

SOCIAL SCIENCE, SERVING BOWLS AND THE QUESTION OF ETHNICITY:

Deconstructing Material Culture Correlates of Ethnic Identification

A Thesis

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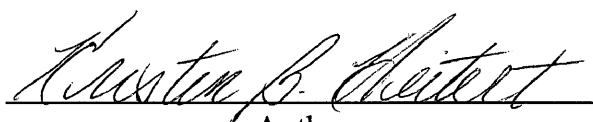
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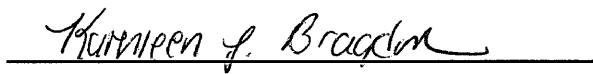
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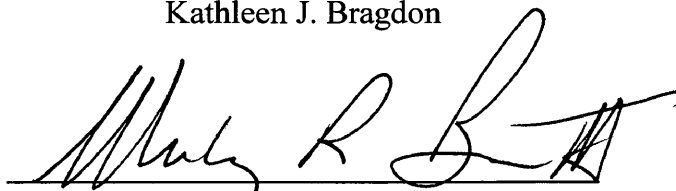
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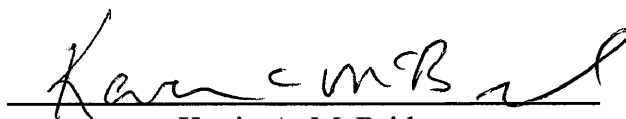

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ABSTRACT

The concept of one-to-one associations between specific items of material culture and specific ethnic groups has been met with a great degree of enthusiasm among historical archaeologists, particularly those engaged in African-American archaeological research. There has been comparatively little methodological evaluation, however, of the construct of “ethnicity” as an active agent in shaping material culture patterns. This oversight has resulted in tautological arguments rooted in biased data selection and unrecognized logical errors in the hypotheses designed to ask questions concerning ethnicity and material culture. Using formal models of social scientific theory construction, I will address this problem through the methodological “unpacking” of ethnicity as it is used in archaeological analysis, particularly as applied to South’s pattern analysis model, with specific examples taken from Vernon Baker’s seminal study of Black Lucy’s Garden in Andover, Massachusetts, and John Otto’s monograph on Cannon’s Point Plantation in Georgia. I plan to make suggestions for the qualitative expansion of the archaeological data base, using socio-economic as well as ethnic criteria, to provide a more rounded picture of material culture patterning. I will also make recommendations for an increased specificity in historical and social research as a means to grasp more tightly the concept and process of ethnic identification.

**Social Science, Serving Bowls and the Question of Ethnicity:
Deconstructing the Material Culture Correlates of Ethnic Identification**

Introduction

This paper is an attempt to clarify some of the issues surrounding current attempts to delineate African-American ethnicity in the archaeological record. Given the amount of theoretical attention that historical archaeologists have focused on the subject within the past two decades, there has been surprisingly little methodological evaluation of the use of “ethnicity” as an active agent in the formation of material culture. This inattention has led to poorly constructed theories tested with even more poorly executed methodological questions, and has obscured the nature of ethnic identification and the processes which shape it.

Chapter I examines the definition and role of “science” in archaeology and how the relative rigidity or flexibility of its interpretation structures archaeological inquiry. In Chapter II, historical archaeology is firmly embedded as a “social science” and a basic glossary of terms and applicable methodologies for the construction of strong social scientific theory is provided.

Chapter III reviews the anthropological concept of ethnicity and many of the theoretical confluences and contradictions subsumed by the term, particularly as it applies to African-American identity. The use and abuse of South’s pattern

analysis in ethnic interpretation is addressed in Chapter IV, with a specific emphasis on its theoretical misuse in relation to culture versus culture process, and how that misuse has been extended to incorporate the process of ethnogenesis. In Chapter V, all of the above information is brought to bear on the analysis of Vernon Baker's archaeological interpretation of Black Lucy's Garden, an African-American site in Andover, Massachusetts and John Otto's monograph on Cannon's Point Plantation. The problem with the theory of unique African-American behaviors as reflected in unique ceramic patterns is addressed through the "unpacking" of ethnicity as an explanatory concept and demonstrates how the resultant co-variation of economic and racial variables render the theory untenable. Suggestions for improved theory construction through a better rounded, less biased data base are also provided. Chapter 6 provides a cautionary look at the role of economics in defining ethnicity and recommends increased specificity in historical and social research as a means to understand better exactly what is represented by the concept and process of ethnic identification.

In the final analysis, this paper will raise more research questions than it can answer about the nature of African-American ethnicity and how it is best identified in the archaeological record. I hope that through the examination of social scientific theory construction and the methodological problems which it helps to illuminate, it will provide a cogent and clearheaded means to circumvent those problems.

Chapter I. Historical Archaeologists: “Scientists” or “Aimless Balladeers”?

For well over a generation an archaeological debate has raged as to the relative merits of *processual*, or scientific, method and theory versus *post-processual*, or interpretivist, models of archaeological explanation. The analysis of “ethnicity” in the archaeological record, particularly that of African-Americans, has fallen prey to this dichotomous situation. Historical archaeologists are caught between, on the one hand, a desire for a deep “interpretivist” discussion of the roles of Africans in the New World (Yentsch 1994; Ferguson 1992), and, on the other, a more rigorous, “scientific” approach to the illumination of those lives through the use of pattern analyses, sherd counts and statistics (Otto 1984; Armstrong 1990). Unfortunately, as Posnansky (1989) and Sanford (1996) have pointed out, we have a long way to go as anthropologists before we can consider ourselves even nominally “immersed” in the culture and history of Africans and African Americans. Furthermore, as scientists, our attempts to understand manifestations of ethnicity in the archaeological record through the formulation of clear, determinate research designs, the hallmark of good scientific research, have provided questionable results at best (Otto 1984; Baker 1978; Zierden and Calhoun 1983).

This situation is largely the product of the polemics surrounding the intellectual status of archaeology, generally, and historical archaeology, specifically. As the argument is familiar to anyone working in the field, it requires no further elaboration here; it is worth noting, however, that much of this tension is aggravated by an overly rigid understanding of the word “science” and its application.

Two basic working assumptions of practicing scientists are that there is a real, knowable world that can be empirically perceived and described and that the empirically observable behaviors of the entities making up the real world is orderly (Watson et al. 1984: 3).

Archaeological Explanation, written by Patty Jo Watson, Steven LeBlanc and Charles Redman, provides the most explicit discussion available on the role of scientific method and theory in archaeology. The authors charge archaeological analyses with the role of *explanation*, which they define as the demonstration that a particular case one wants to explain is an example of general relationships described by an established general law. Science, they argue,

is deterministic and scientists assume that general laws can be confirmed that will allow the explanation and prediction of the behavior of all empirically observable phenomena (Watson et al. 1984:4).

This definition is expanded by adding that

Random variation is only that portion of the world for which we have no

explanation. The division between systematic and stochastic variation is imposed by the analyst and depends on what explanatory variables are available and included in the analysis. Given the right explanatory variables, the world is entirely predictable (King et al. 1994:59).

While *explanation* certainly must remain the primary goal of archaeology, it does not follow that so rigid a definition of science as that cited by Watson et al. necessarily must govern such explanation. Arguably, this definition of science envisions a world in which all human behavior is predictable, and that element of behavior which is unpredictable is only unpredictable insofar as that proportion of its unknown constituent variables. Human agency is lost in this picture and the notion of a *generalized, creative, non-systematic predictability* of variation and change in human culture is subsumed under a model of *specific, routinized, systematic predictability*.

Gary King, Robert O. Keohane and Sidney Verba offer another option to the deterministic model of science in their book, *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Their model allows for the existence of a probabilistic world, one in which

Random variation exists in nature and the social and political worlds and can never be eliminated. Even if we measured all variables without error, collected a census (rather than only a sample) of data, and included every conceivable explanatory variable, our analysis would still never generate

perfect predictions. A researcher can divide the world into systematic and non-systematic components and often improve on predictions, but nothing a researcher does to analyze data can have any effect on reducing the fundamental amount of non-systematic variation existing in various parts of the empirical world (King et al. 1994:59).

While this definition may read as the counter to Watson's notion of science, perpetuating rather than reconciling the processual/post-processual, science/humanities dichotomy, it is rather more flexible in its applicability. King et al. endorse a systematized collection and organization of data rooted firmly in the "scientific" tradition, but tacitly accept the notion that the interpretive and predictive powers of this data will always be a variable function of the specific and unique characteristics of the time and place from which the data were collected.

This is not to suggest that every event, archaeological or otherwise, is absolutely unique or, more properly, not amenable to some level of predictive generalization. If this were the case, than *postdiction* (Watson et al. 1984:6), or predictions about the past, would be impossible and historical archaeology in fact would be nothing more than "a mere handmaiden to history" (Noel Hume 1975:3). Indeed, as one historian has commented,

if the emphasis on "uniqueness" is carried to the extreme of ignoring all regularities, the very possibility of social science is denied, and historians are reduced to the aimlessness of balladeers (E.L. Jones in King et al.

1994:43).

The word “historian” only need be replaced with “historical archaeologist” for the sentiment to apply with equal force to our own discipline.

It is ironic that many social scientists, but most remarkably archaeologists, have set for themselves a sterner task in the definition of science than do most “hard” scientists. Anthony O’Hear has remarked of the physical sciences that

...we have to insist that proposing and testing universal theories is only part of the aim of science. There may be no true universal theories, owing to conditions differing markedly through time and space; this is a possibility we cannot overlook. But even if this were so, science could still fulfil[sic] many of its aims in giving us knowledge and true predictions about conditions in and around our spatio-temporal niche (O’Hear 1989:43).

Historical archaeologists must contend with the complexities of an infinite variety of spatio-temporal niches, each contingent on any number of internal and external variables. Out of these possibilities they must try to extract some generalizing principle so that meaningful questions can be asked of the archaeological record. Even Watson et al.’s belief in a deterministic and entirely predictable world is confounded by the notion of variation, although the concept is not acknowledged explicitly. The authors comment

in practice, one can usually find reasons for concluding that some “In C, if A, then B” assertions express deterministic or causal connections, while

others reflect only contingent or accidental relations (Watson et al. 1994:7).

The challenge is to devise a method to distinguish between “accidents” and “causalities” in the archaeological record, knowing what represents a meaningful relationship between artifact and behavior and what represents only coincidence, contingency or irrelevancy.

