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The Conference on Historic Site Archaeology Papers 1967 - Volume 2, Part 2

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**The Conference on Historic Site
Archaeology Papers 1967**

Volume 2, Part 2

THE CONFERENCE ON HISTORIC SITE ARCHAEOLOGY PAPERS
1967

Volume 2, Part 2
September 1968

HISTORICAL ARCHAEOLOGY FORUM - 1968
on
Theory and Method in Historical Archaeology

A Paper by Clyde D. Dollar with Comment
from Colleagues

Stanley South, Editor

Additional copies of this volume are available at
per copy (Part 1 and 2) from the editor
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HISTORICAL ARCHAEOLOGY FORUM

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INTRODUCTION

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While listening to Clyde Dollar's paper presentation at the eighth annual Conference on Historic Site Archaeology in Macon, I found myself alternately agreeing and then strongly disagreeing with various points he was making. A number of those present were taking notes, something that doesn't often happen at these gatherings. In discussing this later it became apparent that there were definite reactions to, and an interest in this paper. As a result, and with Clyde's cooperation in furnishing extra copies of his paper, the chairman contacted a number of members of the conference and requested that they express their thoughts on "Thoughts on Theory and Method in Historical Archaeology", using it as a springboard for their own ideas on this subject. The following section of this volume, therefore, is devoted to the presentation of his paper, the reactions and comments of various colleagues, and Clyde Dollar's rejoinder. Through this means a broader sampling of thoughts in the field can be combined relative to this topic.

The plan at present is to continue this HISTORICAL ARCHAEOLOGY FORUM as an annual section of the conference papers volume, utilizing a key paper on preservation, laboratory techniques, field methods, ceramics, theory, etc., as a pivot for thoughts of various conference members, and in so doing provide a broader range of ideas than those emerging from the conference papers alone.

Thanks are expressed to those who have participated in this first forum, and especially to Clyde for allowing his paper to become the target for the arrows of his colleagues, and providing a stimulus from which their own thoughts were forthcoming.

Stanley South, Chairman
The Conference on
Historic Site Archaeology

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Editor's Note

Because of the introduction of the HISTORICAL ARCHAEOLOGY FORUM in this Volume 2 of The Conference on Historic Site Archaeology Papers 1967, it was not possible to compile all of the papers from the conference plus the forum papers into one volume. Therefore, the papers presented at the conference have been presented as Part 1 of Volume 2, and Clyde Dollar's paper and the critiques and rejoinders constituting the HISTORICAL ARCHAEOLOGY FORUM have been combined in this volume as Part 2 of Volume 2.

Conference members who paid their \$3.00 membership dues prior to the publication of this volume will receive both Part 1 and Part 2. Any purchase of the volume after that time is priced at \$6.00, due to the added expense of issuing Volume 2 in two parts. Additional copies are available from the conference chairman.

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SOME THOUGHTS ON THEORY AND METHOD
IN HISTORICAL ARCHAEOLOGY

Clyde D. Dollar

Rosebud Sioux Tribal Council
Rosebud, South Dakota

Good morning, ladies and gentlemen - or afternoon, as the case may be. I regret very much not being able to be with you in person today, but time and distance have successfully interfered with my plans for coming to Macon, so with a rousing 'Washta How Toka' I send you greetings from the Rosebud Sioux Reservation in South Dakota and wish you a very successful and pleasant meeting.

A conference, such as the Eighth Annual Conference on Historic Site Archaeology, serves a number of important functions within our profession, not the least of which is the renewal of friendships and partaking of 'viele gemutlichkeit'. Along with these necessary activities goes the equally important (and sometimes equally enjoyable) function of disseminating information about sites, research, and new ideas.

The subject of my paper for this year's meeting falls in the category of 'new ideas' - or, more appropriately, I should say 'old ideas expressed in what is hoped to be a new way'. As you can tell from the title of the paper, I am presenting these ideas in the form of 'thoughts' on the subject of theory and method in historical archaeology, rather than axioms or theorems, as I feel that the subject is just beginning to develop and will require the consideration and discussion of all of us before a body of method for historical archaeology begins to solidify.

