

The preference, in normal cases, for saving a human life over the life of an animal when a choice *has* to be made is a preference based on the characteristics that normal humans have, and not on the mere fact that they are members of our own species [p. 24].

Although Singer accepts evolutionary theory and uses it to buttress his case—only a “religious fanatic,” he believes, can continue to maintain that *Homo sapiens* is separate and distinct from other species—he ignores important differences that exist between various units of evolution. Biologists do not group organisms into species or order species on grounds of similarity. Two forms can be extremely similar and yet be classed as two separate species (sibling species). Conversely, a single species can be made up of extremely dissimilar organisms—

for example, polytypic species like dogs and dimorphic species like the birds of paradise. And species are grouped into higher taxa because of descent, not degrees of similarity. From the biological point of view, the relations that exist between races and between sexes of the same species are different in kind from those that exist between species. If the principles of evolutionary theory are to be taken seriously, there are excellent reasons for us to exhibit a greater moral commitment to a child than to a porpoise, even if that porpoise has a greater capacity for suffering or experiencing enjoyment. If this be speciesism, then make the most of it.

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Economic Archeology

Ancient Civilization and Trade. Papers from a seminar, Santa Fe, N.M., Oct 1973. JEREMY A. SABLOFF and C. C. LAMBERG-KARLOVSKY, Eds. University of New Mexico Press, Albuquerque, 1975. xiv, 486 pp., illus. \$20. School of American Research Advanced Seminar Series.

The conference from which this book stems was called as the result of a renewed concern among archeologists with the subject of “trade.” The study of trade is no longer seen simply as description of patterns of raw substances and finished objects but, rather, as offering a potential key to the nature of the organizational structures lying behind various modes of exchange. The introductory paper by Colin Renfrew lays out ten forms of exchange and comments upon the importance of trade as an information exchange mechanism. The papers offered in the volume are quite diverse, ranging from a discussion of the role played by military force relative to trade in the rise of the state (Malcolm C. Webb) to the possible role of trade in the formation of the secondary civilizations of southeast Asia (Paul Wheatley). Case studies in archeology range from Shang China (K. C. Chang) through Iran (C. C. Lamberg-Karlovsky) and Iraq (Gregory A. Johnson) to Mesoamerica (Jeremy A. Sabloff, David A. Freidel, and William L. Rathje).

Some background to the argument between economists and economic anthropologists over what should be taken as

the paradigm of “economic” behavior for ancient and nonliterate societies is given by George Dalton in the course of an exposition on “Karl Polanyi’s analysis of long-distance trade and his wider paradigm.” The main concern of this discussion is whether or not Polanyi’s theory (which illustrates “how the organization and functions of monetary objects, markets, and foreign trade systematically vary with the type of domestic economy and society in which they appear” and which attempts to “contrive a paradigm for the socioeconomic organization of aboriginal bands and tribes and for the internal and external sectors of early state systems”) is more informative in the analysis of its subject than formal (or Marxian) economics. Dalton gives a useful outline of the general disagreement between economists and anthropologists over the applicability of the concepts and terminology derived from formal economics to preindustrial societies. The interested reader may wish to fill in the background further by looking at *Themes in Economic Anthropology* (edited by Raymond Firth) and at “Economic Anthropology” by R. Salisbury (in *Annual Review of Anthropology*, vol. 2, edited by B. Siegel *et al.*).

The archeological “case studies” presented in the volume may be examined in relation to how they are articulated to this broader discussion in economic anthropology and to how the authors justify the borrowing of formal economic terms to describe preindustrial (archeo-

logically known) societies. Upon reading these papers it becomes apparent that most of them simply sidestep the issue and proceed with their analysis, using varying terms with widely varying definitions and having almost no shared set of basic concepts. One wonders upon what body of discussion and study the economic ideas used in the case studies rest. A comparison of the reference lists indicates only that several authors have read Sahlins or Polanyi, or both—little else outside of the archeological speculations of their closest associates is cited. As inventive and interesting as some of the archeological presentations are, they speak largely to the concerns of other fieldworkers in search of research strategies rather than to the broader theoretical issues underlying the study of trade as an aspect of economic anthropology. This is a great disappointment, for, as Dalton points out in his conclusion, primitive and early economies are subjects very much in need of theoretical formulation. “Intelligent men still disagree utterly in their interpretations of the basic functioning of economies remote from our own.” Thus we need careful, step-by-step analyses of ancient trade systems starting with hard data and using clearly defined concepts with an explicit understanding of the derivation, connotations, and limitations of the terms used.