Chapter II. The Promise of a “Social Scientific” Approach

Historical archaeology is understood (professionally, if not popularly) to operate within the realm of the *social sciences*. Yet despite this disciplinary self-recognition, there seems to be little or no stated consensus as to what exactly the methodology of historical archaeology as a social science should be. Max Weber’s 1949 classic, *The Methodology of the Social Sciences*, provides a comprehensive examination of the philosophical underpinnings of social scientific inquiry, namely “ethical neutrality”, “objectivity”, and the “logic of the cultural sciences”, but provides little in the way of practicable methodological guidance. Watson et al. in *Archaeological Explanation* eschew the term *social science* in favor of the word *science*, leaving the reader to wonder how archaeology differs in its aims and methods from mechanical engineering or podiatry. Introductory texts on archaeology may or may not refer to the field as a social science, and those that do invariably fail to offer any concise definition of the term (e.g. Barnouw 1989:11-13; Fagan 1996:2; Meighan 1966:1-5; Renfrew 1996:11-13).

It is only in turning to the Oxford English Dictionary (1989), that a clear definition can be found. *Social science* in the OED is defined as

The scientific study of the structure and functions of society; any discipline

that attempts to study human society, either as a whole or in part, in a systematic way

in distinction to *science* which is defined as

A branch of study which is concerned either with a connected body of demonstrated truths or with observed facts systematically classified and more or less colligated by being brought under general laws, and which includes trustworthy methods for the discovery of new truth within its own domain.

The point of this seemingly hair-splitting semantical distinction is to suggest that in the absence of an explicit definition of archaeology as a social science, a science expressly concerned with the study of **human society**, a critical aspect of theory construction in archaeology is often overlooked.

A fundamental truth of the nature of knowledge and the process of inference is the notion that all conclusions derived from the scientific method are inherently uncertain. This precept, also called *the fundamental problem of causal research* (King et al. 1994:79), acknowledges the fact that we will never know a causal inference for certain as any theory designed to show a causal relationship is a distillation of systematic and non-systematic variables from an infinite universe of such variables.

By failing to stress the **human** aspect of archaeological inquiry, that infinite universe of possible variables is methodologically truncated. *Archaeology as science* potentially reduces individual and societal variation as laundry list of

utilitarian wants or needs. A.M. Hocart commented that, “Utility dominates the study of culture because it dominates the culture that studies” (Sahlins 1978:36) and the truth of this criticism is evident in many “settlement system” and “resource procurement” models within the New Archaeology paradigm.

This is more than a mere intellectual point. The recognition of *archaeology as social science* is vital to the construction of coherent social scientific models, particularly in historical archaeology. If we accept that the goal of archaeology is the development of theories of explanatory inference about the past, and if the reduction of uncertainty in those explanatory inferences is the primary substantive improvement to such theories, than human behavior, in all of its manifestations, must be addressed specifically in order to control for and predict patterns in the past. A rigidly scientific approach to archaeology often amounts to a reductionist, determinist perspective on human behavior. This perspective on culture and the archaeological remains of culture is not only intuitively false, but can lead to profound errors in research methodology and, subsequently, profound errors in the theoretical conclusions derived from those methodologies.

As Ferejohn has pointed out,

We want social science theories to provide causal explanations of events... (and) to give account of the reasons for or meanings of social action. We want to know not only what caused the agent to perform some act but also the agent's reasons for taking the action (Ferejohn 1993:124).

Barbara J. Little, in her 1994 article, *People with History: An Update on*

Historical Archaeology in the United States, makes that point that methodology remains the primary stumbling block to the process of asking “questions that count” in historical archaeology. She argues that while there are many examples of procedural or technical methodologies which serve to structure the current questions being asked in the field, there is a dearth of method informed by theory that is structured to ask those questions in a unique way (Little 1994:13).

I would backtrack a bit and suggest that a critical reevaluation of much of the methodology currently being used to answer some of those questions would be of great utility. This re-evaluation is most profitably approached at the level of basic social scientific theory construction.

Many of the long-lived and deep-rooted problems in historical archaeological research are the result of tautological arguments created and perpetuated by indeterminate research designs. A *research design* is a plan that shows, through a discussion of a model and its constituent data, how one can expect to use the collected evidence to make inferences concerning a specific problem (King et al. 1994:118). It is the process of arriving at a descriptive inference through a well-executed research design that is the ultimate goal of social scientific theory.

Descriptive inference is the method of understanding an unobserved phenomenon on the basis of a set of observations, and relies on the identification of *systematic* and *non-systematic* factors present in the phenomenon or set of

phenomena that the researcher is attempting to understand (King et al. 1994:34). An *observation*, in this context, refers to one measure on one unit for one *dependent*, or outcome, variable and includes information on the values of the *explanatory* variables, which can encompass *control variables* and a *key causal variable* (King et al. 1994:117).

Related to, but often erroneously interchanged with, the notion of descriptive inference is the concept of *causality*. Causal theories are designed to show the causes of a phenomenon or set of phenomena by positing relationships between assigned variables that create observable implications (King et al. 1994:77, 99-100). Causality and causal theories are part of the process of descriptive inference and, as theories, are constructed to be tested using either inductive or deductive methods. Unfortunately, much of the implementation of causal theory concerning culture process in the archaeological record stops short of rigorous testing and many of the hypotheses which constitute the theories are structurally unsound due to imprecise understandings of the rules for constructing causal theory.

There are five fundamental rules in the construction of causal theory. While many of them are basic, historical archaeologists' lack of adherence to these rules has contributed greatly to the incoherence within the field concerning pattern analysis and ethnicity and the supposed causal links between the two.

The first rule is always to construct falsifiable theories (King et al.

1994:100-105). A researcher should always ask what evidence would falsify a given theory, and recognize that the process is designed to ensure that the theory always remains tentative and does not become dogma. Karl Popper, a pre-eminent philosopher of science, went so far as to suggest that the verification of a theory is almost irrelevant to its falsification (Popper 1968:252). While it certainly is true that no theory is completely verifiable as one can never test all of the possible observable implications of the theory, the evaluation of any theory should not be merely the attempt to falsify it. Nearly every social scientific theory is falsifiable on one level or another, but the process of falsification should be viewed as a means to define the applicable bounds of the theory, not as grounds for its immediate dismissal.

The second rule in constructing causal theory is to build theories that are internally consistent (King et al. 1994:105-107). Inconsistencies, defined as the generation of two or more hypotheses which contradict one another within a single theory, render the theory not only falsifiable but unequivocally false. When this situation occurs, there is no amount of evidence from the empirical world which can support the claims made by the theory and it should be tossed out.

Third, the selection of dependent, or outcome variables, should be made very carefully. The dependent variable always should be, by definition, dependent, meaning that it does not cause changes in the explanatory variables, a common problem in qualitative research design known as *endogeneity* (King et al.

1994:197-198).

As a hypothetical example of this problem, suppose an archaeologist wishes to *postdict* the likelihood of armed conflict among Native Americans and Euro-Americans based on the relative number of gun-smithing sites across a range of colonial townships. The explanatory variable in this formula would be the number of gun-smithing sites within towns of equal size and economy; the dependent variable would be the presence/absence of armed conflict between English colonists and the local native populations. The problem with this approach is that the number of gun-smithies within a given town is not necessarily a random phenomenon, but represents a self-assigned variable, a variable influenced by a pre-existing, town-wide perception of the threat of native attack. In other words, the anticipation of conflict by a colonial community, in essence the dependent variable, directly affected the number of guns and gun makers, the explanatory variable, that the community chose to support.

A corollary to the point of endogeneity is the maintenance of *conditional independence*, the assumption that observations are chosen and values assigned to the *explanatory* variables independently of the values taken by the dependent variable (King et al. 1994:115). If the explanatory variables are chosen by criteria that are correlated with the dependent variable, than the theory is unsound.

Consider the example of a site settlement analysis of 17th-century Native Americans in southern New England in which an archaeologist wishes to

demonstrate the effect of segregated living patterns between Pequot Indians and European colonists on the Christian conversion rates among the Pequots. The conditional independence assumption would be violated if the archaeologist only examined the association between two variables, Pequot site location in relation to European settlements and conversion to Christianity, to determine their causal relationship. The proposition that Pequots who lived among and interacted daily with the larger white community converted to Christianity versus those Pequots who lived in isolated, insular villages and maintained traditional worldviews may be confusing physical location with ideology. The reason for this is that those natives who settled among the Europeans may have done so out of a *conscious choice* to convert to Christianity as opposed to those natives who chose to settle away from European centers as a strategy to preserve their traditional belief systems.

In this instance, what the researcher may be measuring are degrees of ideological resistance/acceptance among Pequots to Christian conversion efforts rather than the effect of presumptively random Pequot settlement patterns on that conversion. The effect of location could be addressed, in theory, if sites which comprised Pequot communities with roughly similar attitudes toward Christian conversion could be located in both segregated and de-segregated contexts, and the attendant conversion rates then measured.

The selection of observations based on the dependent variable so that the

dependent variable remains constant is also an obvious source of bias to be avoided (King et al. 1994:108). When a researcher chooses a particular phenomenon to study and chooses only those observations in which the dependent variable reflects that phenomenon, than nothing substantive can be learned of the actual causes of that phenomenon. For example, if an archaeologist wished to understand the environmental factors which encouraged limited horticulture among Late Archaic Native American populations in the Northeast, but chose only those sites on which farming was known to have been practiced, than those associated environmental conditions are of little predictive value. Of more value would be to select sites where horticulture did not occur but where the prevailing environmental factors were similar to other sites where the phenomenon did develop. In this way, the archaeologist may reassess the explanatory variables within the hypothesized causal relationship between environment and horticulture and generate more fine-tuned theories as to the relationship between the two.

Finally, the dependent variable chosen should be representative of the variation one wishes to explain (King et al. 1994:108-109). Again, an obvious point, but in the process of any research design it is valuable to take a step back and make sure that it is, in fact, the variation of the dependent variable which is of interest and not the background factors that the design holds constant. Returning again to the Pequot example, suppose an historical archaeologist wanted to explain patterns in the material culture remains of Christianized Pequots; namely, why

there appear to be such profound differences in the levels of material consumption from household to household within a single community. Such criterion as family structure and size, employment levels, and accessibility to external markets may all be taken into account to explain the observed discrepancies and hopefully would provide a workable inferential model for the analysis of future assemblages. If, however, the effect of the presence/absence of Christian conversion on the material culture assemblages was the explanatory variable of interest, then the design would be flawed as that variable would not vary within an entirely Christianized native community. In order to assess the effect of Christianization on native assemblages, non-Christianized communities similar in all other socio-economic respects would need to be included in the data base.

