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CONTENTS

INTRODUCTION 1
Dale Maul

CURRENT ISSUES IN ARCHAEOLOGICAL RESOURCE MANAGEMENT 3
James H. Gramann

WHO WERE THE PLAINS INDIAN BERDACHES? 41
Donald J. Blakeslee

CULTURAL DIFFUSION: A BRIEF OVERVIEW OF POPULAR EXTREMES, SOME CONCEPTIONS AND MISCONCEPTIONS 66
Ben Urish

HUMAN EVOLUTION: AN ALTERNATE MODEL OF HOMINID SOCIAL DEVELOPMENT 78
William M. Metz
INTRODUCTION

The series of papers presented in this volume are thought provoking and show originality. It is hoped that the reader will enjoy the authors' ideas about their particular aspect of the field of anthropology.

The first paper in the series of articles is entitled "Current Issues in Archaeological Resource Management". This study deals with the evolution, history, and coordination of federal and state conservation laws and how they pertain to archaeological sites. One concept that is dealt with in this article is, what are the determining factors in making one archaeological site more significant than another. Mr. Gramann has done considerable research on the formulations and intricacies of the conservation laws.

The second paper in our series is by Dr. Donald Blakeslee, entitled "Who Were the Plains Indian Berdaches?" Blakeslee examines the standard anthropological view toward berdaches. The problems of Western ideas are brought to the forefront when examining this phenomena of the Plains. He examines the old concepts against the weight of historic and ethnographic literature. This paper brings forward that the concept of abnormal sexual deviancy cannot be applied to the majority of cases in which the Plains groups had berdaches.

The third paper in our series, hopefully will bring a smile and chuckle from the reader. The title of Mr. Urish's paper is "Cultural Diffusion: A Brief Overview of Popular Extremes, Some Conceptions and Misconceptions." This satirical
look at "unqualified extremism" and "qualified extremism" is prevalent in today's multitudes of popular thought on the origins of the human race. Mr. Urish examines the ideas and methods of these pioneer scientists, who include Ignatius Donnelly, Erich von Daniken and Thor Heyerdahl. Many societies and former cultures, like Mu, Atlantis, and even extraterrestrial trials are dealt with by the author.

The fourth and final paper in our series is titled, "Human Evolution: An Alternate Model of Hominid Social Development." This paper takes a serious look at hominid evolution. Mr. Metz, instead of using the baboon analogy, offers us another, that of the Patas monkey. He attempts to show that the adaptive strategy of the Patas monkey can be used to explain hominid social evolution at least as well as the baboon analogy. Mr. Metz has brought forth a new and alternate analogy that will stimulate the interests of many readers.

- Editors Comment -

One way of helping to achieve academic excellence for the Journal is receiving comments from our subscribers. In the forthcoming Journal we are hoping to print comments about papers. We cannot implement this tool of education unless we receive comments from our subscribers. We urge comments of these or other papers in the future. We cannot make this work without you.

D. E. Maul
CURRENT ISSUES IN ARCHAEOLOGICAL RESOURCE MANAGEMENT

by

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The focus of this paper is on the issues and controversies that have arisen in connection with the management of archaeological resources at both the federal and state levels. Although archaeological resource management can be considered as falling under the broader umbrella of historic preservation, it has succeeded (if that is the proper word) in generating a series of exasperating problems unique unto itself. After a brief introduction to the science of prehistory and the need for management, these problems are discussed within the context of three important management tools: legislation, education, and planning. This division is convenient for pedagogical purposes, however it should not mask the fact that the concerns of archaeological resource management in the real world are highly interrelated. It is hoped that the presentation in this paper sheds some light on their true complexity.
According to the most liberal estimates, the genus "Homo", of which modern man is the most recent species, has existed on this planet for perhaps 1.5 million years. As vast as this time period may seem to us, it still represents only a tiny fraction of the total lifespan of the earth (about three hundredths of one percent). Even so, within this tiny fraction of time the written history of "Homo sapien" is shorter still, ranging from its beginnings about four thousand years ago in parts of the Far East to less than two hundred years ago in extensive areas of Africa and the western hemisphere. Therefore, there is relatively little in terms of the breadth of time covered that studies of written history can tell us about the evolution of Cultural Man. For greater insights into this problem we must turn to the discipline of "prehistory".

Prehistory is the science that seeks to explain changes and stability in patterns of human behavior over time through the systematic study of cultural remains. It is useful to draw a distinction between this study and the body of tools and methods employed by the prehistorian in data collection. This body of techniques we call archaeology, and note that it has applications in the study of written history and other pursuits as well. The terms "prehistory" and "archaeology" are often used interchangeably, however, and this paper will provide no exception to that custom. At the beginning, though, it should be made clear that the two terms are not synonymous,
and that archaeologists are by no means confined to the study of prehistory.

The subject matter of prehistory is human culture, which is itself defined in a wide variety of ways. The definition employed in this paper is undoubtedly one of the simplest ever devised. **Culture** is everything that people think, have, and do. An **archaeological resource**, then, is a product of these activities that conveys information about what people in the past thought, had, or did. Any archaeological resource has two essential attributes. The first is modification by man; the second, contextual information. By itself, neither attribute is of much value to the prehistorian. A projectile point of a particular size and shape, for instance, reveals very little about the people who made it. Most of this knowledge is derived from information concerning other artifacts associated with the point in a particular site, and the relationship of that site to other sites in horizontal space or vertical strata. These attributes comprise the context, or provenience, of the point and also include such environmental characteristics as the biotic community and landforms associated with its manufacture and use.

Essentially, it is the site of discrete assemblages of artifacts that forms the basic unit of analysis in the study of prehistory. Prehistory is the analysis of sites as manifestations of social activity, together with their temporal and environmental interrelationships. Thus, in Illinois, archaeologists study Paleo-Indian sites, Archaic sites, Woodland sites,
and Mississippian sites among others. At a more complex level, groups of sites linked by some functional interdependence, such as the so-called Hopewell Interaction Sphere, are also analyzed. The information contained within these sites forms the basis for theories about human development and environmental adaptation. The goal of "archaeological resource management" is to preserve and manage this essential data base in order to provide the optimum long-term benefits to both the science of prehistory and to society as a whole. This strategy often involves issues and trade-offs that are not easily decided. At times the perceived benefits to society and to prehistory are mutually exclusive. A choice must be made between one or the other, and it is rare when both can be maximized in the same decision. This paper examines the more critical issues that must be faced by those involved in archaeological resource management.

THE NEED FOR MANAGEMENT

Three overriding characteristics of archaeological sites must be considered by resource managers in light of the continuing rapid pace of land-use alteration in this country through agricultural growth, industrial development, and residential and commercial expansion.

First, archaeological sites comprise a finite resource; there are not an unlimited number of them.

Second, sites are perishable. They are extremely vulnerable to almost any type of ground-disturbing activity.
Third, archaeological sites are nonrenewable. Once they are destroyed, either through ground-disturbing activities, vandalism, or even professional excavation, they are gone forever; they cannot be replaced.

Although estimates vary, it is thought that within the boundaries of Illinois there were once something over a million archaeological sites (Struever and Farnsworth, 1977). Today, approximately 30,000 have been identified and recorded by the Illinois Archaeological Survey (Hild, 1977). Based upon extrapolations from known site densities, there may be anywhere from 360,000 to 735,000 sites remaining to be discovered. In other words, during the last two hundred years there has been an estimated loss of sites ranging from a quarter of a million to 625,000 in the state of Illinois alone. Needless to say, if this pace were to continue unabated for another two hundred years we would be dealing with an impoverished resource indeed. It is this realization that has led to the increasingly prominent position that cultural resource management programs of all kinds have assumed in federal and state land management agencies, and in the general historic preservation movement as a whole.

Management Tools

Those concerned with the preservation and wise use of archaeological resources have three principle management tools at their disposal: legislation, education, and planning. Each of these is discussed in turn in the following sections of this paper.
To a great extent, the history of archaeological resource management in the United States can be traced through the history of its legislation. At the federal level, this history reaches back to the year 1889. In that year, a law was enacted in the U.S. Congress to preserve Casa Grande, the last remaining "big house" of the prehistoric Hohokam culture in southern Arizona. Continued concern with vandalism and "pot hunting" in the ruins of the Southwest led to passage of the American Antiquities Act in 1906. To this day, the Antiquities Act is the only piece of national legislation which provides penalties for the destruction or unauthorized collection of artifacts and other "objects of antiquity" on federal lands. Presently, its constitutionality is in doubt due to the presumed vagueness in the definition of an object of antiquity. This has led recently to a movement to rewrite the law so that convictions under it can be more easily obtained (Collins and Green, 1978).

The Historic Sites Act of 1935 was the only other piece of federal legislation passed prior to World War II that had major significance for the preservation of archaeological resources. Under it the National Historic Landmarks Program was created authorizing the Secretary of the Interior to acquire, or assist in preserving through cooperative agreements, historic sites of national significance. The Cahokia Mounds complex near Collinsville, Illinois, is an example of an archaeological site which has been designated a National Historic Landmark.
Although the Historic Sites Act provided no protection against the destruction of sites accorded Landmark status, it did establish a national policy of preservation which was to be further strengthened in the ensuing decades.

Another important consequence of the Historic Sites Act was the creation within the National Park Service of the Interagency Archaeological Salvage Program. This unit, established following the end of World War II, was given primary responsibility for overseeing the recovery of archaeological resources threatened by a stepped-up national program of dam construction and waterways development. It was under the auspices of this agency, buttressed by the subsequent Reservoir Salvage Act (1960), that the derogatorily labeled pursuit of "salvage archaeology" experienced its greatest boom. Massive amounts of earth were moved as a result of this program in a feverish attempt to identify and recover prehistoric sites threatened by construction and reservoir impoundment. The Federal-Aid Highways Acts of 1956 and 1958 authorized similar salvage efforts during highway construction.

It is generally agreed that the archaeology conducted during the salvage boom of the 1950's and 1960's was, with few exceptions, of low quality (Schiffer and Gumerman, 1977). As the saying goes, sites were "dug like potatoes." Storehouses of artifacts accumulated and basic temporal sequences were established, but the overall record of analysis and publication of results was poor. In part, this was due to a chronic lack of funding for any activity beyond simple site
surveys and excavations, and in part to the dilution of professional talent occasioned by a demand for qualified personnel which outstripped the supply. During this period several archaeologists were hired for administrative positions in federal agencies with nothing more in the way of academic qualifications than a baccalaureate degree and a few weeks of field experience (Interagency Archeological Services Division, 1976).

This situation has improved measurably in the last decade to the extent that many reputable archaeologists no longer look with disdain on "conservation archaeology." Indeed, they can hardly afford to ignore it. Today, federal agencies such as the U. S. Forest Service, and Bureau of Land Management, and the Army Corps of Engineers, in addition to state and local governments and private industry, provide the major sources of funds for archaeological research (Schiffer and Gumerman, 1977). One reason for this state of affairs has been the enactment of several pieces of important legislation at the federal level since 1966.

The National Historic Preservation Act of 1966 was the first of these measures to be passed. It has been important to archaeological resource management in two ways. A provision authorizing federal grants-in-aid to states for historical surveys has resulted in the discovery of thousands of additional archaeological sites. In Illinois, this inventory has been coordinated by the Illinois Archaeological Survey with headquarters at the Urbana-Champaign campus of the
University of Illinois. In addition, the act requires all federal agencies to take into account the effect of their undertakings upon properties listed in an expanded National Register of Historic Places (first created by the 1935 Historic Sites Act). To monitor this process, an Advisory Council on Historic Preservation was established with commenting power on all federal actions which would adversely impact a listed site. The National Historic Preservation Act has created as many bureaucratic headaches as it has benefits, however. These are discussed in later sections of this paper.