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Over the past eight years, I have become increasingly more convinced that researchers in the field of historical archaeology are encountering problems the solutions to which seriously strain the ability of traditional anthropological methods to solve. I think that it is time to give serious consideration to the recognition that there is a major difference in the concepts of methodology used in the excavation of a prehistoric site and those concepts necessary for use on an historical site. As I see it, the field of historical archaeology is coming of age as a distinct socio-scientific discipline (even duo-disciplinary in nature); we must, therefore, critically examine all aspects of the subject in order to arrive at valid new concepts for what is essentially a new discipline, and not necessarily borrow concepts and methodology wholesale from the existing body of anthropological thought. No doubt the argument of whether the historical archaeologist is an historian with a shovel or an anthropologist with a history book will not be resolved by this paper - if indeed a resolution is required at this time. If anything, the argument will probably only intensify. So be it, but even this will afford an excellent opportunity (perhaps even a necessity) for the historical archaeologist to do serious reflecting on just who he is and what he is trying to do.

My paper will be divided into two major sections, the first part being a very brief discussion of general concepts and limitations in the fields of history, archaeology, and anthropology, and the second being a presentation of ten theses for your consideration and discussion. When the paper is

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finished, I would invite you to record your comments on the reverse side of this tape.

It has been said that the historian works primarily with 'words', the archaeologist works principally with 'things', and the anthropologist deals with 'culture'. While the situation is in reality considerably more complicated, this statement does define the three main areas in which these specialists do most of their research: 'words', meaning historical documents; 'things', meaning archaeologically obtained data and artifacts; and 'culture', meaning the observable characteristics of human existence. The three are manifestly interrelated and inseparable to a great degree, yet the laws, concepts, and research methodology pertaining to each are by no means directly substitutable for the others - just as the application of the laws of optics to research on the human eye does not fully explain the eye's functioning. Very frequently the differences in the research methods used in these fields (history, archaeology, and anthropology) are quite subtle, and being subtle, these differences are difficult to clearly define.

The historian is trained to seek out written documents covering the subject of his research, peruse these documents, assess and weigh them for their validity and content accuracy in relation to the situation in which they were written. The very situation in which the historian works makes the obtaining of total historical objectivity an impossibility as he cannot escape his 'temporal present' entirely, and the historian who is sensitive to the responsibilities of his profession will readily admit this. We can no more objectively and fully 'know' what took place in the past anymore than we can physically visit that past. The very act of

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interpreting a source creates a subjective atmosphere which, at best, can only be partially, never completely, clarified. St. Paul must have had the historian, as well as the theologian, in mind when he stated that "we see through a glass, darkly".

The historian, therefore, works in a subjective atmosphere while constantly attempting to achieve objective findings. As an aid in penetrating the inherent limitations of his 'temporally present conception of the past', the historian uses two main research methods or 'tools'. The first of these is the logical process of deductive reasoning, or, going from the general to the particular, and the second is the application of tests for validity, or, the research processes of verification. Being essentially deductive in nature, the historical research framework is very legalistically and microscopically oriented, and verification of each step of the research process is therefore a basic necessity if that research is to be considered valid and usable.

Now let us turn our attention to the archaeologist and his sphere of activity in regard to 'things', meaning his work with archaeologically obtained data and artifacts. Perhaps I should state at this point that my concept of 'archaeology' does not include the premise that the technique is the exclusive property of the anthropologist. Indeed, for specialists in Classical, Ancient Near Eastern, and European archaeology to have had advanced training oriented along anthropological concepts is a definite rarity, and I am inclined to believe that the interchangeability of the words 'archaeology' and 'anthropology' which we so frequently practice in this country - is the result of an association that, while having served its purpose in the past, is now somewhat outmoded. I conceive of archaeology

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as a field technique only, a method of data control at a site, and within the framework of this conception, it is usable by a qualified researcher in any of the paleo-temporally oriented disciplines. During the past one hundred years or so, the field techniques of archaeology have been primarily used by anthropologists, and the character and interpretational aspects of its methods have been influenced quite naturally by this discipline. The paucity of temporal information at archaeologically investigated sites (almost without exception prehistoric in nature) has led to the development of statistical techniques centering around the use of cultural materials found at a given site as temporal indicators. Many of these statistical techniques involve the use of archaeologically obtained information translated into terms having a context removed from the physical matrix of the site itself, and while this practice has gained general acceptance throughout the field of anthropology for use at prehistoric sites, I question the validity of such techniques and data for applying to research at historic sites. This statistical usage I refer to as the 'extended' useage of archaeological techniques, and I am referring particularly to the practice of typology and seriation. Typology is defined as the process of arranging into groups those artifacts with a significant similarity of observable physical characteristics, and seriation is defined as the process of arranging these typological groups into certain patterns or orders in an attempt to determine temporal sequence or relationship. It has become a matter of increasing concern to me that these 'extended' uses of archaeological techniques do not seem to be able to produce totally distortion free information when tested at an historical site. The reasons

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for this, I believe, are inherent, but very subtle and profound, differences between the requirements for historical validity and the application of anthropologically influenced archaeological techniques at an historical site.