Fourth, in constructing a causal theory it is always important to maximize the concreteness of the theoretical question being asked (King et al. 1994:109-112). This requires choosing observable, precise concepts rather than unobservable, vague concepts whenever possible - the mortal sin of tautology looms large when fuzzy dependent variables are predicted by even fuzzier explanatory variables. Among the more imprecise concepts listed by the authors of *Designing Social Research* is “culture”, a situation which poses an obvious problem for archaeologists. Abstract concepts such as “culture”, “ethnicity”, or “status” render explanatory theories suspect unless those concepts can be measured independently of the dependent variable one is trying to explain.

Realistically, however, unobservable constructs such as “culture” or “ethnicity” often are used in the construction of causal theory in archaeology, as these are most often the anthropological issues in which archaeologists are most interested. Consequently, *indicators*, in the form of artifacts, are selected as observable manifestations of the concept under examination. That there exists a gap between concept and indicator is inevitable - it is only when “the indicator, which may only bear scant relation to the concept if any relation at all, is reified and labeled with the abstract concept itself” (King et al.1994:111), that grave theoretical problems arise. The relationship between concept and indicator is not, in and of itself, at fault, but the degree of association that a researcher assigns between the two may be. A careful and consistent theoretical distinction between concept and indicator is necessary to maintain a viable theory.

Concreteness also should extend to the words used to describe a theory. Both the description and implication of the theory should be stated in precise language which, ideally, leaves little room for interpretation or ambiguity. As James Deetz noted in *Flowerdew Hundred*, “it is often better to risk being wrong than to retreat into timid equivocation” (Deetz 1993:45).

Fifth, and last of the rules for causal theory construction, is the principle that theories should be stated in as encompassing ways as possible. Those systematic features of the theory that it make applicable in other areas should be stated and then, of course, the theory should be tested in all of its applications. So

long as the theory remains testable, than the broader the better; that is, the broader, the greater the leverage of the theory over the stated problem (King et.al. 1994:113-114).

Maximizing leverage, or explaining as much as possible with as little as possible, is one of the keys to developing strong theory. The principle of leverage, as distinct from the more common term *parsimony*, provides a uniquely productive perspective from which to evaluate social scientific theory.

Parsimony, as defined by Jeffreys (1961:47), posits that simple theories have higher prior possibilities. Parsimony, therefore, implies a fundamental judgement or assumption about the nature of the world, namely that the world is a simple place (King et al. 1994:20). Unfortunately, the social sciences deal with anything but a simple world - the mission of most social science is to reduce the world's massive complexity into interpretively manageable units.

Leverage, or explanatory potential, acknowledges complexity and is more appropriate to social scientific research. Leverage requires only a parity of explanation and prediction, that statements of explanation are logically equivalent to statements of prediction in any given case (Watson et al. 1982:5). Whether many explanatory variables are necessary to meet this condition or whether true parsimony can be achieved merely indicates whether the researcher has a lower or higher degree of leverage over a particular problem (King et al. 1994:104-105). Exceptions added to a theory are acceptable, but reduce the leverage of the theory

to a given problem. Too many exceptions and a “theory” may become little more than a useless composite of exceptions and exclusions - at that point it may be necessary to discard the theory in its entirety.

These five rules are designed to safeguard against the construction of *indeterminate research designs*, designs from which virtually nothing can be learned about the causal hypothesis (King et al. 1994:118). A major source of indeterminate research design lies in the biased selection of observations with which a causal theory is constructed. Ideally, the best “intentional” design selects observations to ensure variation in the explanatory variable, as well as any control variables, without regard to the values of the outcome variable (King et al. 1994:140). Observation selection and strong theory construction go hand in hand, and often a deficiency in the former leads to in the latter. There are cases, however, where selection bias is far more subtle in its implications, offering no direct obstacle to theory building but nonetheless rendering a theory logically inconsistent. As King et al. warn,

since selection criteria in qualitative research are often implicit and selection is often made without any self-conscious attempt to evaluate potential biases, there are many opportunities to allow bias subtly to intrude on our selection procedures (128).

Multicollinearity, a term borrowed from statistical sciences, occurs when one explanatory variable can be perfectly predicted from one or more of the

remaining explanatory variables (King et al. 1994:122-124). Often multicollinearity occurs, whether by design or chance, when a small data set is used and two or more causal hypotheses, which may be conceptually distinct from one another, are perfectly correlated. Causality is confounded in this situation as the two component hypotheses meant to test the observable implications of a causal theory cannot be analytically separated from each other. For example, suppose an archaeologist hypothesizes that 1) 18th-century Native Americans are more likely than 18th-century whites to structure their diets around venison and 2) People of limited economic means are more likely to have a venison-based diet than those with more money. The observations available to test these hypotheses, however, consist only of 18th-century sites known to be occupied by poor Indians and, conversely, sites known to have been occupied by wealthy whites. In this instance, the causal effects of the hypotheses cannot be meaningfully assessed as they cannot be separated given the limitations of the data base.

Another error in qualitative research design is the selection of observations on both the explanatory and dependent variables so that the two vary together in ways that are known to be consistent with the hypothesis that the research purports to test (King et al. 1994:142). This is not necessarily a deliberate or dishonest attempt to skew causal inferences in one direction or the other. More often, it is a result of a circumscribed data base, particularly if most of the available data, or observations, are related to the dependent variable (King et al. 1994:132).

Returning to the example above, if an archaeologist hypothesizes that 18th-century Native Americans were more likely to consume venison than 18th-century European Americans, but gathers all data from an isolated, traditional, rural Reservation context due to a lack of excavated sites elsewhere, than any conclusions drawn from that data will be flawed. Arguably, the socio-economic and political context of the sites within that Reservation, by definition a Native American community, will skew predictions favorably toward the researcher's proposed hypothesis.

Omitted variable bias is one of the most common errors in research design and one of the more dangerous. In dealing with a complex world and in attempting to causally explain that world with the most leverage, many social scientists attempt to introduce the fewest number of explanatory variables into their theories as possible. While this is an important consideration, a theory has little explanatory force if the omission of critical variables can produce substantive changes in the hypothesized causal relationship. In evaluating an explanatory variable for inclusion or omission, omission is obviously feasible if it has no effect on the outcome variable. Likewise, an explanatory variable can be omitted safely even if it does have a strong effect on the dependent variable so long as it does not co-vary with any of the other included explanatory variables (King et al. 1994:169). Co-variation within a single hypothesis is similar in effect to multicollinearity between hypotheses, and similarly confounds causal theory

construction.

Back to the venison example, let's say that a certain archaeologist proposes that 18th-century Natives relied heavily on deer meat in their diet as evidenced by a predominance of deer bones in 18th-century Native faunal assemblages. While this seems to be an assertion supported by the facts, another look at the data by a second researcher uncovers a flaw in methodology. Upon careful re-examination, the researcher finds that while, indeed, there is a predominance of deer bone in the faunal assemblages, there seems to be a bias in the data set. A majority of the sites containing appreciable faunal assemblages were excavated without the use of screens. In this case, the omitted variable of recovery method strongly biased the results toward larger, more visible deer bones to the exclusion of smaller, less easily recovered bones such as fish or bird and thus biased the conclusion of the primacy of deer meat in Native diets.

Research design and causal theory construction are as critical to archaeological research as they are to any other social scientific discipline. The analysis of the relationship between artifacts and the people who produced them is both a quantitative exercise (sherd and minimum vessel counts, pipestem formulas, mean ceramic dates), and a qualitative exercise (What meaning did particular vessels have to an individual or society? How did tobacco impact socio-economic relationships in Virginia? When did capitalism start exerting observable influences on the material culture record?).

Of particular interest to historical archaeologists in recent years is the role of ethnic or racial self-identification in the patterning of material culture. The manifestation of *ethnicity* in the archaeological record has become largely an a priori assumption and is believed by many to “mark” archaeological assemblages indelibly. There are two major complications with this idea: first, many of the causal hypotheses advanced to test the causal theory of ethnicity as a creative force of distinctive archaeological patterning are fraught with internal contradictions and faulty hypothesis construction; second, the concept of ethnicity itself is defined poorly and is often non-reflexive. It is precisely the nature of ethnic identification or, better yet, the lack of precision in the concept of ethnic identification, which leads to indeterminate research designs, tautological arguments, anthropological presentism, and historical misrepresentation.

Chapter III. Ethnicity a la Carte

Anthropologist Ernest Barth defined the concept of an ethnic group in 1963 by stating that,

A categorical ascription is an ethnic ascription when it classifies a person in terms of his basic, most general identity, presumptively determined by his origin and background. To the extent that actors use ethnic identities to categorize themselves and others for the purpose of interaction, they form ethnic groups in this organizational sense (Barth in McGuire 1982:160).

It is this definition of an “ethnic group” that implicitly informs much of the theory and research on the subject in historical archaeology. With this definition, historical archaeologists not only attempt to discern patterns of ethnic interaction in the past, but also interact with the past on a personal level. This personal interaction reveals itself in the form of late the 20th-century conflation of race, economics and ethnicity and the unreflexive assumptions this conflation creates which then are used to interpret the archaeological record.

At the root of this archaeologically expedient concept of ethnicity is the cultural anthropological debate between ethnicity as a *primordial* categorization versus an *instrumental* categorization. Primordialism suggests that ethnicity is an

innate aspect of human identity, so intrinsic to the human cultural sphere that it should require no explanation, only description, and serves the psychological function of giving individuals a sense of identity as members of a discrete group (Banks 1996:39). A.L Epstein is a strong proponent of this position in his work *Ethos and Identity* (1978), in which he characterizes an “ethnic identity” as a “terminal identity”, an identity

that embraces and integrates a whole series of statuses, roles and lesser identities (Epstein 1978:101).

Primordial ethnicity is, in large part, a response to the more pragmatic and situationalized concept of instrumental ethnicity, a perspective advanced by Abner Cohen in his work on the Ibadan Hausa of Yoruba, *Custom and Politics in Urban Africa: A Study of Hausa Migrants in Yoruba Towns* (1969). For Cohen, the assertion and maintenance of ethnic identity is undertaken for economic and political purposes and does not exist as an *a priori* set of values and customs. Instrumental ethnicity is goal-oriented and “motivated. It comes into being for a purpose and its continued existence is tied to that purpose”(Banks 1996: 39).