The National Environmental Policy Act of 1969 (NEPA) and Executive Order 11593 (1971) have had important impacts upon the development of new directions in archaeological resource management, as well as in the expansion of previously existing programs. The enactment of NEPA reemphasized the commitment of the federal government to the preservation of prehistoric and historic resources. Section 102(c) of this act requires that an assessment of impacts be made for major federal projects significantly affecting the quality of the environment. This means that first of all the baseline state of the environment (including archaeological resources) must be determined, and secondly, that probable changes in this baseline state, together with possible mitigation procedures, must be identified. For major federal projects, at least, archaeologists have suddenly found themselves in the business of impact forecasting, heretofore a totally foreign concern.

Under Executive Order 11593 (Protection and Enhancement
of the Cultural Environment) federal agencies were ordered, somewhat unrealistically, to complete a total inventory of the cultural resources under their jurisdiction by July, 1973, and to nominate all eligible properties to the National Register of Historic Places. In addition, agencies were to refrain from impairing eligible properties under their control whether or not they were listed on the Register. This latter directive established the so-called principle of "interim protection." When combined with the very broad Advisory Council definition of "eligible" archaeological sites, it has, in effect, given the Council the right to comment on the fate of every archaeological site in the country affected by a federally funded or licensed action. As indicated in the following sections on Education and Planning, this situation has created a great deal of resentment toward archaeological preservation among state and local officials. It is one of the major problems that must be solved if a workable program of archaeological resource management is to be developed.

A final piece of national legislation of obvious importance to archaeology is the Archaeological and Historic Preservation Act of 1974. This is an expansion of the 1960 Reservoir Salvage Act and empowers federal agencies to appropriate up to one percent of a total project budget for the recovery or protection of threatened historic and archaeological resources. Although this one percent provision has pumped additional funds into archaeological resource management, it is still inadequate to fully comply with the new responsibilities delegated to the
affected agencies. Some agencies have been forced to divert these funds to the comprehensive inventories required by Executive Order 11593 (Banks, 1977). This is technically illegal, but has been made necessary by the failure of the Order to authorize monies for the inventories. The result is less funding available for actual salvage and protection.

A benefit directly attributable to the Archaeological and Historic Preservation Act has been the broadening of the mission of the old Interagency Archaeological Salvage Program. Previously, this organization had functioned as a coordinating unit for emergency archaeological salvage projects. This mission restricted its activities to such immediate problems as those occasioned by reservoir impoundment, highway construction, and railroad relocations. Significant, but gradual impacts on archaeological sites, such as those caused by the Soil Conservation Service's land leveling projects in the lower Mississippi valley, could not be mitigated by existing salvage policies. In recognition of this problem, the 1974 act broadened the responsibilities of the Secretary of the Interior to include coordination of archaeological mitigation efforts among all federal agencies. The Interagency Archaeological Services Division evolved from the old salvage program as the unit primarily responsible for this effort. It has been active in promoting and coordinating innovative programs at all stages of the archaeological resource management process.
State Laws

No discussion of legislation as an archaeological resource management tool would be complete without at least a brief look at state laws. Federal legislation, of course, has an important impact in every state, since it generally applies not only to federally-owned lands, but to federally funded and licensed projects as well. Therefore, agencies such as the Department of Transportation, the Environmental Protection Agency, and the Soil Conservation Service, who are not empowered to own land, nevertheless fund a great many archaeological resource management activities through various grant and licensing programs. In addition to this, however, several states have enacted their own versions of national laws to cover those situations where the latter do not apply. Three basic types of state laws will be considered here.

State Registers of Historic Places. Under the provisions of the National Historic Preservation Act of 1966 all fifty states were required to adopt their own historic preservation plans in order to qualify for National Park Service grants-in-aid. In many states, including Illinois, this has included the creation of State Registers of Historic Places. The Illinois Register differs from the National Register in two important ways (Sculle, 1977).

First, it does not extend interim protection to sites which are eligible, but not yet listed on the Register. Second, the protection it does afford to listed sites is much more powerful
than that provided by the National Register. For this very reason, however, only a small number of highly significant archaeological sites are likely to be nominated to the State Register. Archaeological excavation is, by its nature, a destructive process. It is possible that some future legitimate excavation of a listed site might be precluded, or at least seriously delayed, by listing on the Illinois Register due to the fact that rather strong safeguards against any type of destruction have been written into the law.

*State Environmental Policy Acts.* A second class of state law modeled after federal legislation is the State Environmental Policy Act. Illinois does not have such a law, but other states, such as California, do. Generally speaking, these laws require that projects funded or licensed by state agencies be evaluated in terms of their environmental impacts, and that whenever possible adverse or irreversible impacts be mitigated. As in the national law, the term "environment" is often interpreted to include both the cultural and natural surroundings. Thus, archaeological sites clearly come under the purview of many state environmental policy acts.

*State Antiquities Acts.* Finally, there are those laws, patterned after the 1906 Antiquities Act, designed to protect archaeological sites on state (and sometimes private) lands from unauthorized excavation or collection. These laws vary widely in the penalties they prescribe, with some, such as that enacted in the state of Washington, assessing no penalties at all,
relying instead on voluntary compliance. Illinois has no such law in force. As a matter of interest it should be pointed out that several American Indian groups, including the Navajo and Hopi, do have ordinances which protect archaeological resources on tribal lands.

Preservation law has been dealt with at some length in this section, not only because it is an important management tool, but because it provides a convenient vehicle for presenting an outline of the growth of archaeological resource management in the United States, along with some of its current administrative problems. The following two sections on Education and Planning take a closer look at the scene today, and elaborate on some of the issues already introduced.

EDUCATION

By education is meant not only an attempt to instill an appreciation for the scientific value of archaeological resources in the public at large, but also a similar effort directed at decision-makers and administrators responsible for large-scale projects affecting the ground's surface. In Illinois, public education has been concentrated in such institutions as the Department of Conservation's Division of Historic Sites and in the Illinois State Museum. The education of decision-makers and administrators, however, is properly the responsibility of professional archaeologists. These are the people that archaeologists work with "on the ground" during the day-to-day conduct
of management activities, and it is these people who have the main responsibility for assuring compliance with preservation law. Unfortunately, history has shown the track record of archaeologists to be something less than sterling in this regard. The primary educational thrust has been aimed at the top of the decision-making pyramid, principally at the U. S. Congress. Ironically, the success of this effort, as witnessed by the passage of much significant preservation legislation during the last two decades, has been the major contributor to the growing dissatisfaction with archaeological resource management programs among lower-echelon officials. Referring to the lack of communication between professional archaeologists and agency officials, the Historic Preservation Officer for the state of Iowa has made the following comment (Anderson, 1977:50):

Archaeologists have contributed to the problem of dealing with archaeological sites by not educating the public, the SHPO, and the Federal representative, to convince them that archaeological resources are worth the time and cost of protecting them...There is no denying that there is an increasing level of hostility and resentment among local officials, Federal agency representatives, and State agencies, at the need for, and high cost of, archaeological investigations associated with almost every application for Federal funds. If the Archaeological profession does not act to provide adequate justification for their research, I fear that archaeology will be the first element which will be removed from the protection of preservation law and the Advisory Council process.

In a similar vein, an official of the Environmental Agency notes that:
...such trade associations as the American Concrete Pipe Association...are interested in whether archaeological preservation requirements are slowing down municipal grants...This is being combined with a general disbelief on the part of municipal officials that Federal agencies are actually requiring them to fund, using 25 percent of their monies in our case, archaeological surveys, and in some cases to stop projects to do archaeological salvage (Olson, 1977:46).

In the states of Idaho and Virginia officials have done more than just complain. Legislation has been proposed in both these states that would restrict nominations to the National Register of Historic Places, and in Georgia there is an agreement between the State Historic Preservation Officer and the state department of transportation which calls for reviewing archaeological preservation for adverse impacts on highway projects, rather than the usual reverse procedure (Crecco, 1977). Finally, an official with the U.S. Army Corps of Engineers, noting that many "insignificant" archaeological sites have been prematurely placed on the National Register, points to the resulting bad publicity as creating, "a loss of credibility for the archaeological community in general, for the Advisory Council, and the National Park Service (Banks, 1977:14).

From the point of view of the individuals cited it is apparent that professional archaeologists have a great deal of educational work yet to do. However, this situation is not entirely the fault of the archaeological community. Archaeological resource management has experienced a rapid period of expansion.
In any such situation, there will inevitably be growing pains as both agencies and archaeologists work to refine their procedures and adjust to new responsibilities. Archaeologists are currently debating among themselves the relative merits of the inductive "old" archaeology and the deductive "new" archaeology, the latter having appeared on the scene only within the last fifteen to twenty years (Dumond, 1977). If there is disagreement within the archaeological profession itself as to the fundamental goals of the discipline, it is not surprising that the face presented to the outside world should be somewhat inconsistent. The archaeological preservation movement differs from the rest of historic preservation in that it has no centralized educational organization equivalent to the National Trust for Historic Preservation. Perhaps it is time to consider such an organization for archaeology, or perhaps the National Trust should expand its scope to more fully embrace the preservation and management of prehistoric resources.

PLANNING

Following legislation and education, the third major tool available to archaeological resource managers is planning. In this regard, archaeology is similar to any other endeavor where planning is an important consideration. The basic questions to be answered are the same, although unique issues surround them. These questions include:
What Do We Want: Politics in Archaeology

Determining what it is we want from archaeology brings us to the crux of the whole issue of archaeology and society. The noted British prehistorian, Grahame Clark, states the problem succinctly (1969:251):

...the question has to be faced whether the study of prehistory has any relevance to modern society, or, more specifically, whether it is sufficiently relevant to warrant the diversion of funds and of potentially productive men, skill, materials, and land. Does prehistory really mean enough to us today to support such large claims on social resources?

A comparison of this passage with the quotation from Anderson on page 18 reveals that little has changed in the decade or so since Clark published his book; and this, despite the increasing amount of legislation and public monies being committed to archaeological resource management. Clark resolves his personal dilemma by pointing out the world-wide integrative potential an expanded knowledge of human history may have. His argument is not unlike that heard when the first photographs of "Spaceship Earth" were published following the Apollo VIII moon mission. Prehistory may bequeath to us a broadened social perspective, transcending petty concerns and enriching our experience so that we may, "live more
Of course, archaeologists generally consider themselves to be social scientists as well as humanitarians, and there is little doubt that the field of prehistory has contributed much to our knowledge of the processes involved in cultural change. Archaeological data are also of use in other disciplines, including climatology, geology, and land-use planning. However, the basic question still remains: when a choice has to be made between preserving an archaeological site or constructing a waste treatment plant, on what basis do we arrive at a decision?

As with all questions of this type, the answer is ultimately a political one. In a democratic society, political solutions to problems usually entail some degree of compromise, and archaeological resource management is no exception to this rule. In answering the questions, "what do we keep?" and, "how do we do it?" the extent of this compromise is delineated. However, even before this can be done, we must first discover what it is we have in terms of archaeological resources. This is accomplished during the inventory stage of the planning process. As we will see in the following discussion, though, even the inventory process is the subject of some yet-to-be-resolved controversies.

What Do We Have: The Archaeological Survey

Executive Order 11593, Section 2(a), ordered that federal
land management agencies should complete a 100 percent inventory of all cultural resources under their jurisdiction by July 1, 1973. This deadline has not been met, nor, in most cases, has the inventory task even approached the 100 percent goal established by the Executive Order. As an example of the adjustments which have had to be made to accommodate reality, the U.S. Forest Service is currently operating under a 1990 service-wide deadline for completed cultural resource inventories (Wildesen, 1977).