One of the reasons why 'extended' archaeological techniques are producing distorted and even erroneous data at historical sites is that the field techniques of archaeology are at best only a prolonged statistical sampling process of any given site, no matter how thoroughly the site is excavated, and historical sites have been almost invariably subjected to previous, extensive, and sometimes undiscernable, statistical samplings of various types, accidental or deliberate. In other words, field archaeology is only a statistical sampling of a statistical sample, and most generally not of the total population (statistically speaking). Another reason why 'extended' archaeological techniques tend to be unreliable for use at an historical site is that the recovery of data from an archaeological site requires the researcher to make an interpretation based only on what can be physically seen and measured at any one time and place within and during the site's excavation (the keeping of extensive field notes notwithstanding). This situation cannot help but place the researcher in a subjective position in relation to the data being obtained. And the third, and perhaps most important reason why the use of 'extended' archaeological data does not seem to produce valid and usable historical information at an historical site is that such data, especially seriation information, is not subject to verification, and its use in the historical research process may introduce an invalid component upon which other hypotheses then will be constructed.

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The dangers in doing this are obvious. In reality then, 'extended' archaeological data is constructed on the basis of a statistical sampling of a statistical sample arrived at through very subjective observation and is not inherently verifiable. This situation is somewhat comparable to typing and seriating the words of the King James english version of the Holy Bible in order to construct a hypothesis regarding the scholastic and intelligence qualifications of the original writers!

It is now time to discuss certain aspects of the anthropologist as he works with 'culture'. Anthropology has been defined as the study of Man (capitol M), and 'culture' can be defined as the physical and observable expressions of the way Man (again capitol M) lives. These are two meaningful and usable definitions within the framework of their specific discipline, and I have no quarrel whatsoever with their formulation. However, I would like to point out that the study of Anthropology is, by its very definition, the study of Man as a collective entity, i.e., Man's various cultures are seen, and therefore defined, by the anthropologist primarily using those expressions of culture that are the most numerous and/or most frequent. Individual cultures have what can be termed a 'center' (not necessarily referring to a geographical location) which can be defined as those cultural expressions reflected by the most people participating in that culture in the most similar manner at the same time. This 'center' of culture is therefore, the most vivid and easily recognized expression of that culture, especially when studied on the prehistoric level. In addition, specific cultures have what can be termed 'peripheral areas' (again, not necessarily geographical in location), being very similar to what we might call 'country cousins' in relation to the cultural center. The cultural expressions of

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these peripheral areas differ from those of the center somewhat, but not necessarily greatly, and therefore are only infrequently recognized on a prehistoric level. And finally, specific cultures have what can be called 'variants' within their cultural complex, and these 'variants' can exist at any time or place within the cultural center or peripheral areas. These variants include the 'odd balls', the 'beatniks', those who don't exactly conform to the cultural center to a noticable (and therefore bothersome) degree, as well as the thinkers, the explorers, and the inventors (those who will ultimately and profoundly influence the cultural expression of the surrounding cultural centers and peripheral areas). While these variants are sometimes difficult to overlook on a modern level, there does not seem to be any way to specifically recognize the existence of individual cultural variants on a prehistoric anthropological level through the use of archaeological techniques. Since the individual, or variant, exists only as a statistical expression within any general anthropological culture, the anthropologist uses archaeological techniques primarily as a means of further delineating the center and peripheral areas of the culture with which he is working. The variant of that culture simply does not exist for him because it cannot be recognized. His archaeological interpretations are therefore geared to the statistical definition of culture and he, over the years, has developed some very refined statistical tools, or, 'extended' uses of archaeology, to help him understand his findings anthropologically. It would seem to me, therefore, that the study of anthropology, in general, is incapable of producing techniques for the recognition of either specific actions or single cultural contributions of any given individual within any

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given culture (the study of modern culture excepted). Anthropological thought, as it has grown over the years, is basically inductive and 'macroscopic' in that the aim is to construct generalities based on observed particulars, and the verification of such generalizations can only be inferential and circumstantial. Furthermore, the statistical processes of an anthropological nature do not possess the inherent characteristic of being verifiable on a level or scale smaller than the cultural peripheral area level, and, by the very nature of the thing itself, the study of an historical site involves intense encounters with individuals (who, on an individual basis cannot help but reflect pronounced cultural variants) and not anthropological culture on a center or peripheral area level. The anthropologist deals with 'people' and the historical archaeologist deals with a person or persons. 'People' have cultural expressions on a cultural center and peripheral area level; a 'person' is basically a cultural variant, and must therefore be dealt with historically and deductively.