Historical archaeologists have latched onto the primordialist view of ethnicity for obvious reasons. Primordialism implies a least common denominator in the cultural analysis of different groups, a synchronic denominator which archaeologists enumerate as a static laundry list of material culture. This presumed stasis is a seductive analytical tool in that it reflects the inherently static

nature of the archaeological record. Staticity, however, was never advocated by the original proponents of the concept. Philosophically, primordialism suggests only that individuals possess what Epstein referred to as “cognitive maps” (Epstein 1978:11) which structure the inherent disposition of individuals to group into ethnic categories as a means to develop a corporate identity. The exact parameters of that identity are situational, bounded and defined by the corporate identities of other groups, and while they may remain stable for certain times and in certain places, it is those times and places which are instrumental in creating the parameters and as such must be incorporated into any analysis of ethnicity.

One issue which, for many years, was not addressed directly in anthropological research is the critical distinction between the analytical conceptual tool of *ethnicity* as defined through primordial and instrumental explanations and the observable reality of *ethnic groups*. This distinction is, in essence, the etic versus emic tension that has cross-cut anthropological research for decades.

Anthropologists, as noted above, have long debated the genesis and nature of ethnicity as a means to sharpen the concept as a research tool (Warner and Lunt: 1942; Barth: 1969; Zenner: 1985; Yelvington: 1991) . More current research, however, has begun to question the validity or usefulness of ethnicity as an etic academic perspective in the analysis of cultures which develop their own emic folk concepts of ethnic groups.

Karen Blu, in her research with the Lumbee Indians of Robeson County,

North Carolina (1980), has commented that the notion of ethnicity serves as an “external”, esoteric form of research which does little to illuminate the internal and external dynamics of the people it attempts to categorize. Blu suggests that the concept should be discarded in favor of the notion of *ethnic identity*, a native category, one which is believed to be immutable and fixed by those who define and group themselves under that rubric even if the academic definitions of ethnicity could unmask it as a relatively recent invention or transformation (Blu 1980:220).

The Lumbee Indians present a compelling situation for the definition of ethnicity or ethnic groups in that they are comprised of black, white and Native American individuals seeking a basic group identity as Native Americans despite a diverse and intermittently fractious background (Blu 1980:2). Blu does acknowledge that in gaining Native American recognition the group would accrue substantive political and legal benefits, an instrumentalist perspective on their self-identification as a cohesive ethnic group. She goes on to say, however, that their primary aim was to express their basic group identity as Native Americans for its own sake, a primordialist approach to the concept of ethnic grouping.

While this approach may come closer to the reality of how people perceive themselves through the lens of ethnicity, it is an approach which historical archaeologists have not utilized. This may be because of the inherent limitations of the data base with which archaeologists work, namely the frustratingly mute

remains of long- dead individuals and communities who cannot be interviewed directly for their perspective on the world. Blu discusses the problem in her own work of interpreting the actions and motivations of deceased individuals in the formation of ethnic identities and comments that all that exist are accounts of the way they behaved (Blu 1908:184).

While this situation is analagous for those archaeologists fortunate enough to stumble across salient documentary information in the course of their research, more often than not in the investigation of the disenfranchised there is no such information. When such information is uncovered, it is often in the form of documents penned by the elite, an elite against which the ethnic group under consideration has set its boundaries as a means to define itself. Delineation of ethnic identity in this instance is a study in contradictions and presuppositions. A hierarchy of increasingly abstract levels of understanding is set up when an archaeologist a) Must assume an inherent understanding of the elite group which has commented on the world around it and then b) construct an objectified, academic identity of a group subordinate to the elite on the basis of that commentary and then c) transform that objective identity, through another layer of theory, into a subjective or folk “ethnic identity” and, finally d) extrapolate that “ethnic identity” to the material remains of the group under study. Nowhere in this intellectual endeavor is there a single living informant, only an observer (the archaeologist) observing another observer (a long-dead elite) observing the

observed (a long-dead “ethnic group”). This will remain a problem, until it is more intensely scrutinized within the field, and will continue to plague efforts at the reconstitution of ethnicity and ethnic groups from the past.

Dell Upton’s 1996 article, *Ethnicity, Authenticity and Invented Traditions*, provides a particularly cogent discussion of the current use, or misuse, of “ethnicity” as a tool for interpreting the archaeological record. His critique of the epistemological assumptions which structure the issue not only point up theoretical flaws in the ethnic “recovery of meaning” (Leone and Potter: 1988) from the archaeological record, but flaws in the attendant social scientific methodology as well.

Positivism is the dominant philosophy in ethnic interpretation, one that posits a “catalog of values and practices uniquely associated with a particular group of people as distinct from those practices and values of another” (Upton 1996:1). As an example, one element in the unique catalog of values of African-American ethnicity, as asserted by tour guides at the slave quarters at Carter’s Grove Plantation in Virginia, is a cultural predisposition towards “thinking in the round”. This predisposition, one is lead to infer, stands in stark contrast to the more “rectilinear” thinking patterns of Anglo-Americans. Evidence to support this assertion takes the form of the “kraals”, or semi-circular wattle fences which surrounded the slave quarter reconstruction.

While an interesting argument, it is crippled on two levels. First, from a

theoretical perspective, it betrays an *a priori* catalog of values and practices unique to African-Americans, a rigid, etically-constructed catalog which speaks more to an interpretive imperative among archaeologists looking at a vast array of disparate material culture than to any demonstrable test of reality.

Second, this unique catalog of values and practices, which is a testable hypothesis, is methodologically curtailed. The causal relationship between African-Americaness and a particular pattern of material culture, in this case the presence of kraals, is tested against nothing. The existence of semi-circular pens on an African-American site is forwarded as evidence of a one-to-one correlation between “thinking in the round” and African-American ethnicity. Unless this *theoretically* unique value is tested *methodologically* against other data it remains little more than a tautological argument. The argument that circular fences and African-American ethnicity are uniquely linked remains convincing only so far as no one points out other potential cases against which to test the assertion. Comparable test cases can be found on the grounds at Carter’s Grove itself, such as the archaeological evidence that Governor Harwood, the 17th-century administrative head of the abortive settlement at Wolstenholme Towne, had a semi-circular fence surrounding the front of his home at Site A, or that the domestic unit of the town was surrounded by a roughly circular fence.

A prevailing belief in *the stability and staticity of ethnic culture*, essentially an unexamined, over-simplified primordialist position, is another hallmark of

archaeological interpretation (Upton 1996:1). In this model, ethnicity is regarded as a monolithic entity composed of that catalog of values and beliefs uniquely associated with a specific group of people. The point at which this catalog of behaviors, or *traditions*, froze in time and space and became the canonical text for interpretation of a particular ethnic group is never stated explicitly - one can only imagine a mythical time in the past in which all cultural standards were set and social change effectively ceased (Posnansky 1989:1).

Theoretically, this notion is a-historical and presentist. To select, without explicitly stated standards, a group of “traditional” behaviors to represent an entire cultural unit through time deprives the individuals which make up the cultural unit any degree of agency or control over the course of their own lives. The tacit message is that **but for contact with European culture**, “ethnic” cultures would have remained entrenched in “traditional” cultural patterns and lifestyles. The level of a-historicity in the study of African-American ethnicity is further aggravated by this notion of traditionality by a

naive assumption that there is a commonality of African traditional culture spread over a wide geographical area and over a long period of time (Posnansky 1989:1).

Methodologically, the correction to this bias lies in a more rigorous analysis of *African* archaeology and history, to “search out every bit of information on the origins of the ancestors of the Afro-Americans who occupied a particular site or

area” (Posnansky 1989:5). This type of analysis will inevitably broaden and deepen archaeological inquiry at African sites in the New World and counter attempts to draw simplistic cultural parallels among the continents.

Of primary importance to historical archaeologists is the recovery and identification of that material culture which represents ethnicity. Underlying this goal is a sense that ethnicity is invested somehow in the material world and, furthermore, that some artifacts are more “authentic” signs of ethnicity than others (Upton 1996:1). This, too, leads to a host of theoretical and methodological problems in the delineation of ethnicity in the archaeological record.

In discussing the role of material culture and African-American identity, Upton has remarked that

The celebrated cowrie shells and blue beads excavated on slave sites speak as eloquently about our desires as they do about African-American culture (Upton 1996:3).

In the theory-driven search for the material culture correlates of ethnicity, it has been the case that some signs, such as blue beads and cowrie shells, have been judged more “authentic” than others. This judgement is not based on comparative evaluations with other forms of material culture, but on pre-conceived, somewhat romanticized, and largely ethnocentric ideas about what we believe constitutes the ethnicity of the “other”.

The methodological danger in the use of blue beads or cowrie shells as

ethnic “markers” is that it violates an important rule concerning causal theory construction. The use of a “cowrie shell” as an “ethnic marker” is analogous to the use of an “indicator” as an observable manifestation of an ambiguous “concept”. As noted earlier, there is nothing intrinsically wrong with this approach and, in fact, it may be the only profitable way of dealing with the archaeological record. Much of the literature on the identification of specific objects as “ethnic markers”, however, uses those objects not merely as testable implications of a given hypothesis but as concrete proof of a one-to-one correlation between a specific manifestation of material culture and a specific ethnic group. In effect, the material object is reified as the concept it is more properly meant to test, once more a tautological situation which is both the product and perpetuator of an overly simplistic and static view of culture process.

L. Daniel Mauer, in his article, *Chesapeake Creoles: The Creation of Folk Culture in Colonial Virginia* (1993), has characterized ethnicity as

a dynamic and polysemic set of behaviors. Ethnic groupings are situational, historical, culturally relative, fluid, negotiable, multi-dimensional, invented and re-invented every time they are used, discussed, researched and thought (Mauer 1993:106).

The modifier of *politically or nationally-constructed* could also be added to this characterization, a somewhat inflammatory notion but one that has serious theoretical and methodological consequences for the historical and archaeological

investigation of ethnicity.

In her 1992 article, *Of Straightening Combs, Sodium Hydroxide and Potassium Hydroxide in Archaeological and Cultural-Anthropological Analysis of Ethnogenesis*, Brackette Williams tackles this notion of political power relations and ideological agendas in the formation of ethnic groups, and how such contemporary relationships color our vision of the past. Two of her major points which have direct bearing on the study of ethnicity concern the analytic categories of race and class.

Williams discusses the contradictions in the use of the term “ethnicity” and states that ethnicity is

Not synonymous with the race concept, but it is a relative categorization that cannot be understood apart from the concept of race,

and is

Not analytically parallel to the concept of class, but as a categorical distinction employed in nation-states, it cannot be understood apart from the idea of class as a culture-bearing unit (Williams 1992:609).