Although a 100 percent inventory of cultural resources is certainly a laudable goal, whatever the deadline, the obscured evidence of many archaeological sites makes this objective difficult, if not impossible, to achieve. It can be argued, in fact, that Executive Order 11593 is actually detrimental to sound resource management planning.

Planning is a multi-stage process, involving a gradual weeding out of various alternatives. It is only in the final stages of this process, when a single course of action has been decided upon and funded, that a complete survey should be undertaken for impact assessment and mitigation purposes. Placing the comprehensive survey at this stage of the planning process is entirely consistent with the guidelines of the Council on Environmental Quality for compliance with the National Environmental Policy Act (Scovill, et al., 1977). However, it conflicts with Executive Order 11593, and also with the guidelines of the Advisory Council on Historic Preservation for implementing the National Historic Preservation
Act. This latter directs that comprehensive surveys should be carried out at, "the earliest stages of planning or consideration of a proposed undertaking" for purposes of determining the eligibility of cultural resources for inclusion upon the National Register (36 CFR 800.4). The problems such a directive creates for land management agencies in terms of archaeological resources are summarized in the following statement by a Bureau of Reclamation archaeologist (Weakly, 1977:17):

When we start out under something called Multiple Objective Planning Procedures, we may, for example, be looking at seven to fifteen alternative locations for a reservoir. Are we required, is it necessary, is it reasonable to do an intensive 100 percent survey of all those alternatives, when you know that only one of them will probably be picked if it is built?...
If you have $150,000 - $2,000,000 to do an entire environmental impact statement, based on your general investigation, you cannot spend it all on archaeology; that just can't be done.

There is little argument that a detailed "pedestrian survey" of an area is necessary for the final stages of a project plan. A U.S. Forest Service rule of thumb concerning the entire inventory process reflects this orientation (Wildesen, 1977): the closer a project is to actual on-the-ground implementation, the more detailed the inventory work required. The pedestrian survey entails an intensive ground-level search for archaeological sites, frequently augmented by test excavations. Its purpose is to approach as closely as possible the goal of a 100 percent inventory of sites. This
information can then be used to assess the impact on archaeological resources of a project proposal, while still providing time for modifications to be made in the project to accommodate threatened cultural resources.

But when the purpose of a survey is to identify the archaeological resources of an entire region, such as the state of Illinois, or when it is meant to provide a general idea of the distribution of archaeological sites during the preliminary stages of planning, this strategy is obviously impractical. Accordingly, archaeological resource managers have been seeking to develop a new inventory technique, the so-called "predictive survey." The objective of this type of survey, as the name suggests, is to predict the distribution of archaeological sites in a region on the basis of a representative sample of just a fraction of the area (King, 1978). Predictive surveys, of course, should always be verified by on-site inspection prior to any ground-disturbing action. However, if a reliable predictive survey for an area can be developed, it would not only save on costs, but would provide an indication, at the earliest stages of the planning process, as to where potentially sensitive areas may be located. The value of such knowledge to land developers and archaeologists alike is undeniable. Much of the hostility and adverse publicity surrounding construction projects delayed by archaeological salvage can be attributed to a lack of site location information during the preliminary planning phase. If this information is available from the beginning, even in a general form, plans can be
adjusted to minimize the impact on archaeological resources, and to reduce the need for salvage operations at a later date.

It must be stressed that predictive surveying is still in the experimental stages of development. The Interagency Archaeological Services Division has encouraged this development by funding such operations for the outer continental shelf of the Gulf of Mexico (Gagliano, 1977), the metropolitan St. Louis area (Benchley, 1976), and east-central New England (Dincauze and Meyer, 1976). At the state level, the Illinois Department of Conservation has divided the state of Illinois up into ten regions and is currently sponsoring an experimental survey in each (Brown, 1978). Although predictive survey techniques are still in the developmental stage, and thus require additional refinement, they show promise of becoming an indispensable planning tool in the field of archaeological resource management.

If the purpose of an archaeological survey is identification, to answer the question, "what have we got?" then for such information to be of optimum use, it must be available during the initial stages of the planning process, when a number of alternative courses of action are still being considered. The predictive survey may prove to be the most efficient and economical technique available for collecting inventory data during this period. But when a single alternative has finally been selected, a comprehensive on-site inspection of the affected area should be undertaken, using as its point of departure information gained during the more general predictive survey.
Evaluating the significance of archaeological resources is one of those necessary evils that all prehistorians involved in cultural resource management must face. And yet it is also an activity that every practicing archaeologist carries out any time a decision is made to excavate "here" rather than "there", or to collect "this" rather than "that."

Executive Order 11593 states that cultural resources should be administered, "in a spirit of stewardship and trusteeship for future generations." (Section 1). At the same time, however, there is a realization that not every prehistoric site can be preserved for the future, or until its excavation is required to test some as yet unformulated research hypothesis. The fact of the matter is that some sites will be sacrificed to the bulldozer, and others will be excavated "before their time" lest they meet the same fate. As distasteful as this situation may seem to some, it is still a marked improvement over the wanton destruction of sites that characterized the first 150 years of this country's history. Therefore, archaeologists will have to come to terms with the reality that some scheme must be devised for ranking archaeological sites in order of their significance. In answer to the question, "what do we keep?" the response is, "we keep those sites which are most significant."

What, then, should be the basis for measuring significance? The currently popular approach to significance evaluation in archaeological resource management involves an assessment of
the extent to which a site will contribute to an understanding of regional research problems in prehistory. This is the criterion of "scientific significance." Its application is being encouraged by the Federal Office of Archaeology and Historic Preservation (Glasgow, 1977) and it is also a principle component of Illinois' embryonic archaeological resource management plan (Downer, 1978). The advantage of such a criterion is that it allows a single site to be considered in terms of its relationship to other sites in a region rather than as an isolated entity. In other words, more of the contextual information of the resource is taken into account in evaluating its importance. The approach also provides a focus for research through the incorporation of at least a general problem orientation.

If the criterion of scientific significance proves workable (and there are those who think it won't), it will not preserve from salvage all significant sites. But it will result in salvage archaeology that produces something more than a simple catalogue of artifacts collected during excavations. Findings will be related to substantive problems in the field of prehistory, at least to the extent that such problems have been adequately defined. In this last respect, some regions have clearly surpassed others, but if archaeological resource management is to make a real contribution to prehistory, comparable to that of scientific archaeology, a major emphasis should be placed on the continual development and testing of research hypotheses, even though construction projects instead
of theory will provide the major dictum as to which sites are to be excavated.

An interesting point is that scientific significance need not be restricted to the discipline of prehistory. The knowledge gained from archaeological sites has made demonstrable contributions to such other endeavors as flood control need assessment, soil genesis and management, climatology, and land-use planning in marginal environments (Dixon, 1977; Schiffer, 1978). These potentialities should also be considered when evaluating the scientific significance of archaeological resources.

Those familiar with historic preservation law may ask why there should be concern for establishing criteria for archaeological significance in the first place. Title 36 of the Code of Federal Regulations (Section 800.10), in reference to the eligibility of properties for the National Register, clearly states that any archaeological site is significant that has, "yielded, or may be likely to yield, information important in prehistory or history." Although at first glance this statement seems compatible with the criterion of scientific significance, it lacks the focus provided by the incorporation of specific research problems. From a planning standpoint, the National Register criterion has not been particularly useful. It has proved to be the case that in applying this definition, there has hardly been an archaeological site in the country that was not ruled eligible for inclusion on the National Register, and thus labelled "significant." There is nothing
intrinsically wrong with this as long as the Register is not used as a planning tool. Unfortunately, the recent tendency has been toward just this application of the Register coming about. The result has been an archaeo-bureaucratic headache of rather large proportions. Federal agencies are required to take into account, and if possible avoid, the impacting of any project they license or fund on properties eligible for the National Register. By this criterion of "legal significance," however, almost every archaeological site is defined as eligible. Those eligible sites which would be adversely impacted must then be salvaged or protected. Again, when the criterion of legal significance is applied, there is scarcely a site known which would not be eligible for protection under these guidelines.

It would seem that such a state of affairs would be an archaeologist's dream. The problem is that it threatens to create an unworkable situation, in which agency representatives would as soon "overlook" a site as burden themselves with the salvage of every one they locate. Under these circumstances the willful destruction of genuinely important resources becomes a very real possibility.

The blanket coverage afforded archaeological sites by the National Register definition of significance has been especially criticized by the director of the Advisory Council's Office of Review and Compliance (McDermott, 1977:58):

We cannot, under present circumstances, protect or salvage every lithic scatter in this country. We do not, for example,
take the posture that every building
that is 50 years old has to be con-
sidered worthy of preservation. We
must learn to be selective.

In the attempt to establish a more workable system for
ranking archaeological sites, factors other than legal sig-
nificance must be considered. In addition to scientific sig-
nificance, others which have been suggested include ethnic
significance, public significance, and significance for tech-
nical or methodological problems.

Ethnic significance, as defined by Moratto (1975), refers
to a site having, "religious, mythological, social or other
importance to a discrete population." The increasing awareness
and concern among many ethnic groups for their cultural heritage
has made this an important consideration in the evaluation of
archaeological sites. There have been situations in the recent
past where such significance has been an overriding factor in
site evaluation. It is ignored only at great peril.

Public significance refers to sites which might contribute
to public education through interpretation and exhibition, or
which might benefit the local economy as tourist attractions.

Finally, technical-methodological significance becomes im-
portant when evaluating those sites which may afford the oppor-
tunity to experiment with new archaeological methods and tech-
nologies. Examples might include devising ways to identify sites
where the ground is obscured by vegetative cover, the testing
of remote sensing techniques such as thermal infrared scanning,
or the establishment of temporal sequences for nonstratified sites.
In summary, the crucial issue in answering the question, "what do we keep?" revolves around the evaluation of site significance. Scientific, ethnic, public, and technical-methodological significance are all factors which should be considered in ranking the importance of archaeological resources. Once some measure of consensus is achieved as to site value, attention can then be focused on various strategies for assessing the impact of project effects on those archaeological resources which are deemed significant. Issues related to impact assessment are discussed in the final section of this paper.

How Do We Do It: Archaeological Impact Assessment

Prior to the 1970's, the subject of impact assessment was of little concern to archaeologists. The direct effects of a project were assumed to be either inconsequential, or to lead to total site obliteration; no impacts between these two extremes were considered. In addition, there was a marked indifference displayed toward the analysis of any secondary or tertiary impacts resulting from ground-disturbing activities.

This situation has changed during the last decade as a result of the passage of the National Environmental Policy Act. Not only are more detailed forecasts of the extent and types of impacts needed, but when such impacts are most likely to occur should also be known. The former information is necessary to establish management priorities for mitigation procedures (including salvage and/or avoidance of sites) and to satisfy the requirements of Section 102(c) of the National Environmental
Policy Act; the latter is required to schedule efficient mitigation procedures in those cases where adverse impacts seem unavoidable.

In order to adequately forecast impacts, one must first realize that while all construction projects generate effects, such as excavation or ground clearance, not all effects result in impacts. Whether or not an effect's potential for impacting archaeological sites is realized depends primarily upon the nature of the resource in the area. In order to carry out an impact assessment, each of the following kinds of information must be taken into account (Schiffer and Gumermen, 1977a):

(i) The effects of all activities that occur during a project's planning, construction, and operating stages

(ii) The nature of the archaeological resources in all affected areas, including their adjudged significance

(iii) The relationship between anticipated effects and the actual impacts on archaeological resources

It should be noted that while items (i) and (ii) are relatively easy to ascertain, item (iii) lies at the very frontier of current archaeological knowledge. Schiffer (1976) conceptualized the relationship between effects and actual impacts with the aid of the dual constructs of c-transforms and n-transforms. C-transforms embody those principles related to the operation of cultural processes upon the archaeological record. Plowing may displace small artifacts both vertically and horizontally and damage larger ones.
Pot hunting by amateur collectors often results in a site being deprived of its finely finished tools, or in the occurrence of predictable strata disturbances occasioned, for example, by tendencies of some collectors to dig in the corners of pueblo rooms where mortuary offerings are usually buried. This interaction between the activities of amateur collectors and the status of archaeological resources is the subject area of the new sub-discipline of ethnoarchaeology.

