Free totally rejected higher critical thinking. He claimed that the confirmation of the Bible was not his primary aim, but the thrust of the study is totally apologetical.

Free (1953, 1954, 1955, 1956, 1958, 1959, 1960) directed the excavation of the biblical site of Dothan in the 1950s. He chose Dothan because it was connected with the Patriarchal cycle of stories. Methodologically, he followed the classic Fisher method, but with some influence from the work of Kenyon at Jericho. Squares were laid out using baulks, but apparently they did not function as sections. Free died in the 1960s before any report could be drawn together, and no thorough summary has as yet appeared, so it is impossible to assess how thorough his recording was. The most important discovery is Tomb I, a huge tomb dating to c. 1300 B.C. (Free 1960). The tomb has yielded more

than a thousand vessels. John Monson, a Ph.D. archaeologist from Harvard, now leads the program at Wheaton College, and he intends to publish the Dothan material as extensively as possible (personal communication, 2002).

The Challenge from Within

Students of Albright and Wright, and biblically focused archaeology colleagues, also challenged the Albright-Wright synthesis. The new generation of students trained by Wright at Shechem was nearly all clergymen, but many left the pulpit to focus on archaeology. William Dever has described this career change as “not ‘defrocked,’ but unsuited” (personal communication 1983). These students, like all students, felt the need to distance themselves from their mentors. The result was cataclysmic for biblical archaeology.

Paul Lapp

One of the first to directly challenge Wright was Paul Lapp. Lapp fit the mold of biblical archaeology, being an ordained clergyman and a pottery expert. He studied under both Albright and Wright, doing his dissertation on the pottery of the Hellenistic and early Roman periods in Palestine (Lapp 1961), a study that remains normative for this ceramic horizon. He directed the Jerusalem School from 1961 to 1965 and remained as professor there until 1968. He began in the field at Shechem, where, under Wright’s tutelage, he developed his ceramic expertise. He went on to direct the excavation of ‘Araq el-Emir, the site of an apparent Jewish temple during the Hellenistic period (Lapp 1962). He also reexcavated Ta’anach and Tell-el-Ful for the American School. His career was cut short in Palestine by the Arab-Israeli war of 1967. Lapp was strongly pro-Arab, and decided to dig on Cyprus rather than remain in Jerusalem. Tragically, as he was preparing for the excavation at Idalion in 1970, he drowned while swimming off the coast of Kyrenia. He was only 39 years old.

In a series of lectures given in 1966, Lapp (1969) attacked both the presuppositions of biblical archaeology and Wright’s own field methods. He first looked at the process of historical research, emphasizing the presuppositions of any historian, and concluded, “History is ultimately a personal construction” (28). That being said, he directly challenged Wright, contending that only a secular historian could produce an “objective” history of the Bible, for such a scholar would not be concerned with “God’s Great Acts” (64) and would not suffer from the same bias as theologians. Thus Lapp, like Finkelstein before him, evidently thought that a secular historian’s bias would not affect his his-

tory in any negative way. For Lapp, “objective” appeared to equal “nontheological”; yet, that choice is in itself a theological one.

Lapp believed that biblical archaeology should be subject to the same archaeological standards as any other branch of archaeology; he would get no argument from Wright on that point. Similarly, he saw biblical archaeology as primarily an archaeological endeavor, in keeping with Wright’s de facto definition. Lapp and Wright diverged over the accuracy of the Bible. Lapp considered it the “height of sacrilege” to think archaeology could answer the question “Is Christianity true? The person who does that *undermines* faith by making it less than a gift” (91). Here is the heart of Lapp’s challenge: scholarship based on faith versus scholarship based on empirical fact.

Lapp became enamored with the field methodology employed at Tell Deir ‘Alla, an excavation in Jordan under the direction of H. Franken of Leiden (figure 4.2). In a long review of Franken’s work, he again attacked the methodology of Wright and biblical archaeology (Lapp 1970). Franken aimed to clarify the chronology of the Late Bronze–Iron Age transition by a stratigraphic,



FIGURE 4.2 H. Franken at Tell Deir ‘Alla in Jordan. Photo by author

not a typological, study of the pottery. Lapp considered American methodology as developed by Albright and Wright to be too rigidly locked into a typological analysis of the pottery that would be collected under the Wheeler-Kenyon method. This rigidity prevented the objectives of the excavation from being the determinant of the methodology employed (1970: 244).