Four papers will serve to illustrate the positions taken in the conference in relation to the broader subject at hand. Chang, in “Ancient trade as economics or as ecology,” takes the side of Polanyi, asserting that “trade can be studied only in the total context of the distribution of raw and processed natural resources within a societal framework.” He objects to an ecosystem approach, which often seems “to assume that the population as a whole, or its procuring segments, act in concert, according to survival needs. The homogeneity of the population in terms of its survival interest as a whole is also implicitly assumed.” He points out, “Man often interacts with his environment in peculiar ways that are sometimes detrimental to his own interests or to the interests of some members of his own group.” Thus the structure of the human group is an essential part of any solution to the interpretative problem posed by the identification and distribution of natural resources and must be described. A problem arises in attributing significance to the distribution of products in space in the absence of any evidence concerning their mode of movement (tax, tribute, raid, purchase, gift, or whatever), since some modes involve

reciprocity and some do not. Chang concludes that "archaeologically, one can seldom if ever distinguish among these diverse situations, and consequently the quality of reciprocity is hard to characterize in terms of individual units of material goods." Another difficulty, he says, is that the amount of increase in the value of objects is "relative to factors beyond precise calculation in archaeology. Value can also be measured in services as well as goods. Neither labor nor services in the empirical sense leave any archaeological imprint." Thus "trade" must be dealt with not in isolation but as part of the total economic behavior pattern of an ancient society.

Rathje's "Last tango in Mayapan" does the opposite of what Chang advocates. Rathje combines systems analysis terminology with a concept of "cost-control" to analyze changes in Maya civilization from the Classic to Late Post-Classic periods as seen at Tikal and Mayapan respectively. In doing so he isolates material culture from the rest of the data on the society and treats its development (a "trajectory") as a separate element for study. For this purpose he uses a concept of "cost-control," defined as a measurement of change within an ongoing system. The concept includes the hypothesis that a decrease in the investment of labor and material in production and distribution should occur concomitantly with an increase in both the number of products and their range of distribution through time (in short, research and development followed by cost-controlled mass replication and distribution of commodities). Rathje's aim is to develop ideas leading to a dynamic model of change using quantified data.

For this purpose he uses architecture, caches, and pottery as "products." The tacit assumption is made that Classic and Late Post-Classic Maya social, political, and economic organizations form a single continuous "system." In order to measure the extent of "cost-control" (that is, change) it is necessary to measure for both periods and for each "product" (i) the amount of labor invested, (ii) the quantity of raw materials required, (iii) the number of "products," or some subdivision thereof, produced, and (iv) the spatial distribution of the product or products. According to the hypothesis the development should proceed through three stages: (i) concentration of resources in craft production with a limited number of products and a limited social or spatial distribution; (ii) mass production and distribution of standardized sets of products (with the implication that standardized sets of objects themselves

produce standardized behavior); and (iii) an eventual increase in the variety of products in response to local demand. As the discussion in the paper proceeds the use of the term "cost-control" in its initial definition as referring to an overall trend in time appears to be replaced by its use to refer to specific actions; thus, Rathje speaks of "cost-control devices" and "cost-control of resource procurement," which sound more like cost-cutting activities than the long-term measurement indicated in the main definition.

The attempt to test this approach is filled with difficulty, as becomes readily apparent in the use of architecture for the purpose. In terms of human labor recycled stone in the later period is seen as less costly than cut stone used in the earlier period. The decline in the gross amount of cut stone used is seen as a quantitative measure of the degree of cost-control of the basic resource between the two periods. The large temples of the Tikal acropolis are compared to the dispersed shrines at Mayapan as a measure of spatial dispersal of the "product." The comparative scale of the two sets of architectural data is illustrated by maps covering equal areas from the two sites. The difficulty with this procedure is that the overall scale of the settlement is not taken into account. The map of Mayapan includes the whole site including the surrounding wall. The map of Tikal contains the ceremonial center only out of the 16 square kilometers mapped. If dispersal is to be measured, surely the placement of structures should be relative to the settlement taken as a whole. Furthermore, it seems inequitable to compare temples from one period with private shrines from another when each type of structure occurs in both.

