Review: Review/Essay: Archaeology's Publication Problems

Reviewed Work(s): Archaeology's Publication Problems by J. Aviram and H. Shanks

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Archaeology's Publication Problems

Rashomon-like, I will offer two reviews of the same book,* an interesting volume which addresses a widely recognized, yet just as widely undiscussed, issue facing archaeology: the chronic delay in publishing excavation reports. The authors deal with Syro-Palestinian data, but in fact the problem extends to the Near East as a whole, and beyond it, it seems, to all cultural regions of the world. I will, in the first instance, express full agreement with the *communis opinio* to which the various authors give voice as to the nature and causes of a state of affairs which is universally decried. But I will also express a fundamental disagreement: the nature and causes identified are real, but envisage merely the surface of the issue. Rather, a much more radical problem is at issue, one which affects the very nature of the discipline.

The majority of the papers derive from a planning conference held in 1994, which was followed by another discussion meeting in 1995, but never resulted in the intended major follow-up conference. The editors added a couple of other papers by authors who had not participated in the planning conference. This implies that a considerable amount of thought has gone into the preparation of what appears now as the published version. The book is nicely produced (one minor editorial oversight is *stricto scensio* on p. 25, instead of *sensu*), and, however slim in size, it represents a solid frame of reference for the set of problems addressed. Three articles stand out because they play a central programmatic role: W. G. Dever, "The Importance of Research Design" (pp. 37-48); J. D. Seger, "Archaeology's 'Midlife Crisis'" (pp. 55-70); and Z. Herzog, "With Time We Are Getting Worse" (pp. 87-110). Other papers identify individual concerns (A. Mazar, A. Ben-Tor, plus the introduction by Phil King), address specific procedures and technologies (H. Shanks, P. F. Jacobs, G. Van Beek), and review aspects of the history of the discipline (J. Aviram, plus the article by A. Herzog).

Six major themes emerge. (1) The personal ability of the excavator as *a determined editor* is a central factor (pp. 19, 24-25; 28; 52; 103). Besides the individual skills (or should we say editorial ruthlessness?) of the director, the general importance of coordination is stressed (pp. 28; 37; 41; 62). (2) *A statute of limitations* should be imposed (pp. 16; 26; 36; 45; 51; 53). In this respect, it should be noted that such a statute is in fact written within the regulations of most Departments of Antiquities, which generally stipulate that after a certain period of time, jurisdiction of the data reverts to the Department. (3) *High costs* must be addressed (pp. 25, 27, 39, 106). The answers vary from suggesting that one forego large and expensive projects (p. 45) to placing trust in new technologies as cost-cutting devices (practical budget issues are not raised, which would indicate that such presumed cost-cutting is only wishful thinking). (4) The *nature of the record* is considered, as being extremely detailed and often incomprehensible as published: "who will read it?"; "what does it all mean?" (pp. 34, 39, 41, 100, 103, 105). Answers proposed vary from suggesting the creation of a new profession, the archaeological editor/writer (p. 51), who would, as it were, translate a congeries of undigested data into readable prose, to proposing acceptable requirements for the final report (p. 28)—suggesting that excavations aim only at answering questions asked explicitly (pp. 42, 43, 46)—to recommending a total publication of the record (pp. 35, 61). (5) Deeper issues are raised only occasionally, such as those pertaining to *epistemology* (pp. 42 with n. 4; 65) or *statistics* (p. 34).

It is surprising that only in one case is there explicit mention of the archaeologists' "*first obligation*...to record and preserve the data [they] excavate" (Seger, p. 66, emphasis mine). It is fair to assume that every other contributor would, in practice, agree with this principle, and the editor refers to a majority opinion that would hold "that we must publish everything" (p. 13). But it is indicative that the topic should not have otherwise been explicitly mentioned in a book which is, after all, devoted specifically to the question inherent in this obligation (what and how to publish). It is even more significant that the flow of the argument occasionally leads an author to maintain the exact opposite. The problem is inevitable when one follows too rigorously the logic of a research design, in such a way that the design becomes a filter which screens out evidence not relevant to the stated goals. Thus, Herzog maintains that "as long as the discovery of the finds represents a goal in itself, the problem [of delay in publication] cannot be overcome" (p. 106). "The researcher should determine the content of the excavation report in advance," he writes (p. 107), "based on the work methods that will be adopted in the course of the research." It is true that the practical consequences inherent in such a position are rejected explicitly in the following paragraph: "One way to overcome this problem is to select and publish only those data considered meaningful. This method must be entirely rejected, however." Reassuring as this may sound, the ominous implication is that the filter has been operative upstream of publication, at the moment of excavation, when the internal logic of the research design becomes a Procustean mechanism that "determines in advance" what are acceptable ("relevant") data. Doesn't the enthusiasm expressed on p. 43 (Dever) for the rapidity of publication that

Archaeology's Publication Problems
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follows a "narrowly focussed" research design betray a similar danger? On p. 42, it is stated that "we get 'answers' only to those questions that we are prepared to ask": if we take this literally, does it not mean that only the material relevant to the "research strategy" will generate enough enthusiasm to deserve publication?

Implicit here, it seems to me, is a fundamental contradiction in terms. If a research strategy is so narrowly defined that it aims to predict the nature of the evidence, whatever is found that was not predicted will not fit in that strategy. Why then publish it, if defining the strategy that limited the applicable range of evidence was the proposed solution to the publication problem? Clearly, the antinomy goes beyond the limits of our volume, as is shown by the repeated acclaim which many of the authors express for the tenets of the New Archaeology. Which leads me to the second perspective from which to look at this book.