Certainly Lapp put his finger on a weakness of biblical archaeology. However, by attacking the field methodology, he was only chasing the tail, not the dog that wagged it. As long as biblical archaeology focused only on questions of *Kulturgeschichte*, its practitioners would use a method best designed to answer those questions: the modified Wheeler-Kenyon method. To break free from such a trap, a new theory was needed that would permit a broader range of questions. Lapp's review was more of a polemic against Wright than a positive statement of what archaeology in Palestine should be. Wright had dismissed Lapp from his position as director of the Jerusalem School, which added a personal element to the debate. There is no way of knowing what direction Lapp would have taken, due to his tragic death off the coast of Cyprus.

Roland de Vaux

In the *Festschrift* for Nelson Glueck, Roland de Vaux (1970), one of the deans of Palestinian archaeology, wrote a quiet paper that was devastating in its insight into biblical archaeology. De Vaux was a French Dominican, director of the *Ecole Biblique* in Jerusalem (figure 4.3). Although he did not consider himself a biblical archaeologist, in many ways he exemplified what a biblical archaeologist should have been. Throughout his career, he used the social sciences, the techniques of biblical criticism, and archaeological data to explain the history of Israel. The Bible may have been a central part of his research, but it was not to the exclusion of other questions.

De Vaux was very active in field research in Palestine. He directed nine seasons of excavation at Tell el-Far'ah (north), the site of Tirzah, one of the capitals of the northern kingdom of Israel (de Vaux 1951, 1952, 1962). The archaeological history of Tell el-Far'ah matched well with its political history as recounted in the Bible. De Vaux (1973) was also involved in research on the Dead Sea Scrolls, directing the excavations of Khirbet Qumran, the remains of the community that buried the scrolls. Methodologically, he was not at the forefront in Palestine, content to use a modified Fisher method in his work. Ceramically, he was not in the same class as Wright and Albright, but was consequently less dogmatic in his classifications. Others were better in the dirt, but de Vaux had few peers at synthetic interpretation. His general studies (de Vaux 1961, 1967), with their broad database, walk a middle ground between



FIGURE 4.3 Roland de Vaux (left) and William Dever (right). Courtesy of William G. Dever

the positivist approach of Albright and the more negative views of Noth. Adding to the quality of de Vaux's work is his elegant style, which comes through even in translation. In de Vaux, the *Ecole* reached its pinnacle.

De Vaux condemned as an abuse attempts to “confirm” the Bible through archaeology (1970: 67). He accepted Wright's belief that the faith of Israel was founded on the interventions of God in history, but pointed out that archaeology can only validate the event on which the biblical writer has placed his interpretation, and part of that interpretation is seeing an event as the act of God. De Vaux had developed this position earlier in *Bible et Orient* (1967), particularly in the section entitled “Peut-on écrire une théologie de l'Ancien Testament?” (59–71).

As a monk, de Vaux was at heart a man of faith, and began each day on excavation with a mass. He was willing to assume the veracity of a biblical account: “Lack of archaeological evidence would not be sufficient in itself to cast doubt on the affirmations of the written witnesses” (1970: 70); even if the footprints of the Divine were harder to find, they were no less valid. Placing himself in the middle between biblical archaeology and biblical criticism, he believed that compromise was possible: “There should be no conflict between a well established archaeological fact and a critically examined text” (70). The key was his definitions of “well established” and “critically examined,” which

allowed him to escape current or potential areas of conflict. Following de Vaux's dictum, the problem of Jericho that so vexed the Conquest model could be neatly evaded by declaring either the archaeology not "well established" or the text not correctly understood from the critical perspective. In general, de Vaux believed that biblical archaeology had done very well in establishing the first part of the equation; it was the lack of a critical examination of the biblical text that he discerned in Wright and biblical archaeology.

A major problem in biblical archaeology that de Vaux dwelt on was the confusion of correlation between cause and effect. He used the example of the Conquest. Clearly, many sites in Palestine were destroyed at the end of the Bronze Age, the time at which the Albright-Wright reconstruction placed the Conquest. De Vaux pointed out the weakness in the reconstruction: that we have no clue archaeologically as to the perpetrators of the destructions claimed to be the work of the Hebrews (1970: 75). He compared this to the problem in classical archaeology of the Fall of Troy. The destruction of Troy VIIA is often attributed to the Greeks of the *Iliad*. However, a direct causal relationship cannot be demonstrated, although a correlation of the two sets of data, literary and archaeological, can be reasonably put forward. Finally, de Vaux reiterated his criticism of the lack of critical study in biblical archaeology—a direct challenge to Wright.