The definitions of *race* and *class* further complicate the matter. The notion of *race* is defined in evolutionary biology as a “*geographically separated, hence genetically somewhat distinctive, population within a species*” (OED 1989:69). *Class* is outlined lengthily as “*large categories of population, distinct from other categories in respect of wealth and related social position, deriving their*

distinctive status mainly from their location in the production and distribution of social wealth, sharing accordingly in distinctive interests either opposing or complementing other group interests, and consequently displaying a tendency to a group - distinctive political, cultural, and social attitudes and behaviors (Kruper 1996:90). While it is evident that these two terms are not strictly interchangeable with the term ethnicity, they are implicitly subsumed by the term in an attempt to maximize leverage and decrease the number of explanatory variables in the problem. In collapsing these individually abstract terms into the cumulatively more abstract rubric of ethnicity, one-to-one correlations between material culture and ethnicity are confounded through the error of omitted variable bias.

For the purposes of designing useful theories and hypotheses about “ethnicity” in the past, the term itself must be deconstructed into its currently formulated constituent parts. The examination of African-American ethnicity from an archaeological standpoint, for example, must include economic status as a discrete explanatory variable to demonstrate whether it does or does not co-vary with African geographical heritage. If all African-American sites examined co-vary with a low socio-economic status, than the variable of African-American “ethnicity” as the causal factor for an observed material culture pattern cannot be separated analytically from the economic explanatory variable, resulting in an indeterminate research design.

Similarly, if ethnicity is always couched in terms of racial background and

all “ethnic” sites are defined presumptively by the racial background of their past inhabitants than, once more, ethnicity and race co-vary and are not meaningfully separable. This situation is even more contentious than the co-variation of economics with ethnicity in that the concept of “race” is ill-defined, controversial and, despite some ill-starred and racist attempts (i.e. *The Bell Curve*, Herrnstein and Murray 1994), does not lend itself well to quantitative analysis.

In separating out the social variables that serve as the current scaffolding for the construct of “ethnicity”, ethnicity itself must be re-evaluated based on a new set of criteria. This criteria, ideally, should take the form of more geographically and historically specific analyses of cultural interaction and should better serve to pinpoint those pieces and patterns of material culture which bear potential ethnic significance.

Chapter IV. Patterns Progress: Ethnicity, Patterns, Process and Problems

Archaeology in the humanities is concerned with human sensuality, sociability, wisdom, ideational internalization, cultivation of the intellect and education toward the enjoyment of life (South 1977:5).

So wrote Stanley South, not without some admiration, of the humanistic goals encompassed by the discipline of archaeology. Citing Noel Hume and Ascher and Fairbanks, South described an academic environment in which “a personal confrontation with the past” (Noel Hume in South 1977: 7) seemed to be the primary aim, an artistically-rendered and, ultimately, subjective interpretation of the archaeological record.

It is exactly this subjective and “unscientific” approach to the past to which South theoretically and methodologically objects and out of this objection he produced his seminal 1977 monograph, *Method and Theory in Historical Archaeology*. In it, South makes the point that in order for historical archaeology to transcend the academic harangue of antiquarianism it must transcend the pursuit of particularistic studies toward the creation of more generalizing scientific theories concerning human behavior as reflected in the archaeological record. He

rightly asserts, contrary to Noel Hume's perspective, that "science" is not merely a technical aid to archaeologists possessed by "outside" experts, but rather a way of organizing and examining data for more efficient problem solving (South 1977:8). South suggests the use of *pattern analysis* as a means to construct lawlike generalizations about the past. The lawlike generalizations to which South aspires are meant to elucidate regularities in culture processes, processes which he believes leave identifiable patterns in archaeological assemblages. He remarks that

Historical archaeologists have concentrated on the reconstruction of culture history and the reconstruction of lifeways and have virtually ignored delineation of culture process. The key to understanding culture process lies in pattern recognition...Once pattern is recognized, the archaeologist can then ask why the pattern exists, why it is often so predictive it can be expressed by laws (South 1988:31).

He does caution, however, that

Confirmed general laws do not 'provide us with an understanding of history'... understanding comes only when we ask why the facts are the way they are. This is theory building (South 1977:16).

Through his examination of specific discard patterns and ratios of artifact classes to one another, South developed the general analytical tool of pattern analysis to quantify such specific assemblages as the Carolina, Kitchen, Frontier and Inventory patterns. Through the demarcation of these assemblages he hoped

to shed light not only on the site-specific processes that created them, but also on the larger historical and cultural processes which served as the foundation from which the patterns derived their predictive strength.

Historical archaeologists have employed South's tool of pattern recognition widely since its introduction; unfortunately, much of its use has been of an uncritical and misguided nature. Charles Orser has been particularly sharp in his criticism of pattern analysis, particularly in regards to the implementation of the "Plantation Pattern" by other archaeologists, and more generally of South's "eclectic" approach to anthropological theory (Orser 1989:23).

The root of the problem lies in a fundamental incommensurability between the theory of pattern recognition and its practical usage in archaeological interpretation. This incommensurability has led to the creation of one-to-one correlations between specific archaeological patterns and specific cultural patterns, essentially an assemblage-based analog to the use of individual pieces of material culture as "markers" for distinct ethnic groups. These correlations are often gleaned from highly circumscribed data sets and then declared general laws, often to the exclusion of further hypothetico-deductive testing. From the outlook of social scientific theory construction, this approach is akin to putting the theoretical cart before the methodological horse.

Many archaeologists have misapplied the notion of pattern analysis to represent not culture process but culture itself. As such, the two terms warrant

specific definitions. The archaeologist Brian Fagan, borrowing from the Victorian anthropologist Sir Edward Tylor, defines *culture* as

the complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and habits acquired by man (or woman) as a member of society (Fagan 1997:36).

He further defines *culture process* as

the attempt to consider all of the factors that cause changes in human culture and how they affect one another (Fagan 1997:36).

An expansion of this notion of culture process would include consideration of not only those factors which cause change, but also those factors which maintain cultural stasis. While cultural change is explicitly dynamic in nature, the process by which cultural traits are retained over time may be considered an equally dynamic, if less visible, form of culture process. The active conservation of long-standing cultural traits against larger currents of cultural transformation can require as much energy as that possessed by the transformative processes themselves.

The interpretation of an archaeological pattern as the symbolic maintenance of a particular cultural identity moves away from the explanation of *culture process* through which the pattern was produced and serves as a metaphor for *culture* itself. A good example of this tendency is the use of the pattern of a predominance of hollow forms versus flat forms in an archaeological assemblage

as a “marker” for African-American ethnicity. The evidence to support this assertion relies on the idea that African-Americans retained traditional culinary practices when brought to the New World, and is a well-founded assertion based on evidence of the conservatism of foodways among all cultures (Goody 1982; Brown and Mussell 1984).

However, this interpretation of the archaeological pattern does not address the dynamic *process* of African adaptation to New World conditions as evidenced in foodways, but mistakenly reifies a single aspect of “African” culture lifted wholesale from the shores of the African continent. One facet of *African* culture, namely foodways, is forwarded as the archaeological representation of *African-American* culture. This reduces a complicated cultural identification to a collection of bowls and plates and, conversely, reifies the bowls and plates as African-American culture itself.

This contradiction is largely the product of the interchangeable analytical use of the concepts of “culture” and “ethnicity”. “Culture” represents a monolithic entity, a product of any number of complex processes which, if frozen in time, would represent a cohesive, if artificial, whole. Pattern analysis stumbles when it mistakes whole cultural entities with culture process (Orser 1989:29), thereby reifying the indicator as the concept.

The meaning of an identified archaeological pattern is confused by the implicit interchangeability of the terms “culture” and “ethnicity”. African-

American identity in the New World is a product of two distinct but enmeshed cultures, namely Western European (specifically English) and African. As mentioned earlier, the specific nature of African culture is circumscribed geographically and, at this point, poorly understood. Nevertheless, for the sake of interpretive purposes, archaeologists have itemized African culture, rightly or wrongly, into a series of concrete attributes or indicators. The patterned presence of these indicators in an archaeological assemblage which then is touted as evidence of African-American ethnicity.

What is actually in evidence, however, is a static whole cultural pattern from Africa which says nothing about the process of ethnic identification among Africans in North America. The contact of African and English “cultures” precipitated a dynamic “process” of ethnogenesis or, perhaps, subcultural identification, among Africans and, to a lesser extent, among the English. The methodological and theoretical error is compounded in two ways: first, pattern analysis is used incorrectly to represent holistic cultural traditions when it should in fact elucidate cultural process and; second, ethnicity is represented archaeologically as a single, static cultural pattern rather than as a dynamic exchange between cultures. In short, ethnicity becomes a synonym for a monolithic cultural tradition and becomes a monolithic concept in and of itself. Whether these errors in identification take the form of material culture patterns or singular items of material culture, the true dynamic nature of ethnic identification

is lost. Brackette Williams describes this paradox in her discussion of ethnic identification among the Genizaro, a New Mexican Native American group, when she states

In search of the material culture of the Genizaro, Cordell and Yannie ignore their status as a product of power relations. This inattention results, first, in the elimination of the diverse factors of production. Second, it leads to a failure to direct attention to the material productions of these power relations (i.e. the Genizaro), which cannot be an authentic denial (i.e. a surviving Indian element), of the very cultural processes that produced the Genizaro as a categorical identity because these artifacts do not distinguish one ethnic group from another. Thus it is unclear how such a processual account is to advance our understanding of ethnogenesis when, in the end, it seems to depend on the methodological denial of the very process out of which the ethnic group was produced... Finding the surviving remains of one aspect of their identity - the Indian- is not to reconstruct them as an ethnic group, but rather to return to the point of contact and, at best, indicate that contact was not able to obliterate (Williams 1992:611).

A second, more general criticism of pattern analysis is best described through Clifford Geertz's "twitch and wink" argument. King et.al. employ this metaphor to discuss the imputation of meaning into the construction of causal

relationships, and it is equally appropriate to a discussion of pattern analysis.

Geertz writes

Consider...two boys rapidly contracting the eyelids of their right eyes. In one, this is an involuntary twitch; in the other, a conspiratorial signal to a friend. The two movements are, as movements, identical; from an I-am-a-camera, "phenomenalistic" observation of them alone, one could not tell which was a twitch and which was a wink, or indeed whether both or either was twitch or wink. Yet the difference, however unphotographable, between a twitch and a wink is vast; as anyone unfortunate enough to have had the first taken for the second knows. The winker is communicating, and indeed communicating in a precise and special way: (1) deliberately, (2) to someone in particular, (3) to impart a particular message, (4) according to a socially established code, and (5) without cognizance of the rest of the company. As Ryle points out, the winker has done two things, contracted his eyelids and winked, while the twitcher has done only one, contracted his eyelids. Contracting your eyelids on purpose when there exists a public code in which doing so counts as a conspiratorial signal is winking (Geertz 1973:6).