Another difficulty lies in the time span being compared. The acropolis at Tikal represents at least 600 years of cumulative building activity, whereas Mayapan represents only about 200 years. Finally, there is the problem of how to measure the total labor put into later structures, which involved hauling rubble, making stucco, cutting wood, and thatching. If architecture is the "product" then all the labor going into both finished products ought to be taken into account for a comparison and not only one activity such as stone cutting. The author concludes by stating that the trouble with anthropological models is that they "often make little or no effort to identify regular patterns in changes in the form and distribution of material culture or to determine the role these critical changes in energy and information transformation played in cultural development," a rather surprising state-

ment in view of the history of archeology and anthropology over the past 75 years in Europe and America. In any event we can certainly agree with him that "whether the resource management cost-control trajectory proposed here withstands quantitative testing is not important. Beginning a quantitative analysis of material attributes of long-term sociomaterial systems is."

In his paper "Third millennium exchange and production" Lamberg-Karlovsky attempts to challenge the paradigm of the inapplicability of formal economic concepts to preindustrial society by arguing against the commonly accepted notion that no profit, price-fixing, wholesaling, supply-and-demand mechanisms, or even private ownership of land for surplus production existed in the temple-palace society of third-millennium Mesopotamia. He argues that a "market network" was in existence by 3000 B.C. According to his definition a market network is the process by which the mechanism regulating an exchange relationship channels commodities and services from an area of high supply to one of high demand. A market network thus represents not a marketplace but a conceptualization of a state of affairs on a synchronic level. The paper then concerns itself with establishing a source area and production center, Tepe Yahya in southern Iran, with a large supply of chlorite artifacts, and an area of demand, the Sumerian city states. The goal is to construct an explanatory model appropriate to the data at hand.

A synchronic study of this type demands, according to the author, that the data be described quantitatively, contextually (in the sense of archeological associations), and technologically; that the data be from more than one locality; and that the data compared be contemporary. The review of the Yahya evidence (based on excavations directed by the author and laboratory analysis carried out by Philip Kohl for his doctoral dissertation) establishes that Yahya was near a source of chlorite that was used for the manufacture of carved objects that occur in various Mesopotamian sites. Since no similar objects are known from sites around Yahya this production seems to have been aimed only toward the Mesopotamian settlements. (The role played by the intervening area of Elam is not discussed.) Since differing quantities of chlorite artifacts appear at various sites in Mesopotamia it is suggested that there was some trading competition between the cities. The question then is: what was the institutional organization for this trade pattern? The author rejects

the suggestion (made by Renfrew) that Yahya had any political connection with the Elamites or anyone else, since there is no archeological evidence to support it. How such connection or lack of it would be manifested archeologically is not discussed. In view of the archeological evidence at Kültepe in Anatolia, which is equally uninformative but where written documents occur showing political arrangements, this point should be dealt with. In any event the fullest development of this production occurs between 2900 and 2600 B.C. according to the excavator and is thus too late to be directly relevant to the presence of proto-Elamite tablets at the site—evidence that might have political meaning. This is the period of Early Dynastic I and II in Sumer, a time of beginning contacts and conflicts with the dynasty of Awan in Elam. The author interprets the general state of affairs (that is, a pattern of production and consumption of a single product in different areas) as constituting evidence for the existence of a market network, in his terms. The further assertion is made that the “demand” from the Sumerian cities was communicated to Yahya by entrepreneurs. “Whatever profits existed in the transactions were principally in the hands of middlemen traders and the exploitive elite of Mesopotamia.” With this statement are ruled out, with no further explanation or justification, all other trade models as discussed by Renfrew, Dalton, and Polanyi. The statement appears as arbitrary as those the author criticizes at the beginning of his article. This is unfortunate, for the matter of wide and diverse patterns of trade (as opposed to local exchange) is important and needs to be dealt with if we are ever to be able to define the kinds of information exchange occurring between cultural areas. By choosing the profit motive rather than some other factor Lamberg-Karlovsky brings us face to face again with the problem of the paradigm for economic anthropology as outlined by Dalton and the validity of transferred concepts in the context of ancient society.