William G. Dever

A student of Wright's, William G. Dever, followed the lead of de Vaux to its ultimate end and totally rejected the validity of biblical archaeology. Dever began his career as a theology student, writing a thesis on the "Present Status of Old Testament Theology" (1959), and was ordained into the Protestant clergy. His interest in theology took him to Harvard to study under "one of the most vigorous exponents of theology today": G. E. Wright (Dever 1959: 157). However, by that point, Wright had lost his primary interest in theology and was deeply involved in archaeology, particularly with the Shechem project. In 1962, Dever accompanied Wright to Shechem and became enamored with archaeology. Like Wright, Dever (1974) felt the attraction of realia and became a pottery expert; his dissertation concentrated on the clarification of the EBIV period through the study of its ceramics. In 1965 he became director of the Gezer excavation, and later served as head of the Jerusalem School. Because of his "in-house" credentials, Dever's challenge of biblical archaeology was particularly influential. After leaving Jerusalem, he went to the University of Arizona, directing a program in Syro-Palestinian archaeology, which combined anthropological with traditional Near Eastern archaeology.

Dever (1974) fired a broadside against biblical archaeology in the 1971 Winslow lectures, challenging the whole idea of a discipline called biblical archaeology. His main target was Wright's de facto equivalency of biblical archaeology and Palestinian archaeology. Like Wright before him (see below), Dever found at Gezer that archaeological data speak only in response to a question, and that biblical archaeology was unacceptable as the dominant mode in Palestinian archaeology because it asked only very limited questions.

According to Dever, the new staff needed for excavation in Palestine would not ask the questions posed by biblical archaeology, but those raised by anthropological archaeology as practiced in North America. No longer would archaeology in Palestine be a subfield of biblical studies, relying on the Bible for its agenda. Syro-Palestinian archaeology (as Dever christened it, borrowing the term from Albright) would be treated as a field of general archaeology, subject to the same concerns and using the same methods. This would benefit both archaeology and biblical studies: (1974: 31):

The separation I have advocated will allow Palestinian archaeology to develop the kind of professionalism which I think is healthy, for it would mean at the very least the raising of standards and the development of new cross-disciplinary programs with our scientific and anthropological colleagues which will multiply many times the value of our excavation and research—not the least for biblical history. But the irony is that as long as our concern is primarily biblical history, Palestinian archaeology will not be able to develop its full potential, and it will remain an amateurish affair not able to command the respect of scholars in other fields, rendering more of a disservice than a service to the cause of biblical studies. (1974: 31)

Essentially, Dever called for the recognition of what Wright (1947) had first advocated in his earliest definition of biblical archaeology: that Palestinian and biblical archaeology have differing interests. Dever rejected “biblical archaeology” as the name for the discipline, because the new questions being asked in the field would demand it: “The term ‘Biblical Archaeology’ imposes a limitation upon our discipline in both scope and time . . . It assumes nothing of importance happened in the Land of the Bible except in the brief period of the second and first millennia B.C.” (1974: 33). In his new enthusiasm, Dever, like any other evangelist, overlooked some of the problems of the new Syro-Palestinian archaeology. In the Winslow lectures, he made it clear that, like Lapp, he thought objectivity was a problem only for theologians, not archaeologists. Dever (1980a) has since admitted that all scholars have this problem, and that Wright was not overly biased in his archaeology.

The Final Straw: The Loss of Realia

Wright's rediscovery of the actual conditions of archaeology in the field at Shechem and later at Gezer added to the pressures directed against biblical archaeology. The *raison d'être* for biblical archaeology was the belief in the objectivity of the archaeological record. The true subjective nature of the archaeological process questioned this basic tenet. Wright was forced to reexamine his own position, and as a result, he reacted passively to the mounting attacks on biblical archaeology.

The Lessons from the Field

The Shechem field method started a process of profound change in Wright. He decided to use the methodology of Kathleen Kenyon, and this decision forced him to pay close attention to stratigraphy. He soon came to realize that the complex record of a tell site allows for many interpretations (Wright 1962b: 39). Stratigraphic questions could not be answered with a blanket principle, for each case had to be examined on its own merits and decided from its individual context. Without a guiding principle, individual interpretation came to the forefront. Through this analytical process, Wright began to think that data and interpretation were more closely linked than he had previously been willing to accept. If this were true, then archaeological data were not the objective realia called for by biblical archaeology.

