

Chapter Title: [I. Introduction]

Book Title: Anthropology without Informants

Book Subtitle: Collected Works in Paleoanthropology by L.G. Freeman

Book Author(s): L. G. Freeman

Published by: University Press of Colorado

Stable URL: <https://www.jstor.org/stable/j.ctt46ntrt.5>

---

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



This content is licensed under a Creative Commons Attribution-NonCommercial-NoDerivatives 4.0 International License (CC BY-NC-ND 4.0). To view a copy of this license, visit <https://creativecommons.org/licenses/by-nc-nd/4.0/>.



University Press of Colorado is collaborating with JSTOR to digitize, preserve and extend access to *Anthropology without Informants*

JSTOR

PART

I

TOWARD A WORKING THEORY

Each of the three chapters in this section addresses a theoretical issue of considerable importance to archeologists of all persuasions. The first and second distinguish the field of behavioral paleoanthropology from other and very different kinds of archeology. When the pieces were written, archeologists in the United States pretty generally assumed that their kind of prehistoric archeology was the only one. But prehistory is defined as lasting until the peoples who are its subject have begun to produce their own written records. In much of the United States, preliterate people were observed by literate outsiders who left good written descriptions about what they had observed. In other cases, preliterate societies lasted until archeologists began to question living informants about the conditions under which they had previously lived. The anomalous nature of a prehistory with living informants, or recorded by contemporaries, should be obvious, and is the exception rather than the rule for archeologists who study the products of long-vanished societies and kinds of humanity that are often extinct. Some authorities claimed (erroneously) that groups of living hunter-gatherers had been “frozen in time” as living relics, so that all that was needed to fill in the gaps in the archeological record was to supply the missing data by analogy with some living group such as the Australian aborigines.

I go on to develop a model for understanding the past, drawn from Malinowski's concept of "institutions." I use a modification of that model of culture because it provides an inherent reason and a plausible mechanism for change, and it includes the physical materials upon which archeological reasoning must be based. I have replaced Malinowski's concept of the institutional charter with that of the "functional mode," which is one purposive aspect of institutional behavior that is more visible archeologically than are his "charters." (The charters of Malinowski's institutions cannot be directly observed by the archeologist, who only recovers traces of the activities the institution has produced.) Years ago, when I was a student, one of my professors discussed the custom of tipping one's hat to a lady. When I asked if the physical nature of the head covering was important, he said that it was not. But, I asked, what if it were a yarmulke? Malinowski would not have had the difficulty with my question that my professor did.

Malinowski was widely (and wrongly) rejected because of flaws in his reasoning about the "function" of institutions, when it would have been easy enough to revise that reasoning instead of throwing his theory out wholesale. I continue to use a re-statement of Malinowski's theory for the reasons mentioned, and especially because it consistently works when applied to real archeological remains. I'll persist in using it despite its relative antiquity and in spite of all criticism until someone shows me that there is a more practical solution.

It was fashionable when I was a young professor to define culture in a "more modern" way, as "shared ideas in people's heads." I offended some of my colleagues by observing that unless the ideas came out of the heads into some material embodiment—in the form of a social usage, or at least into language, which after all can be measured physically—it simply could not be observed at all.

These observations lead me to another important one. We are sometimes told that archeology should develop its own theoretical stance and its own research methods, and that it will never be a mature discipline until it has done so. I do not believe that for a moment, and I speak as one who has had to develop his own programs for the analysis of prehistoric data on a few occasions. In fact, modern theoretical physics has always relied on the techniques of mathematics, which should be a sufficient contrary argument. I advocate instead searching out and using any technique that works, no matter where or by whom they were invented. It is even my experience that several of the specially devised programs for archeological data analysis do not work as well as some of the more general and readily available commercial programs, such as SYSTAT™ or SPSS™; programs that are designed for exclusive archeological use should only be employed (or designed) where no alternative is available.

My second chapter discusses the prevalent idea that the archeologist can only work by making analogies between the behavior of some living or ethnographically known group. I agree that analogy can be useful when it produces hypotheses that are amenable to testing against the realities of archeological data, but the use of analogy to complete a picture of past human behavior where the humans involved are not modern, and may in fact be assumed to be much different from ourselves, is

simply wrong. Old as this chapter is, its attempt to indicate the fallacy of such reasoning remains valid despite all later claims to the contrary.

The late Christopher Hawkes claimed that it should be relatively easy to reconstruct prehistoric economic systems. “The Fat of the Land” attempts to show how difficult even the reconstruction of prehistoric diet can be when all one has to go on are archeological residues. There are many complications to the discussion of prehistoric diet from the archeological record that Hawkes was apparently unaware of, although some of them should have been obvious. This chapter is just the first part of the original paper, excised from the rest, which discussed the Spanish Paleolithic in terms that would not interest most readers. I have added some concluding observations, indicating that the interpretation of faunal remains from archeological sites is not as straightforward as Hawkes assumed.