I am not entirely suggesting that the historian, when he takes to the field to excavate an historical site, wears a white hat, and the anthropologist, when he takes his trowel to an historic site, wears a black one; the situation is not nearly that simple. Nor am I suggesting that the anthropologist is not equipped to make a noteworthy contribution to the understanding of history as a whole. I am suggesting, however, that when the anthropologist is faced with the task of excavating an historical site, he is in reality facing a whole new discipline the problems relative to which he is probably not initially trained to understand or surmount.

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Continued and persistent pursuit of historical sites by archaeologists using anthropological concepts, as I have outlined them in this paper, will, I firmly believe, lead to a growing body of 'generally' accurate historical knowledge that will in actuality contain distortions in the particular, or detailed, areas of historical information.

Increased public historical awareness and improved funds for doing historical research during the past decade have combined to progenerate a situation wherein historical archaeology has become a 'fashionable' professional pursuit. Probably because of the current interchangeability of the words 'archaeologist' and 'anthropologist', those with archaeological experience have been approached by well intentioned groups and individuals desiring historical site excavation and development.

Quite naturally, there seems to have developed a general agreement that 'an archaeologist is an archaeologist' regardless of the type of site being researched or the academic discipline forming the background of the researcher. As a result, we have seen a rash of anthropologically excavated historical sites, and in all kindness to my colleagues in the discipline of anthropology, some of the results have been anything but happy (and, lest I appear too presumptuous, let me state that I am only too aware of the fact that "pot can't call kettle black"). The field techniques of archaeology are, by their very nature, totally destructive, and it is therefore imperative that the researcher using archaeological techniques, be he historian or anthropologist, have firmly in mind the scope and limitations of the disciplinary concepts from which he draws his interpretation of data. As a step toward possible clarification, I would

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like to present ten theses to further define what I feel are certain major methods, techniques, and limitations of historical site archaeology.

THESIS # 1:

Since the late 18th century, the number of different physical cultural expressions in the areas of artifact forms and variants within these forms has increased to the point where the complexity of the subject is almost beyond comprehension. This phenomenon has very important implications in the matter of using certain artifacts for specific dating purposes at an historical site. For example, there were in pre-Civil War America perhaps as many as 300 factories producing and marketing large quantities of cut metal nails. During the period from the Revolutionary War to the Civil War, probably dozens of patents (1) were secured for different manufacturing techniques were in use at one or more factories at the same time. One such factory, the firm of Messrs. A Field & Sons at Tauton, Mass., is reported to have been making "about 1,000 different varieties" of nails a year during

(1). to list only a few: Thomas Clifford (received a patent for a nail making machine in 1790; Jeremiah Wilkinson (cut nail invention in 1776); Jacob Perkins (secured a patent on January 16, 1795, for a nail making machine able to produce 10,000 nails a day); Ezekiel Reed (invented a nail making machine in 1786, which, in 1815, was producing one hundred and fifty million tacks in one year); Jesse Reed (secured a patent on a machine that made tacks at the rate of 60,000 a day during 1807); Samuel Briggs (in August, 1797, received the first patent for a nail making machine issued by the United States); Thomas Perkins (in February, 1794/I question the accuracy of this date/ received a patent for a nail making machine); Samuel Rogers and Thomas Blanchard (received a patent for a nail making machine from the United States in 1817); David Fulson (received a patent for a nail making machine during 1789).

Major source of information: "The Great Industries of the United States: Being an Historical Summary of the Origin, Growth, and Perfection of the Chief Industrial Arts of this Country" by Horace Greeley, Leon Case, Edward Howland, John B. Gough, et. al., published by the J.B. Burr Publishing Co., Hartford, Conn., pp. 1069 - 1078). 1874.

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the early 1870's (2).

As for the subject of ceramics, which seems to be a favorite target for attempts at seriation, an equally complicated situation exists. In the Staffordshire district of England, from which the vast majority of pre-Civil War Americans obtained their dishes, probably more than 400 different potteries were operating during the 1820's to 1850's (3) and producing merchandise of a remarkable range and variety of forms - most of which was simply duplicated from one pottery to another. In other words, not only was there a great range and variety of form in early 19th century English ceramics (sherds of which are found in great numbers at American historical sites) but there was also a great number of different potteries and individual potters making essentially the same designs. As if this situation were not confused enough, I have been able to define at least five different variables in the manufacture of ceramics any one or all five of which could conceivably effect the observable physical characteristics of each and every ceramic sherd found at an historical site (this subject will be covered in some detail in a forthcoming paper I am preparing on archaeologically recovered 19th century ceramics).