N-transforms refer to regularities in the operation of natural processes as they affect archaeological sites. A widespread problem for which there is little information available concerns the impact of flooding on site deposits. Beyond the obvious fact that water gets sites wet, there is very little additional knowledge pertaining to this particular effect. Yet, such information could drastically affect mitigation policies. Under certain conditions, it is possible that flooding may actually be an effective preservation tool. Archaeologists are generally in agreement that, whenever feasible, the preservation of sites is to be much preferred over the premature excavation often entailed in salvage operations. With the continued development of underwater archaeological techniques, it is not unreasonable to speculate that the submersion of archaeological sites, in order to preserve them for future generations with different problem orientations, may become a more common practice in the years to come.

The problems of impact assessment caused by inadequate knowledge of the natural and cultural processes operating on
archaeological resources are magnified when one moves beyond a consideration of the direct effects of a project to its secondary and tertiary effects. The direct effects of a flood control project are easily recognized, for example, and include such activities as clearing the land, digging the footings for a dam, and reservoir impoundment. Secondary effects are those supporting activities related to the primary goal, such as access road development, the construction of temporary housing for workers and displaced residents, and the recreational use of the reservoir. Tertiary effects are the most difficult to deal with, but generally include those not directly related to the first two categories, including increased land use below the dam or construction of vacation homes and resorts along the reservoir shoreline. It is easy to imagine that all of these effects may be directly translatable into impacts upon archaeological resources. To regard a flood control project as just that, without taking into account its spin-off effects, is to seriously restrict the value of an impact assessment. Even if all the processes operating upon archaeological resources are perfectly understood, this knowledge is of limited use if secondary and tertiary effects are ignored.

It should be apparent that, independent of any legal requirements, an expanded knowledge of impact processes would provide considerable scientific benefits to the study of prehistory. There is a huge difference between knowing that plowing will damage a site, and being able to relate the kinds of conditions of plowing to specific disturbances in the
archaeological record. Besides allowing for more comprehensive impact assessments, this knowledge provides the archaeologist with a tool for estimating particular biases in site deposits occasioned by ground-disturbing activities. From a research perspective, this is invaluable information.

CONCLUSION

Although official concern with the protection of archaeological resources in this country can be traced back almost a century, the rapid growth of archaeological resource management is a relatively recent phenomenon. It should not be surprising that this rate of growth has been accompanied by a variety of procedural and methodological difficulties that can only be resolved over time in consultation with all concerned parties. In many ways, archaeological preservation must be treated separately from historic preservation. There is an obvious difference in the nature of the resources each activity is designed to protect, and, indeed, in the purposes for which protection is desired. It is encouraging to note, in this regard, that the Heritage, Conservation, and Recreation Service has singled out archaeological resources as one of the priority areas to be considered in developing its 1979 Historic Preservation Policy Plan (Preservation News, April 1979). A similar concern is in evidence within the state of Illinois' Division of Historic Sites. Although it is unrealistic to believe that we can save every archaeological site for posterity,
neither do we have to abandon them to complete impoverishment. Deciding which sites are worthy of protection, and developing the techniques to accomplish this, will continue to require the concerted efforts of all those involved in archaeological resource management.
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WHO WERE THE PLAINS INDIAN BERDACHES?

by

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Abstract

The standard anthropological view of berdaches is that they were men who took up their society's version of the woman's role by choice -- male homosexuals who adopted women's dress and women's work without any loss of respect from their respective communities. There is very little in the ethnohistoric and ethnological literature to support this point of view, however. The scattered and varied references to berdaches among the Plains Indians reflect a more complex situation. Berdaches may not always have been homosexuals, sometimes did not wear women's clothing, performed roles that were not identical to women's roles, and, in at least some tribes, appear to have inherited their status.

It is probably misleading to assess Plains Indian berdaches in terms of the sex role and sexual identity variations recognized in our own society. The ideas and models implied by the terms homosexual, transvestite, and transsexual are not easily applied cross culturally. The processes of acquisition of sexual identity in Plains Indian societies were probably different enough from those in our own society (which are still poorly understood) to render analysis of them unrewarding at this time.
Introduction

Plains Indian tribes, like the other natives of North America, included in their number a few men who occupied a special status to which the term berdache has been applied. The roles and values associated with this status varied somewhat from tribe to tribe. Unlike other statuses, however, which anthropologists have eventually come to analyze in terms of the social systems in which they exist, the berdache is still interpreted primarily in terms of models taken from the context of modern western culture. It is the purpose of this paper to demonstrate that these models are not useful for interpreting berdaches and to clear up some misconceptions which have arisen from their use.

The standard anthropological view of berdaches is that they were male homosexuals who adopt women's dress and women's roles without the risk of censure by their respective societies. Aversion to the standard male role, homosexual tendencies, and parental behavior have been suggested as reasons why some Indian men became berdaches. In his cross-cultural survey of North American Cultures, for instance, Driver describes berdaches as follows:

Some men, however, had such a strong aversion to this ultra-masculine role that they would have been complete failures in the society if there had not been an escape for them. They donned the clothing of women, did women's work, and sometimes lived homosexually with another man. As Berdaches they were accepted by their societies, and were even allowed, like women, to carry scalps in the victory dance on the
return of a successful war party.

Driver 1961: 535

Ford and Beach provide a similar picture in their survey of sexual behavior:

The most common form of institutionalized homosexuality is that of the berdache or transvetite. The berdache is a male who dresses like a women, performs women's tasks, and adopts some aspects of the feminine role in sexual behavior with male partners. Less frequently, a women dresses like a man and seeks to adopt the male sex role.

Ford and Beach 1951: 130

When we turn to the ethnohistoric and ethnological literature on which this view is supposed to be based, however, the evidence for these conclusions is not altogether obvious. What is apparent is that anthropologists have taken concepts of male homosexuality and transvestism from their own culture and applied them uncritically to berdaches.

The term berdache (or bardache) is derived from the Persian bardah, a slave, through Arabic to the Italian bardascia or bardassi, French bardache, and Spanish bardaja or berdaje, with the meaning of slave boy. It may have entered English either from French or Spanish or possibly both. Webster's New International Dictionary has the Spanish berdaje as the source for the English usage, but the earliest appearance of the term as applied to Native Americans, in 1548, was in the portion of the New World controlled by the French (Thwaites 1959:LIX: 309-310). The Old World term was applied to a New World practice, the exact content of which was seldom made
clear, probably because of its abhorrent nature (from the point of view of the Europeans). Berdaches have been called eunuchs, hermaphrodites, male prostitutes, transvestites, and male homosexuals. Some of these attributions are grossly incorrect while others are more subtly misleading. The problem arising with respect to the use of each will be discussed in turn.

Eunuchs

Native American berdaches were apparently first recorded by Cabeza da Vaca, a Spaniard stranded on the coast of Texas in late 1520's.

In the time I was among these people, I witnessed a diabolical practice; a man living with a eunuch. Eunuchs go partly dressed, like women, and perform women's duties, but use the bow and arrow and carry very heavy loads. We saw many thus mutilated. They are more muscular and taller than other men and can lift tremendous weight.

Cabeza da Vaca 1961: 100

Since da Vaca describes the berdaches as going partly clothed (i.e., wearing skirts), there is no reason to take the term eunuch literally. In another passage, da Vaca refers to "sins against nature," a phrase which is usually a euphemism for male homosexuality. It is likely that this is another reference to his "eunuchs."

The only other mention of eunuchs in native North America of which I am aware (Kurz 1937: 182) also appears to be a
reference to berdaches, which are identified by the terms berdache and hermaphrodite elsewhere in the same work. (Ibid: 211).

Hermaphrodites

A much more common usage was to refer to berdaches as hermaphrodites (Clark 1884: 210, Simms 1903, Wied 1906: 283, Kurz 1937: 211, Dorsey 1906: 139-140). This practice appears to derive from the Plains Indian sign language, in which the sign for berdache was identical to the sign for an hermaphrodite animal.

Hermaphrodite. Conception: Half male, half female. Make sign for Male, then hold lower edge of right hand against breast, fingers extended and touching, back of hand nearly to left; move the hand to right, then make sign for Female, and holding hand as above, move it to left.

The Crow tribe of Indians seem to have had several well-authenticated cases of hermaphrodism.

Clark 1884: 210

The identity of the gestures for hermaphrodite animal and berdache in the sign language does not necessarily mean that any relationship between hermaphrodite animals and human berdaches was recognized by the Plains Indians. Rather, the gesture sign was simply a conventional gloss for the various terms in the many Plains Indian languages. The Crow term, bate (Lowie 1912: 226), the Cheyenne hee'maneh (Grinnell 1923 II: 39), and the Osage mixu'ga (Fletcher and La Flesche 1911: 132) are obviously not cognates, and the Omaha term means "instructed by the moon"
referring to the vision of the moon goddess which validates a man's identity as a berdache. Denig, who spent many years as a trader on the Upper Missouri and who was conversant with the Indian cultures of that area clearly states that berdaches were not hermaphrodites in the biological sense even though he too uses the term: "This does not proceed from any natural deformity, but from the habits of the child" (Denig 1961: 187).

Male Prostitutes

In addition to the references to berdaches as eunuchs and hermaphrodites, there is at least one definition of berdaches which describes them as male prostitutes (Webster's New International Dictionary, 2nd edition), an allegation for which there is no documentation in Plains Indian literature or, as far as I can determine, in any of the literature on Native American cultures.

Transvestites

Modern descriptions of berdaches tend to refer to them either as transvestites or as male homosexuals or both. These concepts, taken from the context of modern western civilization and applied to Plains Indian societies, are almost as misleading as the earlier references to eunuchs and hermaphrodites. This is not to say that some berdaches did not participate in homosexual behavior or did not wear women's clothing but that the referents to our terms homosexual and transvestite, whether used as folk concepts or scientific terms.
There is no doubt that most Plains Indian berdaches wore women's clothing (Cabeza da Vaca 1961: 100, Fletcher and La Flesche 1911: 132-133, Denig 1961: 187, Mallery 1894: 142, De Smet 1905: 1017, Simms 1903: 580-581, Wied 1906: 283). Even so, not all berdaches were thus attired: Cheyenne berdaches occasionally wore women's dress, but they were usually in the attire of old men (Grinnell 1923 II: 39). More important are the psychological factors assumed to underlie the behavior of transvestites. A typical textbook of psychosexual disorders lists five types of transvestism classified according to the "basic force causing the perversion" (Allen 1962: 243-252). They are: 1) heterosexual, 2) homosexual, 3) narcissistic, 4) asexual, and 5) bisexual. In addition, the author discusses fetishism and exhibitionism as other possible roots to transvestite behavior. The question here is which of these factors, if any, apply to the fact that Plains Indian berdaches usually wore women's clothing. I suggest that if we cannot answer this question the designation of berdaches as transvestites is inappropriate since the etiology and connotations of the behavior may be entirely different from those of transvestites in our own society.

Homosexuals

Similar problems apply to the designation of berdaches as homosexuals. It is important to distinguish between our folk concept of homosexuality as a life-long character trait and the more recent scientific approach which allows discussion
of homosexual behavior and homosexual fantasies but which avoids labeling persons as either homosexual or not except in discussion of those individuals whose self image has come to incorporate elements of the folk concept (Pomeroy 1969: 3).