As the Shechem excavations ended, Wright turned in 1964 to Gezer, a site in the state of Israel. Like Shechem, Gezer had been excavated before (by Macalister and Rowe; see above), using older, less sophisticated methodology. Wright was archaeological director at the Hebrew Union College Biblical and Archaeological School that year, and the school sponsored the excavations. He functioned as the director for the first season, after which he removed himself from active fieldwork to allow his student, William Dever, to take over (Dever et al. 1970, 1974). Wright and Glueck (the president of the sponsoring institution) formed an executive committee to free Dever and his assistant, Darrell Lance, to concentrate on the actual excavation and analysis.

The Gezer excavations employed volunteer labor and ran a field school, both firsts for American excavations in Palestine. The initial excavation staff reflected the Shechem approach, being composed largely of ceramic specialists. These men were mostly former students of Wright, and many were ordained clergy (Dever 1974: 12). The staff quickly changed, however, being joined by

specialists from other disciplines. Gezer fulfilled Wright's hopes for such an approach, which had been frustrated at Shechem (Stager et al. 1974: xvi).

Dever actively pressed for the multidisciplinary approach, which by 1970 was a permanent fixture (Dever et al. 1970). He presented this in an article on methodology for *Eretz-Israel* (Dever 1973), calling for the application of the refined Gezer method to one-period sites. Due to the intensity of recording, this methodology does not allow for more than a moderate exposure of any area under study, and thus, following Wright, Dever called for more problem-solving archaeology. Most important, Dever posed questions: What is the basic conception of the excavator? What can we realistically hope to accomplish? "Classical" biblical archaeology no longer had an answer.

During Wright's presidency, ASOR sponsored renewed excavations at Tel el-Hesi, the birthplace of systematic excavation in Palestine. From its inception in 1970, work at the site was multifaceted, using specialists from many scientific disciplines as well as traditional archaeologists. In 1968, the Hesban project, sponsored by Andrews University and affiliated with ASOR, brought a multidisciplinary approach to Jordan. Like Gezer and Hesi, Hesban trained numerous students who went on to lead projects of their own. These excavations brought the multidisciplinary approach pioneered in the Near East by Robert Braidwood to biblical historic sites.

I think Wright's support of these changes also reflected his questioning of the objective nature of the archaeological record. If, as Wright was beginning to suspect, data speak only in response to a question, then more information might be gathered by more questions. A multidisciplinary staff would naturally ask more questions of the data than would a *Kulturgeschichte*-oriented staff.

Archaeological Reaction

Wright (1971) made clear his awareness of the loss of realia when he rejected his old confidence in the objectivity of archaeological data in an article for the *Biblical Archaeologist* entitled "What Archaeology Can and Cannot Do," a direct reference to Roland de Vaux's earlier paper (1970). He quite openly stated, "Archaeological data can only speak in response to a question" (1971: 73). Wright now accepted the use of models and hypothesis testing in archaeology, first making this clear in the 1968 Sprunt lectures at Union Seminary (Wright 1969c). In these theological studies, Wright stated that models impose a necessary form on material—necessary, he reasoned, because we do not receive our data raw and unfiltered. In other words, the interpretation of a fact is an integral part of that fact.

Overt model building and hypothesis testing were new for Palestinian archaeology, although they were a recognized element of anthropological archaeology as practiced in the United States and Europe (Willey and Sabloff 1980). Wright, as we have seen, encouraged the cooperation of anthropological archaeologists and natural scientists on ASOR-sponsored excavations. Despite this apparent support for anthropology, however, he remained adamantly opposed to compartmentalization and specialization, two elements that he particularly associated with anthropological archaeology (1971: 73–75). Wright believed that anthropologists had “short-changed the humanistic aspects of archaeology” in an overzealous attempt to remain “‘non-historical’ and ‘scientific’” (73). Finally, he returned to the vexing question of the objectivity of archaeology in an extremely atypical declaration: “Ambiguity is a central component of history” (75). Realia could no longer be found in the dirt.