What this analogy suggests for pattern analysis is that any observed pattern, in this case a preponderance of hollow forms over flat forms, and the interpretation of that pattern, namely, the maintenance of traditional African foodways, can and

should be arrived at only as a consequence of a detailed understanding of the cultural milieu in which the pattern was produced. Unfortunately, the African and African-American milieu is one with which, as Posnansky has noted, we are hardly familiar. In short, cultural process in Africa first must be better understood before its dynamic interaction with the cultural processes of the English, and the ethnically meaningful products and patterns of that interaction, can be identified.

There is also a danger when looking for ethnic “markers” in the form of archaeological patterns that those patterns may come to embody those markers by default through *researcher-induced bias* or “distortions of survival” (Prown 1982:20). While some patterns may bear closer scrutiny as potential ethnic markers, researchers must keep in mind that they simply may represent part of the range of variation inherent in the formation and utilization of material culture. The question becomes - did a higher percentage of bowls over plates have ethnic or cultural meanings for the individuals using them or is it merely a convenient archaeological pattern to which we must accord significance because bowls and plates are all we have?

The bias of the archaeological record, a *world-induced bias* (King et al. 1994:135-136) of differential preservation and excavation opportunities, is one over which archaeologists often have little control. On the other hand, the collection, or lack of collection, of pertinent historical and cultural data is an *investigator-induced bias* (King et al. 1994:132-134) and one which can have the

most impact on a more comprehensive understanding of particular patterns in the ground. Until archaeologists more completely immerse themselves in the details of African and African-American culture process on a specific level, than the patterns which we now interpret as ethnic “winks” cannot be separated analytically from the material culture “twitches” of random variation, bias, or mere chance.

Chapter V. African-American Ethnicity: The Methodology of Essentialism Versus Context

Over the past twenty-five years, African-American archaeology has become one of the most highly visible and intellectually productive avenues of archaeological inquiry. Much of this inquiry has settled on the material culture associated with plantation contexts in the South and aggregates of African and African-American populations. Most notable among the publications on the subject is John Solomon Otto's dissertation-based monograph, *Cannon's Point Plantation, 1794-1860: Living Conditions and Status Patterns in the Old South* (1984), a comparative archaeological study of the material culture of the three social classes of planter, overseer and slave in antebellum Georgia.

In his quantification of the ceramic tablewares among the three sites examined at Cannon's Point, Otto calculated the proportion of serving bowls, or hollow forms, to be roughly 45% in the slave assemblage, 25% in the overseer assemblage, and 10% in the planter assemblage. Conversely, serving tablewares, such as plates and soup plates, or flat forms, comprised roughly 50% of the slave assemblage, 70% of the overseer assemblage and 85% of the planter assemblage. Briefly, Otto suggests that this variation is the consequence of differential status

based on differential access to economic resources which in turn dictated the quality and forms of ceramics used by the three groups. As a tool for further research, Otto is very explicit in defining the analytical parameters of his research design and interpretive goals. He states as his primary hypothesis that

At Old South tidewater plantations known to have been occupied by white planters, white overseers, and black slaves, one may expect to find status-related patterning in the archaeological and written records of material living conditions... because of differing access to the plantation surplus (Otto 1984:171).

As a subhypothesis he states that

In terms of household and personal possessions, such data categories as ceramic types should reveal patterning that reflects economic status differences; ceramic shapes and forms should reveal patterning that reflects social status differences...(Otto 1984:171).

While inductively reached, Otto has proposed a clear research design for the construction of a causal theory - the effect of status on archaeological assemblages - and then defines the theoretically observable implications of that theory, among them a preponderance of hollow forms within slave assemblages as a reflection of limited economic means. In addition, he effectively maximizes the leverage of his theory by circumscribing it temporally and geographically to the antebellum South and by addressing the relationship among three historically and

contextually-defined economic classes.

Not long after the publication of Otto's research, another African-American site, this time in New England, was revisited by Vernon Baker. Originally excavated by Adelaide and Ripley Bullen in 1945, Black Lucy's Garden in Andover, Massachusetts dates from approximately 1812-1845 and represents one of the first sites extensively reported upon with an eye to the reconstruction of an African-American life.

The story of this site is really the biography of Lucy Foster, a negress. As it sheds some light on the Andover of 100 to 200 years ago, as well as giving the very human story of Lucy, it seems worthwhile to briefly include it here. The history of the specimens dug up cannot be properly separated from the history of those who used them. (Bullen and Bullen 1945:26).

It is unclear whether Lucy Foster was African- or American-born as the documentary record concerning her life is extremely sparse. What is known is that she was in all likelihood a slave within the well-to-do household of the yeoman Job Foster by 1771. During her tenure at the Foster's, she gave birth to a daughter, performed any number of domestic duties common among New England servants, and eventually gained her freedom around 1780. After Job Foster's death in 1782, Lucy remained with his widow, Hannah. Over the course of the next 30 years, Lucy moved from the Foster homestead, to the homestead of Hannah's new husband, Philemon Chandler, and back to the Foster tract, gave

birth to a son in 1792, and was admitted to the South Parish Church in 1793 on profession of faith.

Upon Hannah Foster's death in 1812, Lucy was willed a one acre plot within the bounds of the remaining Foster property on which she built a small cabin and lived until her death in 1845. Lucy's means of support during the 30 years she lived on her own property is unclear, but her enrollment on the Andover parish dole immediately after Hannah's death suggests it was probably a meager existence. In excavating Lucy's acre, including a cellar hole, a dump, and a well, the Bullens were able to reconstruct a picture of an elderly black woman who managed, possibly with the help of her son, to support herself for 30 years through supportive ties with the church and the surrounding Anglo-American community.

They comment that

She seems to be a worthy, respected and faithful person with a flair for collecting pottery (Bullen and Bullen 1945:28).

It was just this flair for collecting pottery that Vernon Baker focused on in his 1978 re-examination of Black Lucy's Garden. In quantifying the ceramic sherds from the site and establishing minimum vessel counts, Baker arrived at a proportion of flat forms versus hollow forms that bore a striking resemblance to that which Otto calculated for the slave sites at Cannon's Point. Out of 113 mended vessels encompassing 49 tableware items, roughly 40% of the tableware represented serving bowls, or hollow forms, a proportion in agreement with that

arrived at by Otto. Additionally, the presence of chopped cuts of meats, suitable for pottages and stews, constituted the majority of the identifiable zooarchaeological remains. This, too, coincided with Otto's findings at the slave sites in Georgia.

From this comparative data, Baker chose a different interpretive path than that of Otto. He suggested that this specific pattern of ceramic vessels and faunal remains indicated not merely economic or status issues, but a pattern distinctive of African-American occupation and the retention of "Africanisms" among transplanted populations.

Although affiliation of the above patterns to African cultural elements is unclear, the presence of serving bowls exceeding 40% of all tableware, plus chopped faunal remains approaching 100% of all such remains, appear distinctive of Afro-American sites, both slave and free (Baker 1978:112).

While circumspect in the exact relationship between these Africanisms and the unique African-American archaeological patterns they seemed to produce, Baker cited the work of Blassingame, Garret and Lomax as references for the nature and quality of African survivals, including such archaeologically invisible characteristics as music, food and dance (Baker 1978:110). As Baker formulated his argument, the patterned "Africanism" of a given percentage of hollow forms in a ceramic assemblage came to represent an approach to cooking and eating "Among traditional West African peoples, (where) it was common practice to stew

grains, vegetables, and meats with pepper in ceramic vessels (Otto 1984:174).

Both Baker and Otto present interesting arguments for the presence of particular patterns among different assemblages, but it has been Baker's study which has received the most currency among historical archaeologists. This is no doubt because Baker's interpretation of Black Lucy's Garden speaks directly to issues of ethnicity and ethnogenesis; indeed, Black Lucy's Garden is cited extensively in current research on the topic of African American archaeology and remains required reading in many undergraduate and graduate archaeology classes.

Baker's monograph, unfortunately, is highly flawed from a social scientific, causal theory construction standpoint. The range of issues discussed in Chapters II, III, and IV converge in Baker's interpretation of Black Lucy's Garden and, more troubling, in the continuing advocacy and reconstitution of his perspective.

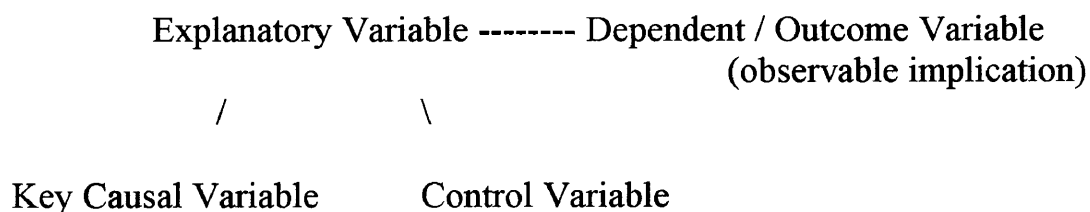
If pattern recognition does not go beyond identifying and labeling pattern, it is a particularistic and inductivist exercise of dubious value in itself (South 1988:27).

Acute interest in the subject of African-American ethnicity has led to many detailed, "post-processual" discussions of power relations, self-identification, and boundary maintenance (Wade 1988; Williams 1992; Fitts 1996; Upton 1996). This post-processual dialogue has been translated into archaeological and "processual" terms through the use pattern analysis.

Specifically, some historical archaeologists have claimed that “unique” material culture assemblages constitute one-to-one correlations with African-American occupied sites. Pattern analysis has been a powerful analytical tool in this enterprise, but also has been weakened through theoretical and methodological misapplication.

The basic theory within African-American archaeological research is understood most clearly in diagrammatic form as derived from the discussion in Chapter II:

General Causal Theory Model



“Ethnicity” Causal Theory Hypothesis

Ethnicity (Key Causal Variable) ----- Specific Archaeological Patterning

African-American Ethnicity Causal Theory Subhypothesis

African-American Ethnicity (Key Causal Variable) ----- Predominance of Hollow

Forms

/	\	
Enslaved	Free	(Control Variables)
North	South	

At the most general level of criticism, the association of a preponderance of hollow forms as indicative of, or caused by, African-American ethnic practices remains largely a tautological argument. The range of excavated sites hold the key causal variable, African-American ethnicity, constant as the sites were, by definition, occupied by African-Americans. This represents an example of selection bias as “the causal effect of an explanatory variable that does not vary cannot be assessed” (King et al. 1994:146). It is a circular argument to assert that a 40% proportion of hollow forms in a ceramic assemblage is representative of African-American ethnicity when the only sites against which the hypothesis is tested are known African-American occupied sites.