(2). Ibid., p. 1077

(3). Major source of information: The Penny Magazine of the Society for the Diffusion of Useful Knowledge, Vol. I, #1, March 31, 1832 - #48, December 31, 1832; Vol. IV, #177, January 3, 1835 - #240, December, 1835; Vol. V, #241, January 2, 1836 - #305, December 31, 1836; published by the Society in London, New York, Boston, Philadelphia, and Baltimore.

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Also touching on the subject of ceramic dating is the problem of the dating of manufacturing technique improvements. For example, in 1829, there was a significant improvement made in the glazing and firing techniques of certain English wares, and this improvement can be usually noted on a ceramic sherd with hardly more than a single glance. Unfortunately, we have no way at the present to know how many of the English potteries adopted these new techniques and how many continued the older processes and for how long. We probably never will entirely know much of this information as it was considered secret by the potters themselves and therefore did not frequently reach the pages and reports of the primary source materials of the period.

THESIS #2

While typological processes, in general, can be applied to any given body of historical site artifacts with a specified spatial and temporal limit (since this is only a grouping of artifacts based on similar or like observable physical characteristics, historical 'validity' as such is not a consideration), seriation processes, or the attempts to derive temporal data from within a typological pattern of historical site artifacts, have not as yet been proven to produce totally non-distorted historical data and therefore must not be used in the construction of historical hypotheses - unless of course, exteriorily known data can be used as corroborative evidence. I would cite two specific examples with which I am personally familiar. The first involves the archaeologically obtained buttons found during the excavation of the First Fort Smith (Arkansas) site by Mr. Jackson W. Moore, Jr., and myself during 1962/63. These buttons were typed and analyzed

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serially in several different ways, and the results of this seriation showed pronounced and unmistakable evidence sufficient to suggest that the greatest inhabitation concentration at this site occurred three years prior to the fort's having been built! In a similar example, the ceramics excavated at the Brigham Young House at Nauvoo, Illinois, (during the 1965 season) strongly indicated that the initial deposition of these artifacts occurred almost twenty years before Brigham Young arrived at Nauvoo and began the construction of his house. Clearly, something is wrong.

THESIS #3:

Every archaeologically recovered artifact from an historical site has two inherent dates: its date of manufacture and its date of deposition. On a prehistoric level, it is not possible to archaeologically distinguish between these two dates (dendrochronology being a possible exception); however, on an historical level, these two dates must be recognized as being an inherent and separative characteristic of the artifact itself, and therein lies the major challenge in the interpretation of historical site data as well as the primary stumbling block for the construction of non-distorted seriations of historical site artifacts.

THESIS #4:

The date of manufacture for every archaeologically recovered artifact from an historical site implies two separate dates: an 'alpha' date and an 'omega' date. The 'alpha' date refers to the point in time at which that particular style of artifact (not a particular recovered artifact) began to be manufactured, and the 'omega' date refers to the point in time

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at which the manufacture of that particular style of artifact ceased. Somewhere between these two dates lies the date of manufacture of each specific artifact of a given style found at the researched historical site. More often than not, these 'bracketing' dates, (the 'alpha' and 'omega' dates) are very difficult to pin-point, and the finding of a certain type or style of artifact at an historical site is not valid historical proof that that certain type or style of artifact's dates of manufacture have any relationship to the site in question. It is a matter of no small wonder and frustration to me to continue to discover more and more historical artifacts that do not as yet have an 'omega' date!

THE SIS #5:

Every prehistorical and historical site has a 'provenience', meaning its definable relationship to a temporal scale. The 'provenience' of any given site can be defined as the period of time during which any significant cultural expression can be discerned. Within the provenience period of any historical site, at least two, possibly more, separate and yet related time periods must be delineated for the purposes of data interpretation. One of these periods (and not necessarily the earliest, depending on the site's history) is the 'historic' period, i.e., the period of cultural expression (and deposition) with which the historical archaeologist is most concerned from the standpoint of recovery of historical information. The other temporal period, which encompasses but does not include the historic period, can be termed the 'alter' (meaning 'other') period (and may even include the prehistoric period, if any) and this 'alter' period may then be