The etiology of homosexual behavior in our own society is the subject of much debate, the content of which need not be discussed here. One line of reasoning about the development of psychosexual identity which may prove applicable to some Plains Indian berdaches, however, is that developed by John Money (cf Money 1965 for a brief discussion). Money sees the acquisition of psychosexual identity in humans as essentially similar to imprinting in animals. Case studies of hermaphrodites have shown that an infant's sex can be reassigned without noticeable ill effect on its later development of the reassignment is made prior to 18 months of age (Ibid: 12-13). These data suggest that at least some cases of abnormal gender identities originate in infancy. Abnormal patterns of sexual identity established at this time may not become evident until puberty, however.

Puberty is also the time at which prior errors and defects of psychosexual differentiation announce themselves in full.... Whether or not psychosexual pathologies may be induced at puberty is arguable, but it is true that a great many of them have a long "psychoembrionic" period before puberty.

Money 1965: 14

The pattern Money describes finds a good fit in Crow theory and practice. According to Denig (1961: 187), the
Crow officially recognized the status of individuals as berdaches when they reached puberty. The preference for the roles and trappings of femininity was exhibited prior to puberty, however, and was thought by the Crow to be a natural and uncontrollable tendency. The berdache-to-be developed this disposition in the face of parental disapproval and contrary to an educational system which emphasized making "men out of boys and wives out of girls" (Kurz 1937: 179).

There were other roads to becoming a berdache which did not fit this pattern, however. In some tribes men appeared to have become berdaches through inheritance of a socially recognized and respected ceremonial status. This will be discussed below. It is interesting to note here, however, that in at least one tribe with this method of recruitment of berdaches, boys were not initiated into that status at puberty but in their late teens (Bowers 1965: 166).

Still another way to become a berdache was to enter this status by choice as an adult. This pattern occurred among the Osage for whom La Flesche and Tixier recorded a number of cases of warriors electing to become berdaches (Fletcher and La Flesche 1911: 133, Tixier 1940: 234). The men in these stories were successful warriors, making any supposition that they were reacting to failure in the male role unlikely (cf. Driver 1961: 535). It is possible that they fit into another pattern described by Money.

The human brain maintains its adult pattern of psychosexual differentiation relatively
stable and constant, for the most part, though as some people gain in age and experience, there may be a lifting of restraints against behavior they once tabooed.

Money 1965: 19

That the Osage warriors who became berdaches may have earlier suppressed a deviant gender identity seem quite possible, since, as we shall see below, the attitudes toward berdaches in Osage society appear to have been quite negative.

It must be emphasized before moving on to a discussion of the sexual behavior of berdaches that the suggestions made here about the psychosexual development of berdaches are tentative in the extreme. We know little enough about the development of deviant gender identities in our own cultural milieu, and the lack of a theoretical base is compounded by the lack of sufficient data for almost all Native American societies.

Turning to the actual sexual behavior of berdaches, we find that much of the evidence in the early literature consists of euphemistic references -- "sins against nature" (Cabeza da Vaca 1961: 79), "gross actions" (Fletcher and La Flesche 1911: 132), "unnatural practices" (Wied 1906: 282) -- but there are a few direct descriptions as well. From the early sources, we have David Thompson's statement that the Hidatsa "are much given to unnatural lusts and often prefer a young man to a woman. They have many berdaches amongst them who make it their business to satisfy such beastly passions" (Coues 1897: 348). Among the later studies, the best direct reference to homosexual
behavior by Plains Indian berdaches is Ford and Beach's description of Crow berdaches.

Sodomy apparently is absent among the Crow Indians, although oral-gential contacts are fairly frequent. A few Crow men adopt women's dress and mannerisms and live alone. Adolescent boys and occasionally men visit these bate, as they are called. The bate stimulates the boys genitals orally. One informant stated that there were such men in his community and that seventeen of his adolescent friends visited them occasionally.

Ford and Beach 1951: 133

An excellent reference to berdache homosexuality, but one which falls outside the area under consideration here, is Devereaux's (1937) description of Mohave berdaches.

In contrast to these statements are a number of indications that some berdaches did not engage in homosexual behavior. It may be significant, for instance, that Ford and Beach recorded intercourse per anum among only two of the fourteen North American Indian tribes in their sample which had berdaches. Fletcher and La Flesche wrote that Omaha berdaches "sometimes become subject to gross actions" (emphasis added), and they mention an Osage berdache who married a woman and had children (Ibid: 133). Denig (1961: 187-188) also describes a Crow berdache married to a woman: "One of these has been married and presents the anomaly of husband and wife in the same dress attending to the same domestic duties." Hoebel goes so far as to imply that Cheyenne berdaches tended to be asexual rather than homosexual.
These people, through sexual sublimation--with their self-abstinence and denial of their natural born sex -- seem to achieve great power. Although we have no direct evidence for it, it appears probable that their presence on war parties is desired mainly because of their high "psychological" potential for stored up virility -- which is just what the Cheyenne feel is necessary for successful fighting.

Hoebel 1960: 77

The fact that Cheyenne berdaches usually wore the type of clothing appropriate for old men rather than women's clothing (Grinnell 1923 II: 39) may be a symbol of their non-sexual rather than homosexual nature.

Many berdaches appear to have married other men, but this arrangement may often have been more economic than sexual. Hoebel, for instance, specifies that Cheyenne berdaches "often serve as second wives in a married man's household" (Hoebel 1960: 77), and Tixier observed an Osage berdache living as second wife to an Osage chief (Tixier 1940: 234). The Hidatsa told Bowers that some berdaches lived as co-wives in extended households while others married "older men, generally without children and having trouble keeping their wives" (Bowers 1965: 167). On the other hand, Bowers informants also made it clear that a berdache was a significant economic asset to a household:

According to tradition, these were well-to-do households. The "man-woman" worked in the garden, did bead work, and butchered as did the women. Being stronger and more active than the women, the berdache could do many things more efficiently and was never burdened down with childbearing.

-53-
Accounts we have of the berdaches tell of industrious individuals working harder than the women of the village and exceeding the women in many common activities. Informants felt that separate households established around the berdache were very often better fixed than those where the men carried on active military duties.

Bowers 1965: 167

The weight of the evidence seem to indicate that berdaches were not preferred for first wives in spite of the economic advantages they could confer on a household. This has implications for the interpretation of the behavior of the men who married berdaches: we can assume that most of them were not preferential homosexuals.

Prevalence of Berdaches

Estimates of the numbers of berdaches among Plains tribes fall into two categories. There are vague references to many berdaches and there are specific counts which tend to be quite low. On the one hand are such phrases as "we saw many thus mutilated" (Cabeza da Vaca 1961: 100), "they have many berdaches or hermaphrodites among them" (Wied 1906: 283), "many berdaches" (Coues 1897: 348), and "hermaphrodites are frequent" (Kurz 1937: 211). On the other hand are the actual counts of the number of berdaches observed among the various tribes. According to Denig (1961: 187), writing in the mid-19th century, "there used to be some five or six of these hermaphrodites among the Crows, 'tho at the present time there are but two or three." At the turn of the century,
Simms (1903) found three berdaches among the Crow, and by 1912 Lowie reported only one. Similarly, Grinnell (1923 II: 39) named five Cheyenne berdaches alive at the beginning of the 19th century, a figure which he says was reduced to two by the latter half of the century. Again, although Maximillian reported many berdaches for the Crow whom he met only as visitors to a trading post, he reported "only one such among the Mandans, and only two or three among the Minitaries" whose villages he visited (Wied 1906: 283). A similar count may be inferred from the fact that La Flesche's informant provided him with tales of three Osage berdaches in 1898 (Fletcher and La Flesche 1911: 132-133).

The only specific reference to more than a handful of berdaches among any Plains tribe is the following description of the Hidatsa:

At the time this study was made, informants could remember two such people in the generation above them, but they had heard that in former times there were as many as 15 to 25 berdaches in their villages.

Bowers 1965: 167

This statement is in stark contrast to Maximillain's count of "two or three" made a century earlier.

I am inclined to accept the actual counts rather than the vague references to "many" berdaches among the Plains Indians. There small numbers, compared with tribal populations that numbered in the thousands: (cf Lewis and Clark 1905: 81-113 for estimates made at the beginning of the 19th century) indicate
that berdaches comprised less than one percent of the adult male population of the tribes considered here.

The Status of Berdache

If we view berdaches as people who occupied a particular status rather than individuals who shared certain deviant behavior patterns, some references to Plains Indian berdaches are clarified. A status is associated with specific roles, modes of recruitment, and ranking relative to other statuses. These facets of a status can be expected to vary from culture to culture, and this seems to be the case among the Plains tribes.

There were at least two patterns of recruitment of berdaches on the Plains. In some tribes, the status appears to have been inherited, while in others it was open to all who had what was considered either a natural proclivity for it or a supernatural claim to it. The clearest description of the inheritance of the status is provided by Bowers in his study of the Hidatsa:

The berdache was a brother or the son of a man holding tribal ceremonial rights in the Woman Above and Holy Woman bundles. There are no known instances of exceptions to this rule, and the Hidatsa believed that only those persons standing in these relationships to those bundles ever assumed the woman's role. The berdache commonly adopted orphans from the village or secured young daughters and sons through the capture of prisoners by their relatives, transmitting their property and their ceremonial knowledge to their younger adopted children.

Bowers 1965: 167
The Cheyenne, who had close ties to the Hidatsa in the protohistoric period, also appear to have recruited berdaches through inheritance. At any rate, all of the recorded Cheyenne berdaches belonged to the Bare Legs band, one of ten such groups that made up the Cheyenne tribe. Since Grinnell records the presence of seven berdaches among the Cheyenne during the 19th century, the likelihood that all seven occurred in the one band by chance is quite low. To conclude that this distribution was determined by some sort of inheritance seems quite reasonable.

In other tribes, there is no hint of inheritance of the status of berdache. Among the Crow, for instance, the people who became berdaches gave early indications of a preference for female pursuits, a predilection which was formally recognized at puberty when they became berdaches.

This does not proceed from any natural deformity but from the habits of the child. Occasionally, a male child, when arrived at the age of 10 or 12 years or less, cannot be brought to join in any of the work or play of the boys, but on the contrary associates entirely with the girls...

Children of different sexes seldom associate either in their work or play, 'tho as has been observed, instances do occur in which a boy acquires all the habits of a girl, notwithstanding every effort on the part of his parents to prevent it. The disposition appears to be natural and cannot be controlled. When arrived at the age of 12 or 14, and his habits are formed, the parents clothe him in a girl's dress and his whole life is devoted to the labors assigned to the females.

Denig 1961: 187

There is no hint in this passage of any formal inheritance of the status, nor is there any indication of supernatural
sanction approving access to the status. For many Plains tribes, on the other hand, we have records of visions of supernatural beings which require that the individual become a berdache (Dorsey 1906: 139-140, Fletcher and La Flesche 1911: 132-133, Bowers 1965: 166). These visions differ only in their specific content from the visions which ensured success in warfare for warriors or which provided the supernatural powers of shamans and medicine men. The visions reported by men who became berdaches provided them with the supernatural power needed for that status just as visions provided other men with the power for the statuses they entered.