Paul Lapp’s sudden death in 1970 forced Wright to become directly involved in Lapp’s field project at Idalion in Cyprus (Stager et al. 1974). Wright was needed to help secure a permit from the Cypriote Department of Antiquities. During the Idalion project, Wright stayed at the Ledra Palace Hotel in Nicosia. Each morning he would send instructions via Sophocles Hadjisavvas, a young Cypriote archaeologist who was commuting from Nicosia to Idalion (personal communication 2003). Dr. Hadjisavvas continued in archaeology and served as director of the Cyprus Department of Antiquities from 1999 to 2004.

Idalion was yet another example of the multidisciplinary approach that had gained such a fervent disciple in Wright. He placed Ruben Bullard, a geologist who had worked with Wright at Shechem and Gezer, in charge of coordinating the scientific specialists. Bullard discovered one of the clay sources of the famous Cypriot White Slip ware of the Late Bronze Age (Stager et al. 1974: xvii). Bullard later worked on Cyprus at Kourion in 1984 with the University of Arizona excavations in the city site. Frank Koucky, Bullard’s teacher, also joined the Idalion team and became the main geologist on the project.

Wright (1974) presented a paper at a 1972 colloquium at MIT on complex societies, addressing the issue of the tell as a basic unit in the Near East. His presentation was still essentially historical, dealing with the tell as an independent unit rather than an element of a regional study. Gordon Willey (1974: 146), the moderator of the colloquium, criticized Wright’s “‘traditional’ archaeological perspective.” Still, the mere fact of his presence at a prestigious anthropological colloquium demonstrated the changes in Wright.

In a posthumously published “fireside chat,” Wright (1975) continued his new-found approach, praising the “new archaeology” of American anthropology and urging his students to explore new options in theory and method.

However, he insisted on a humanistic orientation: "I remain unabashedly a humanist!" (115). In an earlier work, he had defined his humanism as follows: "I am an individual with a history not entirely predetermined by gene or environment. I am a creature of choices, a bundle of biases in my past and present relationships with others, with my environment. In other words, what is important to say is that I am a historical being always making choices . . . I too possess a power structure with the ability to create what no tradition, no depth psychologist, no environmentalist—no combination of them—can entirely predict" (1969a: 63–64). Wright (1975: 115) warned the "pure scientists" that archaeology deals with human beings and thus must remain a humanistic discipline.

Theological Reaction

Biblical Theology came under general attack during the same time that Wright was rethinking his views on realia. Wright's idea of history as the sole medium of revelation was challenged on the grounds that it left out the possibility of revelation in word (Barr 1961, 1976; Childs 1970). Israel's belief in revelation in history—considered part of the uniqueness of Israel by Wright—was shown to be a commonly held belief in the ancient Near East (Albrektson 1967). The relevance of Wright's theology (a concept important in the 1960s) was called into question (Childs 1970: 82–87): Even if God did act in Israel's history, does that have any meaning for today? Under these attacks, the consensus that had formed around Biblical Theology broke apart. By 1970, Brevard Childs (1970; Dever 1985) declared the "death" of the movement.

A Foundation of Sand

In the 1968 Sprunt lectures at Union Theological Seminary, Wright (1969c) worked out the theological implications of the loss of realia in archaeology, which should also be seen against a backdrop of the theological challenges to Biblical Theology. He remained convinced that archaeology should be a tool for the theologian: "If the Bible is the revelation of a new reality in a Near East time and place, why should not the historian's tools be my ally?" (67). However, he had lost faith in the role of archaeology. I think his encounter with the actual state of the archaeological record had changed his views on the directness of the Hand of God in history. "God works in this world by mediate means," said Wright. "Our problem is to know and do what we are called to do" (130). The verification of the Acts of God, which Wright was so confident of before the

Shechem excavations, was now unachievable, and he concluded that “the problem of the Scripture’s truth and validity cannot be solved” (184), abandoning his previous positivist stance. “In the end we can never measure this Biblical reality with reality itself, whether we attempt this measurement in the field of value or in the field of fact . . . God has not committed his truth to respond adequately to our tests” (185). Despite decades of research, the goal of biblical archaeology, to ground the Scripture in realia, was no longer possible. Wright had come full circle. His theology had originally provided the impetus for his archaeological research. Now, that same research forced him to abandon his theological stance.