The one case study among the archeologists that admits openly to the problem posed by the industrial-preindustrial paradigm is Johnson’s “Locational analysis and the investigation of Uruk local exchange systems.” In this paper Johnson attempts to apply locational analysis concepts to the study of exchange and settlement patterns in the Uruk period (3500 to 3150 B.C.) in southern Iraq and southwestern Iran. He recognizes that these concepts rest upon the assumption of economic maximization—“that deci-

sions affecting the economic position of an individual or group are always made in order to maximize that position.” Since other assumptions are possible (the maximization of political power, for example), the applicability of this maxim to non-money-market economies has been under debate for some time. To his credit Johnson recognizes this difficulty and attempts to deal with it by restating the assumption in such a way that it is no longer restricted to the modern market situation. He states that conditions favoring initial centralization are probably highly variable but that once centralization has begun “subsequent development of a central place hierarchy should proceed (1) to the extent that least effort considerations influence the spatial organization of production and distribution of goods and services, and (2) to the extent that the operation of other variables does not intervene in this process.” In other words, since effort minimization may appear in varying degrees relative to economic maximization in the context of market, redistributional, or mixed economies the application of the model need no longer be restricted to the modern market situation. This modification of Walter Christaller’s central place concept (*Central Places in Southern Germany*, 1966) makes the prediction of settlement distribution patterns impossible—a fact that affects the conclusions that may be drawn from an observed distribution pattern. Johnson has a healthy skepticism concerning the potentialities of locational analysis, pointing out that it asks many questions but answers few. Its main value lies in its ability to indicate unsuspected regularities in settlement data and to generate further hypotheses. The attempted application of the method to the Uruk materials is not entirely convincing but is a worthwhile effort. Its limitations are commented upon by Robert Adams in his concluding remarks. A four-level hierarchy of site size is established with a distribution pattern appropriate to a central place model. These data are interpreted as indicating the existence of a local exchange system, which is seen as a major factor in the formation of the pattern itself. The results are promising enough to encourage further study along these lines, with the hope that eventual modification of the basic assumptions in the light of the data may help to create a more acceptable paradigm for the application of the method.

Adams points out that the purpose of this volume is to present, and to encourage, a variety of directions of exploratory thinking by the various authors.

With this goal I can agree. On the other hand, when he says that it is not the time to question the authors about their unexamined premises and contradictory assumptions I do not agree. It is exactly those aspects of their proposals that they should be encouraged to examine further, so that their conceptualization of problem and method will be logically sound, precise, and above all explicit. It is to be hoped (in part as a result of this conference) that they will address themselves to some of the basic problems already under discussion in economic anthropology (to say nothing of making use of, and respecting, well-established definitions in this area of study). In this way a much greater range of communication will be opened up, incorporating both the evidence and orientations of the field archeologist and the broader anthropological study of socioeconomic systems.

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Old World Prehistory

The Neolithic of the Near East. JAMES MELLAART. Scribner, New York, 1976. 300 pp., illus. Cloth, \$14.95; paper, \$4.95. *The World of Archaeology*.

The publication of archeological research is a notoriously slow process. In many cases the excavation of a site or the surface survey of a region takes place over a number of years, and there is often a significant time lag (a decade or more is not unusual) between the completion of field research and the appearance of a full report. During the interim, scholars must rely on preliminary publications that briefly report the most significant or unusual findings and provide tentative interpretations of the data. Most of the archeological research that has contributed to our knowledge of the origin and spread of food production and the development of sedentary communities in the Near East has been conducted since 1950, and few final reports have appeared. In this book the British prehistorian James Mellaart attempts “to bring together in a coherent account” the fragmentary and widely scattered information on the subject (p. 7). The book focuses on the period from approximately 15,000 to 4000 B.C. and covers the area from the Balkans to Turkmenistan; a brief treatment of the transition from hunting and gathering to pastoralism and cereal cultivation in Egypt and the Sahara is also included.