The roles performed by berdaches appear to have varied considerably from tribe to tribe. One set of roles were associated with warfare. Berdaches accompanied war parties of the Cheyenne (Grinnell 1923 II: 40, Hoebel 1960: 77), Sioux (Grinnel 1956: 237-238), Caddo and other tribes (Swanton 1942: 189). The Cheyenne berdaches observed the course of battle and treated the wounded (Grinnell 1923 II: 40), and Sioux berdaches predicted the outcome of battle (Grinnell 1956: 237-238). Marquette (1959: 129) asserts that Sioux and Illinois berdaches took part in the fighting, using clubs rather than bows and arrows. Cheyenne berdaches directed the scalp dance when a successful war party returned (Grinnell 1923 II: 39-44), and Mandan and Hidatsa berdaches danced naked in the ceremonial dances of the warrior societies (Kurz 1937: 182). Crow berdaches had a special role in the Sun Dance which ensured the success of a war party setting out to revenge the
death of a member of the tribe (Lowie 1915: 312). That Hidatsa berdaches, who had the same role in the Hidatsa Sun Dance, were intimately associated with warfare is strongly implied by Bower's (1965: 168) statement, "Berdaches tended to disappear once warfare had ceased and their ceremonial system had collapsed."

In other tribes, there may have been no association of berdaches with warfare. Among the Osage, berdaches either did not go on war parties at all, or, if they did, it was as warriors in male clothing rather than as berdaches (Tixier 1940: 234, Fletcher and La Flesche 1911: 133). Even here, Driver's (1961: 535) suggestion that the men who became berdaches were reacting against the "ultra-masculine role" of the warrior appears to be contradicted since some Osage berdaches were formerly successful war leaders and at least one continued to participate in war parties after he became a berdache (although he abandoned his female attire for the clothing of a warrior when he did so).

Berdachines in some tribes were considered to be medicine men with the ability to cure wounds or diseases (Grinnell 1923 II: 40) or who had other unspecified supernatural powers.

The berdache performed many ceremonial roles. When the Sun Dance ceremonies were performed, it was the berdache's duty to locate the log for the central post from driftwood in the river. Whenever a major ceremony was being given, the berdache would dress like the other members of the Holy Woman society and receive gifts as an equal member with the women of the society.

The berdaches comprised the most active
ceremonial class in the village. Their roles were many and exceeded those of the most distinguished tribal ceremonial leaders.

Bowers 1965: 167

It is obvious from the foregoing discussion that the roles of many berdaches were not identical to women's roles. The exact nature of the differences is not always clear, however, and berdaches are often referred to in feminine terms. Furthermore, they often followed female patterns of behavior in both overt and subtle ways. Osage berdaches used the feminine forms of speech (Fletcher and La Flesche 1911: 132), and the Sioux winter counts record the suicide of a berdache by hanging, which was the women's way of taking their own life (Howard 1960: 375).

The attitudes toward berdaches on the part of the other members of their societies varied considerably. Interestingly, this variation appears to be correlated with the mode of recruitment to the status. In the two tribes in which the status of berdache appears to have been inherited, attitudes toward them were positive. Grinnell (1923 II: 39) describes Cheyenne berdaches as "very popular," and Bowers writes of Hidatsa berdaches as respected and feared people with a great deal of supernatural power (Bowers 1965: 167-168). In tribes in which the status does not appear to have been inherited, on the other hand, attitudes toward berdaches seem to have been negative. The Crow parents of a berdache "regret it very much" (Denig 1961: 187), and when a Crow berdache participated in the Sun Dance, he had to be dragged from hiding by the police, much to
the amusement of the crowd (Lowie 1915: 312). A Pawnee who had visions telling him to become a berdache felt shame (Dorsey 1906: 139-140), and among the Omaha suicides occasionally resulted from this circumstance.

Instances are known in which the unfortuante dreamer, even with the help of his parents, could not ward off the evil influence of the dream and resorted to suicides as the only means of escape.

Fletcher and La Flesche 1911: 132

Summary

The nature of the Plains Indian berdache has been poorly understood. Ethnohistoric and early ethnographic sources refer to berdaches as eunuchs and hermaphrodites. Later works have identified them as male prostitutes, transvestites and homosexuals. All of these terms represent concepts taken from cultures other than those native to North America and are misleading when applied to berdaches. I have suggested that it is more fruitful to consider the berdache as a status. In that light, the modes of recruitment, roles, and prestige associated with the status were examined, and evidence suggestive of two different patterns was found. In at least two Plains tribes, the status of berdache was inherited and a good deal of prestige was attached to it. In other tribes anyone who had the appropriate vision or personal characteristics could become a berdache, but in these societies, berdaches tended to be despised rather than respected or admired.
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CULTURAL DIFFUSION: A BRIEF OVERVIEW
OF
POPULAR EXTREMES,
Some Conceptions and Misconceptions

by
Ben Urish

Best Wishes
Ben Urish
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Cultural Diffusion: A Brief Overview of Popular Extremes, Some Conceptions and Misconceptions

It is an accepted fact that cultural diffusion exists, and has existed. But in the last two centuries, the defining and re-defining of "Cultural Diffusion" has caused several theories which captured the public's attention, much to the majority of the sciences' disparagement. By observing a few of these examples of "extreme diffusionism", an extremest lineage becomes apparent, as well as several similarities in methodological and technical approach.

It should be noted that this work is only a surface probe into extreme diffusionist thought, it has no pretentions for the school of cultural diffusionism as a whole, and is not even intended to be a detailed, in-depth study of the extremist view, (which is, after all, a relatively small area to begin with).

The creative mind in science is often described as a "luxurious necessity". But only when properly disciplined. If unchecked, any attempt at the scientific process is virtually doomed, usually resulting in fanciful flights, grounded only in poor interpretations of the facts used.

Sadly, many useful inquiring minds were closed in the trap of extreme diffusionism. But this is not to say that all such studies were useless. In general, it is only the interpretations of facts that the extremists are censurable for. They are rightfully commended for their research methods and results. The catalogue of traits and other aspects
uncovered by the extremists' in their quests to find evidence that would support their claims and ideas is immeasurable. If for nothing else, they should at least be thanked for these worthwhile accomplishments.

Perhaps the biggest complaint against the extremist (outside of their theories) is their use of aspectual data. By this term it is meant that only certain aspects of the data are used, and only certain datas are considered for use. If a fact doesn't fit, it is thrown out. If possible, it is adapted to the overall theoretical scheme, in almost any fashion possible. Again, this is an example of immense enthusiasm and a lack of discretionary thought. It is also noteworthy that this stylistic approach can seduce the unwary reader into accepting the views as proposed. Also, archaeological evidence can be used or discarded as seen fit.

In this paper, two main types of extreme cultural diffusionist thought shall be examined. The "Unqualified Extremism", such as the works of Ignatius Donnelly, and Erich von Daniken; and, "Qualified Extremism", like the studies of Thor Heyerdahl.

There seem to have been three basic unqualified viewpoints. They are:
1) All "civilization" (In the context of these writings, civilization can be taken as interchangeable with "culture") diffused from Atlantis.
2) All "civilization" diffused from Mu (also called Lemuria).
3) All "civilization" diffused from Extra-terrestrials.

The contention that all culture diffused from Egypt is
perhaps best termed as "Semi-Qualified", and shall be discussed later.

All three aspects of Unqualified Extremism share striking general similarities. Among them:
1) None were originally proposed by professional Anthropologists.
2) All used selective data.
3) All were scoffed at by professionals.
4) All were best sellers.
5) All use artifacts and folklore accounts as supportive evidence.
6) All are extensively cross-culturally comparative.
7) All were initially successful in times of mass strife, i.e., Atlantis, Late 1800's: Industrialization, Recession, "Panics"; Mu: The Great Depression; Extra-terrestrials: The Cultural Upheaval of the Late 1960's-early 1970's.
(Could it be that these works fill a psychological need to either think of better times, or be aware that even the great lands of the past are no more?)
8) All are written in an easy, questioning style, perfectly logical based upon the facts as presented.
9) All take the defensive, and chide the scientific community for not taking them seriously.
10) Each exploits an increasing amount of technological fantasy, according to their Chronology ("Mu" theory has more technological aspects than Atlantis theory, and Extra-terrestrial theory have more than Mu).
11) Extensive use of accredited sources, authors, etc.
   Ignatius Donnelly was the first to attempt a scholarly
work on the Lost Continent of Atlantis. In his book, *Atlantis: The Antediluvian World*, he compares everything from alphabets to body types, from myths to building structures. Cultural evolution was the most widely accepted theory at the time, and Donnely attempted to show "that Atlantis was the region where men first rose from a state of barbarism to civilization" (Donnely, 1971: 1). He also contends that the Egyptians, Mayans, the Phonecians, Babylonians, and Greeks were all "colonies" of Atlantis, and their "gods" were really rememberances of Atlantian rulers. Deluge stories were survivor accounts of its sinking, etc.

The two other theories amount to little more than modifications and expansions of Donnely's work, which shall be referred to as we progress.

The detail of Donnely's book is amazing. It would seem that every aspect imaginable is not only touched on, but significantly probed. Any similarities between any peoples is painstakingly traced back to its Atlantian origin, and the point-proposals mentioned at the beginning of the book are beautifully supported, in a well formalized and enticingly written work. The following point-proposal comparison is of great importance, because it can be seen that the prime directives of Extreme Cultural Diffusion have not changed over time.
Point Proposals of Unqualified Cultural Diffusionism:
A Comparison

Key:
A: Atlantis.
M: Mu.
E: Extra-terrestrials.

Point 1
A: Atlantis once existed, opposite the mouth of the Mediterranean Sea.
M: Mu once existed, in the Pacific Ocean.
E: Extra-terrestrials have visited earth many times, and are still doing so.

Point 2
A: The History of Atlantis is Verifiable.
M: The Story of the Land of Mu is Verifiable.
E: This Proposition is easily proven.

Point 3
A: The Rise of Human Civilization is Atlantian Based.
M: Mu was the center of Human Civilization.
E: The Extra-terrestrials taught man the ways of civilized life.

Point 4
A: Atlantis founded all other ancient cultures.
M: Mu founded all other ancient cultures.
E: The Extra-terrestrials lift colonies behind, to found cultures.

Point 5
A: The Atlantian Age was one of peace and prosperity.
M: The Land of Mu was devoid of strife.
E: While under the Extra-terrestrials control, life for man was idyllic.

Point 6
A: The Myths of ancient civilizations are stories and recollections of Atlantean Royalty and Heroes.
M: The same as "A", but substitute Mu for Atlantis.
E: Same, except Extra-terrestrial activities instead of Royalty and Heroes.

Point 7
A: Atlanteans were Sun worshipers.
M: Muvians were Sun worshipers.
E: Primitives believed the Extra-terrestrials were gods, since they came from and returned to the skys, this resulted in the reverence of heavenly bodies, particularly the sun.

Point 8
A: Atlantis' first Colony was Egypt.
M: Mu's first Colony was Atlantis.
E: Egypt was a by product of Extra-terrestrial Colonization and contact.
Point 9  A: Atlantis was the first place of Metallurgy.  
M: Mu was the first place of Metallurgy.  
E: The Extra-terrestrial taught men Metallurgy.

Point 10  A: Alphabets and Language are of Atlantian origin.  
M: Mu was the seat of literacy.  
E: Extra-terrestrials instructed the human animal in language and writing.

Point 11  A: Atlanteans were caucasoid.  
M: Muvians were caucasoid.  
E: There were many races of Extra-terrestrials, hence, today's racial diversity is to some extent a result of their contact.

Point 12  A: Atlantis perished in a terrible convulsion of nature.  
M: Mu was destroyed by natural disasters.  
E: Unhappy with the human species, the Extra-terrestrials used all means at their disposal to wipe them out, and used survivors to genetically rebuild the species.

Point 13  A: This catastrophe resulted in the Deluge Myths.  
M: This catastrophe resulted in the Deluge Myths.  
E: This catastrophe resulted in the Deluge Myths.