"The primary reason for the delay in archaeological publications is the fact that excavators strive to publish first and foremost depositional inferences, rather than emplacement data."

Even if the point just raised may be seen as a *reductio ad absurdum*, it will serve to indicate why I ultimately find myself in total disagreement with the presuppositions and the conclusions articulated by the various authors. Not because they are untrue or inapplicable, but because they miss, in my view, the basic roots of the problem, and because when they begin to address deeper issues, such as the issue of research design, they propose attitudes and methodologies that are counterproductive. The alternative that I propose will be articulated here briefly under two major headings, leaving for another venue a fuller presentation of the argument. What I am saying here seems still pertinent to the review of the book under consideration, since, by proposing further areas of inquiry, I articulate more clearly the nature of the book's collective effort as bounded by its chosen horizons, which remain, in my opinion, too narrow.

A central presupposition on which my thesis rests has to do with the understanding of what archaeology is. The point is raised, more or less directly, by several of our authors (pp. 40, 47, 60, 65). But a fundamental point which is altogether ignored, and which has a radical impact on the very question of publication, is the distinction between *emplacement and deposition* as the basic components of stratigraphy. The primary task that an archaeologist performs, as no one else does, is the stratigraphic analysis of cultural remains. But what we identify in the ground is properly only the emplacement: discrete features and items, with specific boundaries and recognizable types of contact. Only such emplacement is demonstrable, not deposition. Thus, we cannot properly say that we excavate a foundation trench. What we document is the juxtaposition of volumes which are different in texture and consistency, defined by planes at certain inclinations: say, for example, a hard and compact mass, with inclusions aligned horizontally, bounded by sloping sides against which a softer mass of debris is contained, also compact but with non-aligned inclusions. This is documentable. That this should be interpreted as a foundation trench, cut into an earlier floor and containing compacted fill, remains an inference.

Clearly, the inferential conclusion, i.e., depositional history, is what ultimately interests us. But we must remember at all stages that it is only a suggestion, a reconstruction, based on a wider universe than what is immediately observable and documentable. Hence the fundamental significance for the topic of our book. The primary reason, in my opinion, for the congenital delay in archaeological publications is due to the fact that excavators strive to publish first and foremost depositional inferences, rather than emplacement data (alternatively: only those emplacement data are published which support a given depositional inference). Since inference cannot be documented as such, one does naturally want to increase the margin of safety and validity for the inference by obtaining an ever wider exposure. In other words: if one aims to give an account of depositional history, it is natural that the wider the excavation, the better we ought to understand it. But exactly the reverse is true of emplacement: once the observation has been made and recorded, absolutely nothing more can be added by excavating more, comparing more, reviewing one's notes one more time. Quite the opposite is true: the more one waits, the more the emplacement record becomes subordinated to the understanding of deposition, and presentation of the original observation is subjected to the growing conviction about one's own interpretation of the data observed. So, the crux of the problem of delay in publication lies in the archaeologist's timidity about giving its due to emplacement. To do so would be, I submit, the truly professional approach, that would both solve the publication problem and create the proper respect for the evidence as originally observed.

The second major point that needs mentioning is the insufficient distinction made between *technique and method.* There is, in my view, an excessive reliance on technology, as if by itself it could get reports published. But technique, without method, is as useless as it is deceptive. Think of well-established techniques, such as photography or surveying. Would the mere publication of photos (or video frames) of every single moment of an excavation make a good record? Not so—no more than the sum total of sentences of a language would make a good grammar. Does the presence of a precise grid guarantee the accuracy of stratigraphic relationships? Again—no more than a precise physical determination of colors could do justice to a painting. The same applies to electronic data processing, which is no less neutral for being more recent and more powerful than photography or surveying. It can rather make it easier to hide behind the cloak of "state-of-the-art." How much of the much-vaunted "interactive" dimension of data bases is in fact less interactive than simply leafing through a book? Hyperlinks provide at first an easier way of cross-referencing data than by looking up a normal printed index, but they can often be more limiting, since they are much more channeling and restricting. Or think of the home page approach to publishing (to which a fair amount of space is dedicated in the book under review, where a description is offered of one of the best examples in the field, the Lahav DigMaster Web site). One cannot in good faith give the name of "publication" to data which remain accessible to the public at the discretion of a functioning server. How can we consider as "public" record something which is not independently and invariably available to any interested reader, or rather, "user," a term that is descriptive of the lesser concern for thoughtful absorption? It is, in truth, no more than glorified underground publishing.

This is far from suggesting that any of these techniques are in themselves of no use. Quite the opposite (and I, for one, am an avid user of each and all of them). But techniques should be subordinated to proper methods. Thus, photography should be integrated in a much more detailed manner within the documentary aspects of stratigraphic emplacement, rather than showcasing the final understanding of the architecture (i.e., the depositional construct). Electronic data processing should provide a tiered approach, leading through a capillary system from the higher nodes to the most minute detail; this has to be structured according to a rigorous "grammatical" understanding not only of typology (where much has been done), but especially of emplacement and stratigraphy (where hardly anything is available in print). It is indicative that themes proposed at congresses pay little if any attention to stratigraphic issues. The discipline seems quite content in this respect, as if methods were fully articulated and could be taken for granted. In contrast, I think we must come to the realization that this is the single area of our discipline most in need of attention. Only from such a radical realignment of presuppositions and priorities can a solution ultimately come, I believe, to the problem of the chronic delay in archaeological publishing.

This map of the Khabur region accompanied an article by Giogio Bucellati and Marilyn Kelly-Bucellati in *BA* 60:2(1997):96. The original placed Mozan to the east of its correct location.