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further divided and defined if necessary for convenience purposes. The important aspect to keep in mind is that research (either historical or archaeological) must not be limited to only the 'historic' period of the site being investigated. For the First Fort Smith site, the dates of the historical period were 1817 through 1834 (the period during which troops physically occupied the buildings of the fort) and the provenience period of the site was from 1817 until 1958 (when the shanty town overlying the fort site was burned in preparation for the excavations; since that time, artifacts 'produced' at the site have been insignificant in both amount and historical value). The dates for the historical period at the Brigham Young House site were the period of Young's occupation of the structure (1839-1846); the dates of the provenience period for this site extend up to 1963 when the last occupant of the house vacated the premises. The dates of the historical period of the General Custer House site at Fort Abraham Lincoln, North Dakota, were from 1873, when the General and his wife moved into the newly constructed building, until late 1876, when the General's widow moved back to her family home in Ohio. The provenience dates of this site extend through 1894, however, when the local settlers dismantled the (by that time) abandoned buildings of the fort, including the Custer House. The recognition of these two separate but interrelated periods at an historical site by the historical archaeologist is vitally important to the validity of the archaeologically recovered historical data and artifacts from that site as it means that most, or all, of the recovered artifacts must be presented as having come from both the historical and

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provenience periods of the site being researched - unless the researcher can present valid proof of the fact that these artifacts can be assigned to either of these two periods.

THESIS #6

Every archaeologically recovered artifact from an historical site has two definable locations in relation to the matrix of that site, and I will call these the 'locative' characteristics of an artifact. The first of these locative characteristics is an artifact's depth, or vertical, location in relation to the site's ground surface. The second is an artifact's plane, or horizontal, location in relation to the artifacts and/or features surrounding it. At first glance, the formulation of two locative characteristics for an artifact might seem a bit pedantic, and I must admit that I was of this opinion also at one time. However, experience and tests have caused me to somewhat alter my thinking on the subject. It seems to be an almost universal characteristic of historical sites that the artifact assemblage is a thorough mixture of historical and alter period artifacts. Historical sites, as a general rule, are very shallow and have been subjected to rather long and sometimes intense occupation periods. This situation has frequently resulted in artifacts from widely separated time periods being deposited together in a very shallow and mixed stratum. Under these circumstances, any attempt by the researcher to make use of the depth of artifacts in order to arrive at relative dating usually dissolves into utter chaos. The same confused, and generally invalid, situation may not necessarily exist if the horizontal locative characteristics of these

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same artifacts are considered, and, since the vertical and horizontal locative characteristics of an artifact are separable as they are not necessarily temporally related, then this procedure is quite permissible. At the First Fort Smith site, the only historical site at which I have been able to actually test this thesis for applicability, it was found that the different classes of artifacts, when examined from the standpoint of their vertical locative characteristics, suggested badly distorted historical data, i.e., incorrect relative dates, erroneous periods of construction activity and occupation concentration, etc. On the other hand, these same classes of artifacts, when examined from the standpoint of their horizontal locative characteristics, suggested very useful and quite valid historical and architectural interpretations, i.e., areas within the site where specific building materials had been used, the manner in which certain structures were demolished, areas of specialized usage such as living and leisure areas, etc.

Several interesting speculations arise when examining the possible causes of this valid/invalid relationship between the horizontal and vertical locative characteristics of an historical site artifact. First, the phenomenon might be a product of the semantics of the situation, i.e., the vertical positioning of classes of artifacts can only be visually expressed using what are essentially statistical methods and histograms (the 'extended' usage of archaeological techniques), whereas the horizontal positioning of classes of artifacts can be visually presented to the researcher on a map showing direct relationship of artifact with artifact and

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with/to co-existent features. And thirdly, this phenomenon might be the result of what was purely an isolated research situation, and, especially in view of the fact that this thesis has been tested at only one site (the First Fort Smith site), I would advise considerable discussion, caution, and a great deal more testing before the precise formulation of this thesis is accepted.

THESIS #7:

And finally, as something of a summation of the above discussed six theses regarding artifact usage at historical sites, I would like to present for your consideration the thesis that an archaeologically recovered artifact found on or in an historical site cannot be dated based only on the fact of its being found at that site, nor can an historical site be specifically dated by the artifacts found within the matrix of that site. It should be sufficiently clear by this time that variants in the manufacturing techniques of historical site artifacts totally destroy their value as specific dating tools for the historical archaeologist. If the historical sources do not supply specific dates for a site, then the techniques of field archaeology cannot (and must not) be trusted to accurately supply such dates.