Point 14  A: Since all Mankind had central origins, it is no surprise that even today there are many similarities among peoples and their beliefs.  
M: Same.  
E: Same.

As should be evident from the proposal comparisons the Mu theories are little more than rehashings of the Atlantis beliefs. The main differences spring from the contention that Atlantis was a colony of Mu. Many ancient writings have been "translated" to "historical accounts" of Mu and its rulers. From this fundamental difference, many diversities are incurred, but their relevancy to this paper is negligible.

The most successful (on the popular level) of the extremists of our time has been Erich von Daniken. Through at least five books, he has championed the cause of "extra-terrestrial
visitation." And he uses the exact same style and technique that was used first by Donnely, and then by Churchward, of Lemurian fame and notoriety.

The financial success of von Daniken's books has spawned a deluge (sorry) of followers and imitators. Not only has there been a rash of books on lost Spanish (Portuguese, etc.) galleons, Oriental seafarers, Phonecian excursions, etc., but an extensive rebirth of Atlantis books. Fearful of imitators who would stain, poison, and taint his own work, von Daniken himself even refutes several of his own followers, for their "unjustified" hypotheses! (von Daniken, 1974: 115-169).

Apparently von Daniken believes that the best defense is a good offense, or has enough of his own detractors to contend with. As with the Atlantis and Mu schools, von Daniken is very good at taking facts out of natural context and re-interpreting them in a context all his own, and draws upon the points made by his predecessors.

The "Qualified Extremist" school exhibits two particular beliefs of interest: the theories of Grafton Eliot Smith (None of his books are now readily available, as a consequence most information on this subject has been taken from Perry) with W. J. Perry, and the hypothesis of Thor Heyerdahl.

It is Perry's contention that the bulk of civilization diffused from ancient Egypt. By the use of trait "clusters", he postulates his theory. Many maps, with extensive markings and keys are used to show the diffusion of Egyptian culture. Unlike the unqualified extremists, Perry doesn't maintain
that all traits have passed down basically unaltered. Throughout the book he explains how various aspects have been modified and changed, until they often seem very alien to their point of origin (Egypt).

It is interesting to note that the primary "tracer" used by Perry is evidence of sun-worship, which is also the first aspect followed by the Unqualified school.

Aside from supporting his theory, Perry attacks the concept of geographical determinism, and he does so quite well (Perry, 1923: 1-5). Unfortunately, Perry continually bases his assumptions upon the fact that complex cultures cannot spring-up overnight, and no evidence of previous cultures exists. With improvements in archaeological techniques and artifact dating the base of his theoretical pyramid has been ripped away, and the rest of his hypothesis collapses with it.

The clearest example of Qualified extremism is the work of Thor Heyerdahl. He has taken a defined conceptualization (early navigation by primitive peoples) and attempted to prove not only its possibility, but its probability as well. He uses the same variety of supportive data as the unqualified extremists, but he restrains from the over-use of aspectual factualization, although he does engage in this too frequently for most of the scientific communities comfort.

His book Early Man and The Ocean is his best, in every respect. But it too suffers from a little "tunnel vision." Heyerdahl attempts to prove an oceo-graphic network that interconnects all Atlantic societies together, and another
that interconnects all Pacific groups. As stated earlier, he uses the tools and methods supplied to him by earlier extreme diffusionists, and as such inherits their crippling deficiencies.

Throughout the history of Anthropology, extreme diffusionist thought has generally plagued the scene. While achieving popular notoriety, it has come close to disgracing its associated field. By the use of undiscriminating thought processes, practitioners of this school have fooled and thrilled not only a mass of unquestioning followers, but themselves as well. It is a sorrowful happening when bright, inquisitive minds are tethered by the desire to prove a single unsubstantiated idea. Extreme cultural diffusionism is such an idea. And the tragedy of its proponents lies not in the narrow patterns that they fell into, but in the wider ones which they did not.

Not surprisingly, the same means (to different ends) are used by almost all of the extremists. And it is their shoddy method that fails the extremists. But it is perhaps as Churchward said after all, "the facts are there. The theories will have to take care of themselves." (Churchward, 1931: 16).
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HUMAN EVOLUTION: AN ALTERNATE MODEL OF HOMINID SOCIAL DEVELOPMENT

by

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Man has long speculated about the evolutionary pathway taken by our hominid ancestors. The search for human origins has led many people to the farthest reaches of the world in an effort to determine what our early ancestors looked like and how they lived. The discoveries of human paleontologists have provided us with a fairly accurate idea as to what these early hominids looked like. Rather than solving problems, however, these discoveries have seemed to create a plethora of theoretical speculation and totally confuse the picture.

Speculations on the evolution of hominid social structure have become quite popular in recent years. The primary concern of these theorists has been the development of a model which would accurately depict the adaptive strategy of early man. This paper is a response to the models which have been proposed. I will concern myself with one model in particular: the baboon analogy.

The primary problem when dealing with hominid social development is to build a model which would enable the animal in discussion, to move from an arboreal to a terrestrial environment with enough flexibility to account for the further evolution of the species. These models are no more than mental exercises in that any conclusions reached are non-verifiable and usually serve only to justify or criticize the speculators views on modern human society. Nevertheless, these speculations have shaped the course of further investigations and should be examined very carefully before they are
accepted for theoretical generalization.

The baboon analogy has come into great favor in anthropological circles; so much so that it seems to have been accepted as a close approximation of the truth. This paper is an attempt to show that there are other, equally viable, models which can account for the fossil record and the idiosyncrasies of modern human society. An analogy accepted as explanation can be very damaging to future theoretical orientations. It is dangerous to put too much weight on such tenuous speculation. I will attempt to show that the adaptive strategy of the patas monkey can be used to explain hominid social evolution at least as well as the baboon analogy.

Baboon adaptation to a savannah environment has been studied in great detail for its possible implications to the study of human evolution. Irven DeVore and Sherwood Washburn were probably the first major figures to propose this analogy. Their paper "Baboon Ecology and Human Evolution" (DeVore and Washburn 1967) describes baboon ecology, demography and behavior and the implications which this study has for paleoanthropology. The authors feel that "the problems faced by the baboon troop may be very similar to those confronted by our ancestors" (DeVore and Washburn 1967: 159). The study of the baboon, therefore, could give us an idea of the adaptive choices available to the early hominids. This assumption is very reasonable and, in this sense, the study of the baboon does provide some interesting insights. It is only when this assumption is extended into a developmental model for Homo
Clifford Jolly (1970) develops the baboon analogy on the basis of morphological and dental similarities between *Theropithecus* and the early australopithecines. His construct is a two-phase model based on the small-object feeding pattern of the Gelada baboon. In this model, Jolly feels that *Australopithecus robustus* (or *Paranthropus*) is the more primitive form and that the changes evident in *A. africanaus* and *Homo habilis* are the result of cultural and technological developments and their corresponding changes in dietary preference and feeding behavior (Jolly 1970: 23-24). The author does a very good job in pointing out the weak points of other theories on hominid social development. The work of Johanson and White (1979), however, has placed a big question mark over Jolly's evolutionary sequence and, as a result, on his social evolutionary model as well. Johanson and White have discovered a hominid for which is morphologically closer to *A. africanaus* than to *A. robustus*, yet it is dated circa 2.9 to 3.8 million years ago (Johanson and White 1979: 327). If the implications of these findings are followed through, then the *A. robustus* form is seen as a later specialization of the generalized *A. africanaus* stock. If this is the case, the small object feeding pattern would be a secondary development of the hominid line and the *Theropithecus* analogy would not explain the morphological and dental adaptations of the early form.

The rigidly structured baboon analogy has been developed at the expense of other, less structured, adaptive solutions.
Most theories of hominid social development have described all biological and cultural evolution in terms of variation and selective retention. Christopher Boehm (1978) illustrates (once again using the baboon analogy) that rational preselection is a very important factor in all primates (including Homo sapiens) and that this factor, when applied over long periods of time, could significantly alter the course of evolution. I think that this factor has been ignored because it is a very difficult one to handle in the rigid structure provided by most models of social development. I will attempt to integrate this factor into my new model which will illustrate that alternate adaptive strategies must be considered when attempting to analyze the socio-cultural evolution of Man.

The baboon analogy provides one possible avenue of explanation for this socio-cultural evolution, but there is a number of problems which this analogy faces when confronted with the fossil record. Baboon society is built around the power of the adult males and the size of their canines. While this may be an oversimplification of the truth, it is at least a dominant factor in baboon society. The group has a distinct power ranking and is preserved by an active defense both internally, through attempts to maintain or change the status quo, and externally, against predatory attacks. Baboons are well equipped for this active aggression with a large set of canines. None of Man's known or hypothesized ancestors are known to have had such canines. If the baboon analogy is accepted, the weapon(s) for this inter- and intra-group aggression
must be extra-somatic. This leads to the question of how the early hominids survived while they were developing these defense mechanisms.

Ramapithecus is held by some as the earliest known hominid based on similarities in dental morphology with the australopithecines. Tattersall feels that "Ramapithecus constitutes the most convincing present evidence of the ancestry of Australopithecus and subsequent hominids..." (Tattersall 1975: 28). Ramapithecus does not have large canines adequate for defensive action of the type postulated by the baboon analogy. Even though Ramapithecus is considered to have been semi-terrestrial, one would expect some indication of an increase in the size of the canines between this form and the australopithecines. If Ramapithecus were to develop the adaptive strategy of the Savannah baboon with its known physical and dental limitations, it probably would not have made it very far from the trees. This, in my opinion, is one of the major flaws in the baboon analogy. The baboon analogy takes a highly specialized social organization and applies it to a very generalized form. Man's greatest evolutionary asset has always been his generalization. If we are to formulate a model of social and biological evolution based on the ethology of living forms, then we must attempt to find a system which is adaptable to a number of environments and applicable under the physical and morphological restrictions which we can deduce to have existed for the ancestral hominid stock.

If we accept Ramapithecus as the basal hominid, it must
be done on the basis of very limited evidence. Nevertheless, it seems to be a general agreement that the first hominid would be very similar to this form. It has been suggested that Ramapithecus was an arboreal or semi-terrestrial form (Tattersall 1975:29). This assumption would correspond to the commonly held assumption that our ancestors originated in the trees. The analogy chosen, therefore, must provide some indication as to the social system which existed in the trees as well as on the ground. It must also provide an explanation of the continuity in, or evolutionary changes of, the social traits which the analogy seeks to explain.

In this sense, the baboon analogy is found to be lacking. It may explain a possible adaptive strategy for life on the savannah, but it does not explain how this system arose and how it evolved into our present system of social organization and behavior. An analogy which begins its explanation at the midpoint of the problem is useless.

The social organization of the patas monkey (Erythrocebus patas) and the closely related Cercopithecus monkeys provides an analogy which avoids most of the problems mentioned above and yet it can still provide some interesting explanations for some problems which are not addressed by the baboon analogy.

Patas monkeys are large bodied, quadrupedal, terrestrial animals which have long limbs and slender torsos. There is marked sexual dimorphism with adult males being nearly twice as heavy as females (Bramolett 1976: 132). Some sexual dimorphism is also evident (although not always to the same
degree) among the arboreal guenons (Struhsaker 1969: 98). The patas monkey is so close to the guenons that it has been argued that *Erythrocebus patas* be sunk into the genus *Cercopithecus* as a sub-genera (Rosen 1974: 94). The patas lives in the open savannahs of the northern Sudan and East Africa. It relies on speed and diversionary tactics for protection rather than the large canines and aggressive behavior of the baboons whose range they overlap (Simonds 1974: 57). They live in small groups with large ranges which they roam in search of food. In terms of dietary preference, they are primarily omnivores (Rosen 1974: 96). One group has been reported to have occupied 5180 hectares and when food is scarce they may travel as much as 12 km (Bramblett 1976: 134). The primary patas groups are heterosexual with several adult females and young grouping with one adult male. Secondary groups of isolated males and all-male groups are also reported (ibid). This one-male group organization is used by all the guenons and will form the basis for my analogy. In the heterosexual group, the females form a strong ranking system among themselves. The females follow the male by choice with all group continuity occurring through the female. The mother lineage plays a prominent role in the patas group (Simonds 1974: 164-165).