The Joshua Commentary: A New Direction

Wright died while working on a commentary on the book of Joshua in the Anchor Bible Series. However, he did complete an important introduction, which presents a fascinating example of the profound changes in his thinking wrought by his archaeological loss of faith (Wright and Boling 1982). The subject under study was one of the key subjects of biblical archaeology: the Conquest. “The conquest has received little theological study,” wrote Wright, “though, of course, it has been of great importance for Palestinian and biblical archaeologists because of the apparent opportunity for an external check on both biblical and archaeological chronology” (34). For Wright, archaeology’s role as an external, objective check on the excesses of biblical criticism had become only apparent, not true reality.

Wright employed all the resources at his command in setting the stage for the theological analysis of Joshua. He discussed the history of modern critical study of the text (Wright and Boling 1982: 37–72), demonstrating his mastery of all the critical apparatus. He thought the final form of Joshua was the work of a Deuteronomic editor who used several preexisting sources. He found the character of Joshua the individual to be central to the tradition, which he preferred to see as the product of traditions of a Holy War (72, 27–37).

Wright also discussed the “historical problems” of the book of Joshua (Wright and Boling 1982: 72–88). He praised the recently deceased Albright as “the dominant creative figure in the attempt to place the Bible in a perspective of the whole of ancient history” (73). Downplaying his own important contributions, he credited Albright with establishing the basic chronology that linked the events of Joshua to the thirteenth century B.C. He then recognized a major weakness of the biblical archaeology reconstruction: “Yet a carefully defined statement of what archaeology is and is not, does and does not do, has been hard to articulate. Such a statement must follow the experiments of re-

construction, and first attempts may need future modification when the polemical period which is always created when general assumptions are badly shaken is past" (74). Wright considered de Vaux's (1970) article in the *Glueck Festschrift* to be one of the "outstanding attempts" at such a statement. He then presented his own position on "What Archaeology Can and Cannot Do" (74-80), which owes much to his earlier article of the same title (Wright 1971).

After a quick sketch of the development of the field, Wright predicted that the new multidisciplinary staffs on American excavations would generate a much greater amount of controlled information in the 1970s and 1980s. This is one prophecy that has proved to be 100 percent accurate. He then presented his postexcavation assessment of the relationship of biblical events and archaeology:

With regard to biblical events, however, it cannot be overstressed that archaeological data are ambiguous. Fragmentary ruins, preserving only a tiny fraction of the full picture of ancient life, cannot speak without someone asking questions of them. And the kind of questions asked are part and parcel of the answers "heard" because of predispositions on the part of the questioner . . . It is small wonder, then, that disagreement and debate arise. A destruction layer in the ruins does not tell us the identity of the people involved. Indeed, we know that certain black soot and charcoal layers do not necessarily mean destruction. An accidental fire in one part of the town or city, certain industrial pursuits, or even an earthquake may be the answer. (Wright and Boling 1982: 76)

Wright has learned the lessons of the dirt.

Wright still believed archaeology could contribute to biblical study: "Yet the nature of the remains does not mean that archaeology is useless" (Wright and Boling 1982: 76). However, for archaeology to contribute it must be understood as a source of

historical reconstructions [that] have varying degrees of probability. In studying antiquity it is important to recall that models and hypotheses are the primary means by which reconstruction is possible after the basic critical work is done. And, furthermore, it takes a great deal of humility to say frankly what the physical sciences have had also to say; predisposition of minds at any one period frame the type of questions asked of the material and become a part of the "answers" we suppose we have obtained from our investigations.

(77)

The old positivism is gone. The parameters of realia in biblical archaeology are circumscribed and finite:

Final *proof* of anything ancient must be confined to such questions as how pottery was made, what rock was used, what food and fauna were present, etc. Certainly this proof does not extend to the validity of the religious claims that the Bible would place upon us, and we must remember that the Bible is not a mine for scientifically grounded certainties about anything. It is instead a literature that places before us one of history's major religious options. (Wright and Boling 1982: 77)

Wright turned to Roland de Vaux to find a guiding principle: "The dictum of de Vaux is axiomatic: 'Archaeology does not confirm the text, which is what it is; it can only confirm the interpretation which we give it'" (Wright and Boling 1982:79). In summary, Wright expanded on his earlier (1971: 75) observation regarding the ambiguity of history: "We are historical organisms by intrinsic nature, and ambiguity is always a central component of history, whether of the humanities, of social science, or of natural science" (Wright and Boling 1982: 80).