Both Baker and Otto recognize this circularity, at least implicitly, (Baker 1978:113; Otto 1984:175) and recommend testing the hypothesis against poor white sites, both North and South. It seems from a survey of the literature, however, that the hypothetical correlation of a specific ceramic pattern with

African-American ethnicity has become a canonical truth for many archaeologists. This perception has effectively stifled attempts at falsification of the hypothesis and has led to profound investigator-induced bias in the data collected to test the hypothesis.

Theresa Singleton has noted that the increased excavation of sites of antebellum free blacks and emancipated men and women in the South will provide a more “rounded” data base against which hypotheses about African-American ethnicity, including ceramic patterns, may be tested (Singleton in Orser 1989:158-159). Robert Fitts, in his 1996 article, *Landscapes of Northern Bondage*, remarks that without the excavation of more Northern slave quarters, the extent of the similarities between Northern and Southern patterns of resistance will remain unclear. While both authors provide excellent suggestions for the elaboration and further testing of a pre-existing theory, neither allows for variation on the key causal variable, African-American site occupation. It would be more methodologically fruitful to test these hypotheses against data sets in which the key causal variable provided some range of variation.

One possibility lies in the excavation and analysis of poor white sites, both North and South, of the eighteenth and nineteenth centuries. This social class represents probably the only group which is more archaeologically and historically understudied than that of African-Americans. The study of the material culture of enslaved African-Americans has benefited, ironically, from the concentration of

that population in rural plantation contexts, contexts which often are well documented even if those documents do not deal specifically with the African inhabitants of the area. Douglas Armstrong, Leland Ferguson, and Jim Deetz, among many others, have utilized this concentration of information to study African-American lifeways in a community environment.

Poor whites do not often present “convenient” rural aggregations amenable to archaeological study. Such individuals more often than not lived in expedient and highly perishable housing, eked out meager existences from marginal land with little in the way of material comforts, and died, unnoticed and unaccounted. This situation makes it extraordinarily difficult to find these individuals in history or in the ground.

One potentially productive avenue for the investigation of poor whites is in the excavation and analysis of those sites where rural Anglo-Americans did congregate and live in acute poverty, namely almshouses or asylums. The recent completion of the Phase II site examination of the 19th-century Smithfield Town Farm and Asylum, excavated by the Public Archaeology Laboratory, Inc. in Smithfield, Rhode Island (CNEA Newsletter 1997:14), presents an interesting case against which to test the notion of the specific archaeological patterning of ceramic wares as indicative of a particular ethnic group. Like the slaves at Cannon’s Point, the residents at the Asylum relied heavily on donated ceramic wares, usually older and in less than pristine condition. The comparison of the assemblage at the Poor

Farm in Smithfield with plantation slave quarters may provide interesting insights into dining habits and preferences and the purported effect of ethnicity on those preferences.

Another comparative choice is that of Native American sites. Excavations at the Mashantucket Pequot Reservation in Ledyard, Connecticut have unearthed both isolated native farmsteads and a small, rural Pequot community dating to the late-18th to early-19th centuries. Both data sets comprise sizable ceramic assemblages and some primary historical documentation. Like enslaved Africans brought to the New World, Native Americans also experienced massive “culture shock” upon contact with Europeans which precipitated both adaptation and resistance. The comparison of the archaeological assemblages of this group with those of plantation slaves will allow for a more critical evaluation of the degree to which “ethnicity” is materially patterned.

A final comparative possibility is that of communities or homesteads where both Native Americans and African-Americans lived together. Intermarriage between African-Americans and Native Americans was extremely common in the 18th and 19th centuries, in part the result of a shared marginalized existence and a common antipathy towards the larger white community. One such site, the Robert Croud homestead in Sturbridge, Massachusetts, was home to Robert Croud, a Punkapoag Indian, and his African-American wife, Diantha Scott, during the 1840s. In reconstructing the family genealogy, researchers at Old Sturbridge

Village in Sturbridge, Massachusetts, discovered that Diantha's parents, Guy and Hannah Scott, were also of African and Native origin and relatively prosperous until bad health and accumulated debts forced the sale of the family farmstead and adjoining lands. Robert Croud himself did relatively well financially early in his life, although his financial situation at the time of his death in 1889 is currently unknown (Baron, et al. 1996).

Delineating material markers of "ethnicity" in this situation would be complex, to say the least. In this case, not only are there two "ethnic variables" to evaluate but also how those ethnic variables interacted with one another and the external Anglo-American community. This type of data base alone confounds questions concerning the material expression of ethnicity. Are we to afford primacy to the Indian or African element in material culture production? Will one necessarily swamp out the other? If so, why? Despite, or maybe because of, all of these uncertainties, sites such as the Robert Croud house can provide yet another explanatory variable for assessing the utility of ethnicity as a causal factor in pattern analysis.

The degree to which Baker's theory has been uncritically accepted by many historical archaeologists is illustrated in biased data collection and analysis and in contradictory statements such as "A few *preliminary* studies have found...*distinctive* African-American...dining patterns" (Fitts 1996:169). Only through efforts at falsifying the theory, efforts best directed at the construction of

alternative hypotheses such as those suggested, can the general veracity and analytical parameters of the theory be assessed (King et al. 1994:19).

It is an historian, Herbert Gutman, who confronts this point most concretely in his comprehensive study of African-American life, *The Black Family in Slavery and Freedom, 1750-1925*. In it he argues that

Methods are used and questions are asked that take account of the many unique circumstances associated with enslavement but nevertheless remain appropriate to the study of the development of all exploited and dependent social classes, slave and free, white and non-white... a point emphasized not to argue that slaves and free workers had a similar history but rather to suggest that the same questions can and must be asked of such classes to understand important similarities and differences between them (Gutman 1976:3).

A far more abstract problem in both Baker's and Otto's work, a problem extensively addressed in Chapter III, is the essentialist and static definition of ethnicity which the authors imply in their one-one-correlation between African-American ethnicity and patterned ceramic assemblages. This implied definition is shared by many historical archaeologists who view "Africaness...(as)...an ineffable and inexplicable quality..." (Upton 1996:3), but a quality that paradoxically unmask itself in the form of distinctive architectural patterns (Vlach 1976:47-70), ceramic assemblages, and faunal remains. In using "African-American ethnicity"

as a key causal variable in their hypotheses concerning assemblage formation and patterning, Otto and Baker adopt a theoretically reductionist and methodologically meaningless stance.

From a theoretical standpoint, they assume a homogeneity of adaptive strategies among African populations in North America independent of individual desires and historical contexts. Collective behavioral interpretations as embodied in pattern analysis at Cannon's Point are extrapolated implicitly as representations of individual behavior at Black Lucy's Garden. In doing so, Lucy's individuality and specific historical condition are subsumed within the artificial construct of African-American ethnicity (Upton 1996:5-6). Gutman comments on this erroneous assumption by noting

How slaves learned and what they learned from always affected what they believed and therefore how they behaved and the choices they made
(Gutman 1976:31).

The choices made by African-Americans were shaped both by internal and external forces, either in isolation, as in the case of Lucy Foster living in predominantly white, antebellum New England, or in community as among the large slave population at Cannon's Point plantation. On the nature of internal community choice and change Gutman notes

Slave-naming practices and marital rules... are unmistakable evidence of the importance of interior slave beliefs and experiences in shaping their

*behavior. But these beliefs and the **developing culture** that sustained them could not have regularly revealed themselves over time if they were no more than free-floating slave beliefs... Institutional arrangements - especially among slaves- had to exist for such beliefs to be acted on over time, and that is why inter- and intragenerational linkages between slave families are so important (Gutman 1976:259-260, emphasis added).*

External dynamics were unquestionably powerful in shaping African-American identity, slave and free, as well.

What an African learned about slavery and about New World culture differed greatly if the African had prolonged contact with a wealthy Carolina plantation family like the Beverlys, a small planter, a sailor, or a horse trader (Gutman 1976:339).

In other words, African-American beliefs, reactions and attitudes were shaped as much by the diversity of white cultural environments to which they were exposed as by internal African cultural beliefs and customs.

The essential point is that the adaptive capacities of African-Americans, enslaved or free, isolated or aggregated, are the product of the cumulative experience of past generations in Africa and the New World (Gutman 1976:34) and a diversity of white environments, not merely a formulaic, monolithic suite of behaviors lifted directly from the shores of West Africa. It was the long-term dynamics of these variables which shaped individual and group behavior which, in

turn, shaped the resultant material culture.

This assertion requires that the term ethnicity be “unpacked” of its constituent theoretical and historically specific baggage in order to construct analytically useful causal theory. Gutman astutely observes that

Inadequate study of how there developed among Afro-American slaves what Mintz calls “historically derived values, behavior patterns and practices” obscures and even distorts the meaning of accurately observed slave behavior (Gutman 1976:32).

In trying to make sense of the material culture ostensibly representative of African-American ethnicity, we must separate the *value culture* of African-Americans, the customs, beliefs and values presumably influenced by an African heritage, from the *reality culture* or aspects of slave life largely influenced by external forces (Singleton in Orser 1989:142). In separating these spheres methodologically we then can examine how they intersected one another and how that intersection manifested itself materially.

As discussed in Chapter III, the concept of “ethnicity” is rather an amorphous explanatory variable, lacking the degree of concreteness that would allow for its use as a strong key causal variable. The issue becomes even more complex when “African-American ethnicity”, specifically, is used as the key causal variable in the formation of archaeological patterns. In effect, the “ineffable quality” of “African-Americaness” is used as a singular and constant

causal variable, one in which potentially important control variables are ignored. The underlying theoretical message is that merely sharing in a common African heritage is enough to validate “African-American ethnicity” as an adequate cause for observable archaeological patterns. While intuitively persuasive for many, this approach founders on the methodological flaw of omitted variable bias, a bias caused by the collinearity of explanatory variables, and is best observed in the examination of Baker’s investigation of Black Lucy’s Garden.

In hypothesizing the causal effect of the variable of African-American ethnicity, Baker’s casual theory structure takes the form of that diagramed in the African-American Ethnicity Causal Theory Subhypothesis. This model uses an ill-defined concept, ethnicity, as the key causal variable in the formation of the concept’s indicator, a predominance of hollow forms in a ceramic assemblage. Baker’s hypothetical control variables are actually of very little substantive effect in that they do not critically deconstruct the monolith of “African-American ethnicity” but provide artificial controls on a tautological theory.