The adult male acts as a scout or vanguard for the group. He is often found at the periphery of the group scanning the surrounding areas for any signs of predators. The patas habitually employ the bipedal posture to augment their range of vision in order to peer over the tall savannah grass (Napier
1970: 175). A much more complete description of the patas can be found in Hall (1965) and Struhsaker and Gartlan (1970) which, to conserve space, will not be given here. The one-male group can be seen as a continuum of lifestyles from the arboreal guenons to the terrestrial patas in a hypothetical evolutionary construct. The particular adaptive response chosen by the patas monkey will be applied to the hominid fossil record in an attempt to provide a logical construct which could explain some etiological factors of biological and social change. While running through the chronological sequence of human evolution, I will continually return to the patas example when describing particular aspects of development.

The dental evidence points to the conclusion that Ramapithecus was an omnivor and it has been hypothesized that it was a somewhat arboreal creature. The question of whether or not this form ate meat and used tools is still rather doubtful but some evidence to support these speculations does exist (Leaky 1977: 74). I believe this evidence to be very tenuous and will work with the assumption that Ramapithecus did not use tools and occasionally ate meat much in the manner of modern chimpanzees.

The change in habitat from forest to savannah was probably influenced by some change in the environment. The shrinking of the forest during the Miocene could have provided the prime mover for such an exodus (Volpe 1975: 591). The physical characteristics of Ramapithecus combined with the Cercopithecus one-male group pattern would have provided
enough flexibility to accomplish such a gradual transition without placing too much stress on the population. While the actual extent of the shrinking of the forests is impossible to determine, it is probable that this environmental change did have some effect similar to that hypothesized for early man (Leakey 1977: 67). This move from the trees took place over a long period of time and by a fairly large number of groups. Some of these groups would have gone on to develop the patas form of social organization and defense which, considering their physical limitations of small canines and relatively small body size, would have given them a better chance for coexistence with other savannah and forest border forms. Increased sexual dimorphism would be selected for as a large male would provide a better diversion and small females would have an easier time concealing themselves in the tall grasses. Although the fossil record is too incomplete to ascertain whether or not this increase in sexual dimorphism actually occurred, others have argued for the selective advantage of large males to human evolution in other contexts (Brace 1973). The patas males spend a great deal of time scanning the surrounding areas in a bipedal stance within their home range. In a group which is traveling and has no definite home range, selection would favor males which could spend more time in an

1 This article is a clarification of C. Loring Brace's position on sexual dimorphism within individual species of early hominids.
erect stance. The first tool may very well have been a walking stick which was occasionally used as a digging stick rather than a weapon.

Meat eating, which was being opportunistically pursued, could very well have become the preferred food for some of the groups which would then have developed some of the rudimentary hunting techniques. This meat eating preference would then become an additional selective advantage at a time of changing environmental conditions. It would enable a group to move into an area with differing floral and faunal assemblages without having to drastically alter their subsistence pattern.

During this period of territorial expansion and experimentation, the Ramapithecus form has gradually severed its connection to the trees, to a greater or lesser extent, and evolved into a completely terrestrial form; Australopithecus afarensis. This new form can also be divided into two groups based on dietary preference; the first, a group of opportunistic omnivores with rudimentary hunting techniques and, second, a group which has the basic omnivorous dietary pattern with a relatively greater dependence on local floral assemblages. This second group has also opted to remain close to the trees as much as possible while living a completely territorial existence.

The patas model depends on the speed of the individual males and the concealment capabilities of the females for it to work. Based on the fossil record, we can infer that the
australopithecines walked and stood almost as well as modern man (Le Gros Clark 1970: 74). Australopithecus africanus was probably capable of running quite fast (Robinson 1972: 245). A. africanus would develop from the first group; their smaller size and greater cranial capacity resulting from the continued dependence on speed and stealth for survival. Their constantly changing environment would force those groups which are on the move to remain flexible in their choice of adaptive strategy while continually providing new problems to be solved as a group.

The second group developed into Australopithecus robustus (or Paranthropus). This group was moving in the direction of greater dependence on the floral assemblage, possibly as a result of competition from A. africanus. It developed into a herbivorous form with incomplete development of erect posture which was never completely emancipated from the trees (Robinson 1972: 252). Its larger size probably results from its fairly sedentary lifestyle and moving into a gorilla type ecological niche. It is with the A. africanus form that I will primarily concern myself with from here on out as this is the form that I see developing into the genus Homo.

A. africanus developed the rudimentary beginnings of culture and language as an adaptation to a constantly changing environment. Culture has been called Man's major adaptive characteristic (Alland 1973: 270). In this sense, biological evolution is now augmented by cultural evolution. A rapidly changing environment would also increase the amount of genetic
variation and occurrence (Alland 1973: 69). Add to this a
developing cultural system and the chances for genetic change
surviving in a population are greatly increased because the
population has two systems working for the survival of the
group: biological and cultural.

Applying the patas analogy to A. africanus, the society
is divided into a primary heterosexual group and a secondary
group of all-male composition. When the females come into
estrous, the adult male becomes even more intolerant of the
juvenile males and it is, presumably, at this time that the
juvenile males are ejected from the group (Bramblett 1976: 136).
This group is similar in structure to the group structure
hypothesized by Freud and which he called a Primal Horde, as
the young males are forcibly ejected from the group and pre­
vented from gaining access to the females. The stress of
separation for the young males could very well produce some­
thing similar to the Oedipus Complex in a protocultural animal.
At this point, it becomes important to remember that the patas
male rules by consent of the females. If the females refuse
to go along with the male, he has no recourse but to accept
the fact or lose the group. If the combined factors of the
beginning of culture, increase in brain size, increase in
sexual receptivity of the females and the lengthening period
of child dependence on the mother made some high ranking females
want to keep their young, the male would have to accept or be
driven out. The choice to keep young in the group would pro­
probably not come about in the herbivorous groups because this
decision would have a minimal, or negative effect on the group. Brain size and the corresponding increase in the period of child dependency did not increase significantly in A. robustus. In an actively hunting society, however, the benefits of increased male participation in the hunt could easily help to influence the male's decision. Increasing female receptivity would make it harder for one male to service a large number of females and to prevent access to them at all times by outside males. The increase in group size would also ease the burden on the females as primary providers.

If this adaptive alternative were chosen by a few groups, their social structure would rapidly evolve into a family type of social organization as more and more males service fewer and fewer females. At this time, the incest taboo would become a formalized social institution as a holdover from the earlier form and as a way to maintain the new system with a minimum of internal stress. It is interesting to note that in our society, father-daughter incest is most prevalent, followed by sibling incest with mother-son incest by far the rarest form (Leaky 1977: 225). This pattern is similar to what would be expected to result from the gradual integration of males into the group. And whether the males realize it or not, they still rule the household only by female consent.

Some of these newly organized groups developed their meat-eating preference into the organized hunting of large animals. These groups have reached the stage of Homo habilis. At the time that this preference, and the corresponding hunting
techniques, were being refined, another major change had begun to take place in the environment. The climate was getting cooler and the flora and fauna had begun to change.

The *Australopithecus robustus* form, which most closely fits Clifford Jolly's Seed-Eater model, having specialized in a gorilla type ecological niche has developed a dependency on the local floral assemblage. Their dental and physical specialization has severely limited their capability for further adaptation. This limitation was further reinforced by the relative lack of cranial development. The assumption here is that the cranial development could have provided an additional adaptive outlet which could have allowed adaptation despite specialization. As the floral assemblage change radically, these groups gradually die out.

The *Australopithecus africanus* form has continued its omnivorous feeding pattern. The change in the flora and fauna would force this group to depend more on its sporadic hunting attempts than on the increasingly uncertain floral food supply. This increasing dependence on the meat supply would force this form into direct competition with *Homo habilis* which has developed the emphasis on organized hunting and the corresponding techniques and technology. Ecological theory predicts that when two forms are forced into competition for the same niche, the weaker form will either be eliminated or forced into a smaller portion of the niche (Smith 1976: 40). In either case, with a radical change in the environment taking place, the result would be disastrous to the weaker form; which, in this case,
is A. africanaus.

The form which has developed into an advanced australopithecine and which, for sake of convenience, I have followed Leakey in designating Homo habilis, would be able to survive a major change in the environment due to its megafaunal hunting emphasis. These groups, by following the herds, have learned to utilize the differing floral assemblages. The social organization of these groups at this time is similar to that described by Julian Steward as a composite band (Steward 1949: 23). The question of group composition must be addressed in terms of the patas model. There is some evidence that the patas are capable of intergroup cooperation. An account provided by Struhsaker and Gartlan of one all-male and three heterosexual groups at the Bodelaram waterhole will serve to exemplify this assertion:

Suddenly many patas began giving the chattering vocalization. A jackal ran off with a young juvenile patas in its mouth. It was about one-fourth the jackals size and was held by the nape. Three adult males gave chase. After 50m the jackal dropped the young patas, but the three patas continued chasing him for another 30-45 seconds. An adult female patas soon retrieved the young patas who was apparently unharmed. Another jackal was nearby but was not involved in the encounter. Although it could not specifically be determined which males chased the jackal, it is obvious and significant that at least two of them did not belong to the same group as the youngster who was attacked and yet participated in defending him (Struhsaker and Gartlan cited in Bramblett 1976: 157).
Evidence of such cooperation in such a purportedly inter-group-aggressive society indicates the degree of cooperation which is possible in a society in which the level of inter-group aggression is much lower and the survival value is directed towards cooperation. In such an instance, group consolidation and the intermingling of sexes in a family type social organization is possible. Selection would also favor inter-group cooperation in a case such as this where groups follow herds as their primary source of food. As the herds disperse throughout the expanding grassland at the beginning of the Pleistocene (Asimov 1975: 303), the hominid groups would become isolated from other groups of the species with particular groups is bound to occur.

By the time the groups had moved out of Africa, they had evolved into Homo erectus and had begun to use fire (Smith 1976: 62). At this point, a true family type of social organization had evolved through the continued interaction of the groups which followed individual herds. As the megafaunal herds began to disappear, the composite bands began to divide into patrilineal bands which began to exploit small localized environments. In this way, the hunter-gatherer societies similar to those we study today were formed.

The evolutionary scheme presented here is basically multilinear. As the groups moved further from the Ramapithecus stock, they began to utilize different adaptive strategies. I have followed one of these strategies here; a strategy which, I feel, could have resulted in modern human society. Not all
groups would have chosen the same solutions at any particular level of evolution. It is only important that some groups at each stage develop new solutions to the old, and new, problems which would then start them off on an evolutionary pathway of their own.

The patas analogy is an example of one method of survival on the savannah which seems most in harmony with the fossil record. The development of hominid ancestry can be traced from a Ramapithecus-like form through the australopithecines and on into the genus Homo with very little loss of continuity. This analogy also provides an explanation for the extinction of the australopithecine forms.

In conclusion, the patas analogy is better than the currently accepted baboon analogy. It is an alternate model for hominid social development which could provide a better model for describing evolutionary process. It is probably one of many which were actually utilized by Man's ancestors. The emphasis here, though, is that it is just another way of looking at the same information and that there are other possible ways of describing the social development of Man based on the fossil record.
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