The Destruction of the Model

Wright had been forced by his own field experience to come to terms with the actual subjective process of archaeology. In so doing, he had to disregard the view of archaeology as the realia of biblical studies, going against two generations of work by biblical archaeology. His repudiation of the field's theoretical base amounted to the destruction of the movement. After biblical archaeology lost its theoretical foundation, the twin pillars of the Albright-Wright reconstruction, the Patriarchs and the Conquest, soon collapsed.

The Collapse of the Patriarchal Model

Major studies by Thomas Thompson (1974, 1978) John Van Seters (1975), and William Dever (1970, 1977, 1980b) challenged Albright's Patriarchal model. These studies attacked from both biblical and archaeological positions, for if the Patriarchal narratives could be cut loose from their Middle Bronze moorings, then biblical criticism could regain its once dominant role in the discussion of Genesis and the Albright revolution would be overturned. Thompson, Van Seters, and Dever disagreed over aspects of the EBIV (MBI) period (see

Dever 1977, 1980b) but were united in their view that Albright's Patriarchal model was a distortion of the archaeological record. Van Seters was more concerned with questions of biblical criticism, and it was primarily Thompson and Dever who critiqued Albright's archaeological data.

Following the work of Dever (1970) on the EBIV period it became clear that Albright's dates for the beginning of MBIIA had to be raised to c. 2,000 B.C. (Dever 1985), pushing his MBI back into the third millennium. Albright depended heavily on the Royal Tombs at Byblos for his dating, but the full publication of the tomb material by Olga Tufnell (1969) allowed Thompson (1974) and Dever (1977) to demonstrate conclusively that his dating of the tombs to the eighteenth century B.C. was wrong. Thus, the connection of the caravan texts from the nineteenth and eighteenth centuries B.C. to the MBI (EBIV) sites that Glueck had surveyed in the Negev could no longer be maintained. These sites are now placed in the last quarter of the third millennium B.C. The urban data included in Albright's reconstruction have also been challenged. Dever (1977: 99) dismissed Albright's characterization of the EBIV settlement at Bethel as extensive, contending that only a few sherds represent that early phase of the town, and the same date for Shechem was also rejected (Dever 1970: 142-44).

Thompson and Van Seters argued that the Patriarchal accounts contain no historical value, and any attempt to place them in the early second millennium, as biblical chronology requires, is doomed to failure. Dever approached the problem as an archaeologist, dismissing the Patriarchs as a biblical problem, not an archaeological one: "It should be noted that few archaeologists who specialize in MBI have even alluded to Albright's view, and none has accepted it" (1977: 102).

Thompson (1974: 62-88) discussed the evidence for the Amorite connection, pointing out that the picture of Amorites migrating into Syria/Palestine ultimately depends on the biblical stories of the Patriarchs. Only in Genesis, he contended, does a picture emerge of migration from the Euphrates valley to Palestine (Thompson 1978). In rejecting the Amorite hypothesis, Thompson also argued for continuity between EBIV and MBIIA, downplaying any evidence of nomadism in Palestine. Dever initially accepted the Amorite hypothesis (1970), but has since rejected this as an explanation for the new features in MB Palestine (1980b). However, he strongly differed with Thompson's idea of continuity between EBIV and MBIIA and dismissed his notions of nomadism as naïve (1980b: 53-55): "Finally, the 'Amorite' question may be resolved. The considerable linguistic evidence for an 'Amorite' population in Syria-Palestine is now credible—not on the supposition that an 'Amorite' invasion from Upper Syria and Mesopotamia had taken place, but by the recognition

that the indigenous Middle Bronze Age population had *always* been West Semitic or ‘Amorite’” (58).

The destruction of the Patriarchal model has not gone unchallenged and attempts to reconnect the Patriarchs and the EBIV period have been made. John Bimson (1983) has argued that the key to placing the Patriarchal accounts in the archaeological sequence is to recognize that a long period of time is involved. He contended that Abraham dates to the EBIV predominantly, but that later Patriarchal activity dates to the MBIIA period. Bimson rejected the Amorite hypothesis (61) but pointed out that this rejection did not negate the journeys of Abraham, only their connection to a hypothetical “Amorite” invasion. After all, the biblical accounts center on only one family, not an entire national group. Interestingly, despite supporting the historicity of the patriarchs, Bimson did not accept archaeological evidence as completely external and objective, realizing the “limitations of archaeological evidence, and the uncertainties surrounding its interpretation” (88–89). Even in the approach of a scholar sympathetic to the endeavor of biblical archaeology, the old certainty was gone.