In an effort to increase the concreteness of the explanatory variable in order to more accurately assess its predictive power, it is more useful to refer to William’s discussion of the factors which are subsumed implicitly within the construct of “ethnicity”, namely the variables of race and class. In doing so, the diagram takes on the following shape:

African-American Ethnicity ----- Preponderance of Hollow Forms

/ \

Race

Class

It then is useful to further subdivide the categories of race and class into their specific values as represented in Baker's study of Black Lucy and, incidentally, as they conform to the situation of the plantation slaves in Otto's work.

African-American Ethnicity ----- Preponderance of Hollow Forms

/ \

Race

Class

/ \

Black

Lower socio-economic bracket

While the use of the term "black" may produce a sour taste in many people's mouths, for the purposes of this model it is necessary for semantic reasons. The substitution of the term "black" with "African" or "of African heritage", while politically preferable, is methodologically untenable. Both "African" and "of African heritage" imply a whole suite of cultural and historical particularities and, therefore, cannot be used effectively to deconstruct the concept of ethnicity as they represent complicated ideas in their own rights. In this instance, "black" is meant to represent **only** the methodologically defining attribute of the color of an individual's skin as distinct from the skin color of a larger "white" population.

The next step is to assess the relationship of the constituent variables of ethnicity as they have been methodologically unpacked. This assessment relies heavily on specific historical factors and, critically, on the determination of the independence of the component explanatory variables of African-American ethnicity, the requirement that the variables of race and class do not co-vary with one another.

In the case of Lucy Foster, a 19th-century African-American woman in New England, the factors of race and class may co-vary strongly. In discussing the importation of black slaves into the 18th-century Boston market, Beth Anne Bower noted

White servants did not constitute a suitable source of labor...Not only did white servants tend to run away, but Boston's elite dreaded the possibility that the servants, once free, would strive and possibly succeed in raising themselves to their masters' social level. There was no threat of social equality in the case of black slaves, for even free black men were doomed to the bottom of the white social strata by the color of their skin (Bower 1991:59).

Undoubtedly, this “dogged racism” (Bower 1991:61), based on skin color and enforced by the intentional limitation of economic opportunity, applied to both male and female African-Americans and carried over into the next century.

The generally peripheral importance of free African-Americans to the larger

Northern economy also contributed to their economic limitations (Piersen 1988:3). 19th-century New England maintained an industrial economy flooded with cheap skilled and semi-skilled immigrant labor, immigrant labor which, more significantly, had white skin (Horton 1979:9). The Hortons have calculated in their book, *Black Bostonians*, that on the eve of the Civil War, blacks in Boston had a per capita property worth of \$91 in comparison to the per capita worth of \$872 for the entire population of Boston. Even the Irish, scorned and marginalized immigrants to the city, were economically better off than the blacks with a per capita wealth of \$131 in 1860 (Horton 1979:12).

How closely the black experience in urban Boston reflected the conditions of African-Americans in more rural settings, such as Lucy's residence in Andover, is unclear. There are a number of clues, however, which suggest that the economic situation for blacks was not improved appreciably by life in the New England countryside. Karen Hansen, in her study of social interactions in non-urban New England contexts, *A Very Social Time: Crafting Community in Antebellum New England* (1994), has written that white rural and working class New Englanders expressed a social racism to maintain white privilege and exclude free blacks from getting an education, learning a trade, or even traveling as free persons (Hansen 1994:168). This "social racism" generated dire economic consequences for blacks, particularly when manifested as an active opposition to acquiring labor skills.

Obviously, procedural freedom for African-Americans in New England did not equal substantive equality, either socially or economically. In fact, the process of emancipation often led to profound economic misery. Piersen notes that in the year 1742 in Boston, over 7% of the black population was free but living in almshouses. This situation was particularly common among elderly black men, both in rural and urban settings. These individuals, when freed in their old age, were far more liable to live alone and suffer from improper care than either elderly white men and women or elderly black women (Piersen 1988:22). That emancipation and freed status was an economically tenuous situation for most blacks was acknowledged tacitly in colonial New England by a law which legislated “against their manumission unless their masters gave bond for fear they became public charge” (Greene in Cromwell 1994:27). That this condition persisted into the 1800s is testified to by the life of Lucy Foster who, upon the deaths of her former owners, is documented to have been immediately enlisted onto the public assistance roles of the local church (Baker 1978:5).

Based on this very brief and perfunctory survey of the historical documentation concerning the economic status of African-Americans in New England, it seems that the variable of impoverished economic means cannot be separated methodologically from the condition of 18th- and 19th-century African heritage. The co-variation of economics and race appears to be strong. As such, the omission of these variables in the analysis of African-American ethnicity

produces a significant bias which, ultimately, produces an indeterminate research design when based on the hypothesis of a causal relationship between African-American ethnicity and material culture.

Chapter VI. Ethnicity, Economics and Where to Go Next

That Lucy was black and that Lucy was poor is, for methodological purposes, an inseparable co-variation; in fact, Lucy's African identity may be demonstrated to statistically predict her economic condition. Thus it seems that John Otto's original suggestion that differing economic conditions cause different ceramic assemblage patterns was a more sound, and may be applicable more appropriately to the ceramic pattern observed by Baker at Black Lucy's Garden.

Some may object that the dismissal of the causal effect of African-American ethnicity on archaeological patterning, a dismissal based on a proposed co-variation of race and economics, is an etic and anthropologically-sterile point, one that does not address the humanistic issues of how African-Americans viewed their own lives and how, accordingly, they used their material culture. Yet as historical archaeologists engaged in a discipline that demands a processual approach to the past, an approach which concentrates explicitly on the voiceless remains of archaeological assemblages and their reputed patterns, it is absolutely critical that the construction and meanings of those patterns be based on thorough contextual research and coherent, logically consistent social scientific theory design. It is not African-American ethnicity that is being discarded, only the

sloppy methodological use of the concept. As Stine has commented

The complexities of social and economic life need not be blurred by the use of imprecise concepts (Stine 1990:37).

As to the issue of the analytically etic connection of racial background and economic condition, there is tantalizing evidence to suggest that in some places and at some times African-Americans may have connected their “ethnicity” with their economic condition. During Gutman’s research of free and enslaved black families in the South he came across the *Petition of Liberty County*, a document penned by freed Georgia blacks in 1865, in which they wrote

We cannot Labor for the Landowners... (while) our Infirm and Children are not provided for... We are a Working Class of People (Gutman 1976:185).

How intimately this community identified itself as an economic class is difficult to assess; their motivations for creating this connection undoubtedly have complicated political and social underpinnings and may be connected to an instrumental ethnic strategy (Banks 1996:39). But the petition does represent an interesting new avenue of anthropological and historical investigation, one in which an African-American emic perspective on ethnic identification and economic condition is paramount.

Although economic status is, theoretically, a more concrete, more quantifiable condition than that of ethnicity, it is not an analytical panacea for pattern analysis. Robert McGuire has gone so far as to assert that

Economic status, since it results from material wealth, has great potential for adding to the materials at a site and, a priori, should be considered the dominant social dimension evident in the archaeological record of domestic dwellings in a single society or economic system (McGuire 1982:164, emphasis added).

First and foremost, no element of social life should be allowed to take on the proportions of an *a priori* assumption in social scientific theory, even one as seductive as economics. Here, again, Hocart's muse that specific cultural preoccupations dominate the study of culture itself takes on a peculiarly resonant quality. *A priori* assumptions are fatal in social scientific research as they create dogmatic, self-fulfilling theories and suffocate further hypothetical testing.

Second, and more pragmatically, McGuire's assertion was effectively invalidated two years later by, of all people, John Otto. In his research with G.D. Gilbert of a "Plain Folk" cabin, the homestead of a prosperous late 19th-century farming and blacksmithing family in Kentucky, Otto effectively deconstructed the one-to-one correlation of economic wealth and material culture. Through documentary research, Otto and Gilbert discovered that while the Hoover family, the occupants of the plain folk site, were quite wealthy the majority of their wealth was funneled into the purchase of additional acreage, not portable material items. This "plain folk mentality", one that emphasized the use of funds toward investment in land rather than consumer goods, was passed from generation to

generation and resulted, from an archaeological perspective, in misleadingly modest material and architectural remains.

“Excavation of a ‘Plain Folk’ Log Cabin Site, Meade County, Kentucky (Otto and Gilbert 1984:40-53), serves as a cautionary tale in the construction of one-to-one correlations between any social variable and its observable implications in the ground. Theories of this nature should be grounded properly in detailed, historically-specific research, research that allows for the inclusion of a number of explanatory variables. While South might view this concern with historical and social specificity as overly-particularistic and “scientifically” meaningless, it is no more than an attempt to construct a theory with the maximum amount of leverage, an effort to feel out the interpretive parameters of a given theory through the careful development of testable hypotheses.

In researching African-American ethnic identity it seems a mandatory precondition to deconstruct the concept of “ethnicity” altogether and approach it first as a theoretical construct to be built from historically specific explanatory variables. While low economic status appears to be a key variable in the development of African-American ethnicity. Other variables such as geographic location, the composition of the surrounding white environment, levels of political activism or gender may provide their own suites of hypotheses concerning ethnogenesis. Further, economic condition cannot and should not be viewed as the primary factor in the investigation of African-American ethnic identity.

Melvin Wade has commented that

Culture change... occurred as a consequence of changes in the total environment of blacks and whites transplanted to New England; it cannot ultimately be reduced to a simple equation based on the status of the black captives relative to the status of those who held them captive (Wade 1988:176).

The process of teasing out “ethnicity” from the archaeological record should be based on the intensive study of the “community relations” that formed the environment in which people lived rather than the construction of “simple dichotomies”(Stine 1990:41).

Marshall Sahlins has noted that

Any given intention may correspond to an indefinite set of cultural practices, and vice versa, since the intention is connected to the convention by a relative and contextual scheme of significances (Sahlins 197839).

Lucy Foster’s intentions, like those of enslaved Africans at Cannon’s Point Plantation, were shaped by her experiences as an African living in America and her interactions with the larger white community of which she was a part. To reduce her identity to only its African components, as through the interpretation of her ceramic assemblage, is to diminish her life and her struggle as an African-American. Lucy Foster was not a cowrie shell, she was not a blue bead, and she was not 40% serving bowls. She was a product, as are all individuals and groups,

of her environment. The “uniqueness” or “commonness” of Lucy’s condition as an African-American and how that condition may or may not have revealed itself in the archaeological record is best determined through a theoretically circumscribed analysis tested through tightly constructed hypotheses. Only then will Lucy Foster’s “African-American ethnicity” have true historical and, hopefully, archaeological substance.

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