THESIS #8:

Turning now from the usage of artifacts to a more wider view of the subject, I would state that historical archaeology must be architectural in orientation and reconstructive in both purpose and scope. With very few exceptions, historical archaeologists deal with areas that have been (or are) the sites of historical structures, as opposed to kill-sites,

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transient camps, caves, and other similar non-structured prehistoric sites. More frequently than not, historical sites are (or were) multi-structured, as even the humblest log cabin had one or more outbuildings for domestic or livestock purposes (a 'cabin' is defined as a single-room structure and a 'house' is defined as a multi-room structure - both being used for dwellings). The purpose of historical archaeology must be to achieve, insofar as possible, the goal of complete understanding of the history of any given site, and the scope of such research must be to include the recovery of all evidence of historical cultural expression at that site, including all architectural evidence. This situation requires that the historical archaeologist be familiar with such architectural features as prepared foundations, footings, pylong, walks, fences, wells, balloon framing, floor joists, wall bonding, fireplaces, porches, lintels, stoops, basements, cellars, barns, chamfering, drip lines, and steps - to mention only a few. In addition, the historical archaeologist must also be well acquainted with the many ways in which building materials, such as wood, stone, brick, and mortar, can be used. And not only must he be familiar with these aspects of architecture but he must also be able to recognize traces of these features from archaeologically obtained evidence.

At the conclusion of the excavation and documentary research, the historical archaeologist should be able to present a thoroughly documented history of the site prior to its excavation, a lucid description of the archaeological work accomplished, and a synthesis of the results, and it is in this section of the report, i.e., the synthesis, that the reconstructive aspect of historical archaeology becomes most apparent. Ideally, when both the research and report are finished, the site can be theoretically

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(or actually) reconstructed to the desired historical appearance, and reconstruction is defined as the building from new of most or all parts of a vanished historical structure or complex of structures (not to be confused with less inclusive terms such as rebuild, remodel, develop, etc.). The final report should be of such a nature that a competent architect can take the findings and, with very little purely architectural interpolation, proceed with the actual reconstruction of the site. I would like to make it emphatically clear at this point that the reconstructive aspect of the research and report holds true for the historical archaeologist's work regardless of whether the site will ever be actually reconstructed.

This is not the time to engage in a full scale discussion of the pros and cons of reconstruction, but I would like to make a few brief observations on the subject. I would be (and on occasion have been) the first to advise against the actual reconstruction of most historical sites. This recommendation is usually based on the premise that successful historical site development and interpretation is infrequently predicated on full reconstruction. In addition, historical reconstruction per se, unless grounded on a substantial funding basis, is all too often inadequate or unsatisfactory. However, it is disturbing to note the number of individuals (who may or may not be engaged in historical archaeology research) who chronically, and frequently without justification, deride any and all reconstructions of historical sites or structures. I think that these people miss the entire point of historical archaeology, and I would advise them to do some very serious soul searching as to why they are involved in historical archaeology in the first place, if indeed they are.

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In this reconstructive aspect, at least, the historical archaeologist takes on a far weightier and more encompassing responsibility than the anthropologist does in the excavation of a prehistoric site (I am certainly not suggesting that the one is more 'important' than the other). This increased responsibility and scope brings up the spectre of the length of time required to excavate and research historical sites. I would submit for your consideration that an incompletely excavated and researched historical site is far less desirable than no excavation or research at all. In my opinion, the reconstructive scope and purpose of this type of work requires a fully completed project, and I am highly opposed to the 'sampling' (not testing) of an historical site as this procedure introduces statistical unknowns into an already subjective situation. If an historical site is only 'sampled', and then all additional work neglected, or if a project is terminated prior to completion (such as the General Custer House site in North Dakota), then the validity of the results obtained is brought into serious jeopardy. An historical site can no more be halfway researched or excavated than can a structure be halfway built, and I think that it is high time that we stop using prehistoric site time/work experience factors to estimate the duration of an historical site excavation.

THESIS #9:

A considerable and basic dissimilarity exists between archaeological evidence for structures at an historical site and such evidence for structures at a prehistoric site. Historical structures, in almost all cases, were built according to patterns dictated by the thrusts, loadings,

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and stresses required to support the heavy building materials used in those structures, and herein lies the basic reason why the archaeological evidence differs from that of a prehistoric structure (the Meso-America and Mesa Verde/pueblo traditions excluded; it should be noted that not even an earthlodge, as a general rule, matches in intensity the structural loadings of a log house with a shingle roof).