Conservative scholarship retained the linkage of the Patriarchs and Middle Bronze Age Palestine, although not to the EBIV period. Alfred Hoerth, a University of Chicago–trained archaeologist who worked extensively in Egypt and Nubia, directed the archaeology program at Wheaton College after the death of Free. Hoerth was a student of Braidwood and brought a multidisciplinary perspective to the conservative religious school. His recent overview, *Archaeology and the Old Testament* (1998), retains the linkage of the Patriarchs and the Middle Bronze Age. Hoerth places Abraham mostly in the MB, seeing his world as more urban than the EBIV would permit (75–123). He is not a fundamentalist and denies the ability of archaeology to “prove” the Bible (18–22).

Against the Conquest

The Albright-Wright Conquest model has also been severely strained in recent years. As far back as the 1960s, George Mendenhall (1962) postulated a non-invasion model for the rise of Israel, believing that peasant revolt and social revolution better explained the conditions of the Late Bronze Age–Iron Age transition. This hypothesis was expanded in a massive treatment of the social context of early Israel by Norman Gottwald: “On closer examination, it turns out that there is as much—and maybe more—to be said against using the archaeological results to support the conquest model as there is in its favor”

(1979: 198). Gottwald followed the lead of de Vaux in emphasizing the lack of certainty in assigning a destruction layer to the Hebrews. Many destructions of Palestinian tells both before and after the thirteenth century have been essentially ignored, “but much can happen in twenty-five years that need not be attributed to a single historical agent” (202). The lack of evidence at Jericho and Ai, two central stories in the Joshua cycle, also led Gottwald to discount the traditional picture. He concluded:

As a self-sufficient explanation of the Israelite occupation of the land, the conquest model is a failure. On the literary-historical side, the biblical traditions are too fragmentary and contradictory to bear the interpretation put upon them by the centralized cult and by the editorial framework of Joshua. On the archaeological side, the data are too fragmentary and ambiguous, even contradictory, to permit the extravagant claims made by some archaeologists and historians using archaeological data . . . What must be avoided is a facile circle of presumed confirmation of the conquest, built up from selective piecing together of biblical and archaeological features which seem to correspond, but in disregard of contradictory features and without respect for the tenuous nature both of the literary and of the archaeological data. (203)

Gottwald explained the rise of Israel as a sociological reaction by oppressed peasants to the harsh rule of Late Bronze urban power structures. As a result, Israel rejected the urbanized lifestyle and the political order of kings and city-states. It must be pointed out that Gottwald’s blanket rejection of the biblical account of a conquest supported a theological rejection of traditional biblical religion. He hoped that his study would “close the door firmly and irrevocably on the idealist and supernaturalist illusions still permeating and bedeviling our religious outlook. Yahweh and ‘his’ people Israel must be demystified, dero-manticized, dedogmatized and deidolized” (1979: 708). This is a position poles apart from Wright’s “Mighty Acts.”

A recent survey of the history of Israel and Judah rejected any attempt to reconstruct “the age of conquest” (Miller and Hayes 1986: 90). Although still arguing for a Conquest, John Bimson (1978) also rejected the Albright-Wright thirteenth-century reconstruction, preferring to date it back into the fifteenth century b.c. Amihai Mazar, in *Archaeology of the Land of the Bible* (1992), summarized the tension between text and archaeology: “In some cases (southern Transjordan, Arad, ‘Ai, Yarmouth, and Hebron) there is an outright conflict between the archaeological findings and the conquest narratives, while in oth-

ers (Lachish, Hazor, Bethel) archaeology does not contradict these stories” (334). As *Harper’s Bible Dictionary* puts it, “Scholars continue to debate the nature and date of the Israelite occupation” (Mattingly 1985: 178).

Summary

The decade of the 1960s witnessed the collapse of the paradigm of biblical archaeology. Wright had begun those crucial ten years supremely confident in the ability of archaeology to provide an objective answer to the Kulturgeschichte questions of biblical archaeology. Yet, at the end of that decade he had rejected such a view by declaring, “Ambiguity is a central component of history” (1971: 75). Death added to the problems of the field, claiming Albright, Glueck, de Vaux, and Wright himself in the space of five years. In an ironic twist, the students Wright had trained pioneered the new paradigm of Syro-Palestinian archaeology.