Other differences between these two types of archaeologically researched structures include the use of prepared foundations, commonly used units of measurement for building dimensions, structures of widely differing functions built in identical or very similar manners, and architectural features generally unique to historical building traditions and styles (such as porches, steps, wells, outhouses, cellars, and fireplace foundations, etc.). Since historical archaeology is reconstructive in purpose and scope, the researcher must determine both that such features exist at his site and their method of construction. It is a point worth very serious consideration that there are certain archaeological field techniques used to obtain information at a prehistoric site which will actually obliterate vital architectural information when used at an historic site. For example, the practice of 'trenching along a wall line', i.e., excavating immediately adjacent and parallel to an historic foundation, will very probably destroy evidence for: 1). existence, depth, and configuration of a builder's trench, 2). original or historic ground surface, and 3). width of roof overhang (drip line). Incorrect excavation of a fireplace platform (foundation) may result in the loss of evidence for: 1). possible indications

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of floor level and its type of construction, 2). room divider or wall location, and 3). estimated maximum height of chimney, May I again point out that archaeology is a destructive process, and when doing historical archaeology, it is just as important to discover how the construction took place as it is to discover that it took place.

THESIS #10:

To 'identify' a site means to determine its temporal and cultural affinities, and to 'authenticate' a site means to trace the site's historical lineage to establish the authenticity of its historical association with specific individuals or groups. It is a function of historical archaeology to find (and present) evidence, develop hypotheses, and establish facts regarding both of these two aspects of site verification.

Site identification is a universal procedure common to all archaeological sites, historic or prehistoric, but site authentication is a verification function usually unique to historic sites. Unfortunately, site authentication is occasionally omitted in reports, but there are far too many myths, well intentioned but misplaced monuments, and outright pious frauds surrounding historical sites in general for the researcher to overlook the problem. The presence of an historical marker is at best only circumstantial evidence of a site's authenticity (the dignity and social position of the monument's sponsoring institution notwithstanding), and unless there is valid and usable historical evidence to prove the veracity of the monument's location, any previous historical identification of the site must be considered suspect. We are all no doubt familiar with

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'humerous' tales on this subject, but when all of these tales are collected and considered, their numerical implications become much more sobering than funny. For the historical archaeologist to neglect the authentication of a site being excavated makes him a party to the possibility of another historical 'fraud', and this is a serious responsibility indeed.

Closely tied with the problems of site verification is the historical research necessary into the 'come-down' periods of the site - yet another activity unique to the archaeology of historical sites. The tendency on the part of the researcher (and I know this from personal experience) is to unconsciously think in terms of the site's history as terminating with the close of the historical period being researched. It is almost axiomatic in site research that no site ever remains architecturally static once human occupation begins, and usually, the older a structure becomes, the greater will be the number and scope of these architectural changes. Therefore, the construction (or destruction) activity that occurred at the site between the end of the site's historical period and the end of the site's provenience period (what I refer to as a site's 'come-down' periods) vary frequently will be of major importance to the historical archaeologist as indications of this activity will be the first archaeological evidence encountered in a site's excavation. As a result, this 'come-down' evidence may go unrecognized or may even mask or confuse the archaeological evidence for the earlier periods of site occupation. In addition, it is in the unraveling of these 'come-down' periods that important architectural and cultural facets of the historical period of the site frequently can be discovered.

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This concludes the presentation and discussion of the ten theses, and in closing, I would like to summarize the major points covered in this paper:

1). It is time to give serious thought to the recognition of historical archaeology as a distinct socio-scientific discipline with a methodology designed to cope with the unique problems encountered during the excavation of historical sites;

2). Two of the major research methods used by the historian are the logical processes of deduction and tests for validity, and both of these aspects must be a property of the research at an historical site if such research is to be legalistically and microscopically oriented, and therefore considered historically valid;

3). The techniques of archaeology (which are not the exclusive property of the discipline of anthropology) are field techniques only, and any 'extended' use of these techniques by the researcher is grounded on a statistical basis too far removed from the possibility of verification to be usable in historical research processes;

4). The concepts of anthropology are oriented toward macroscopic inductive processes and inferential verification, and training in this field frequently does not prepare the anthropologist to cope with the problems faced when researching an historical site;

5). The ten theses are as follows:

i). since the late 18th century (in America), the number of different physical cultural expressions in the areas of artifact forms and variants within these forms has increased to a point where the subject is extremely complex;

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ii). while typological processes, in general, can be applied to any given body of historical site artifacts with a specified spatial and temporal limit, seriation processes, or the attempts to derive temporal data from within a typological pattern of historical site artifacts, have not as yet been proven to produce totally non-distorted historical data, and therefore, must not be used alone in the construction of historical hypotheses;

iii). Every archaeologically recovered artifact from an historical site has two inherent dates: its date of manufacture and its date of deposition;

iv). The date of manufacture for every archaeologically recovered artifact from an historical site implies two separate dates: an 'alpha' date and an 'omega' date;

v). within the provenience period of an historical site, at least two separate and related time periods must be delineated: the historical period and the 'alter' period;

vi). every archaeologically recovered artifact from an historical site has two definable locations in relation to the matrix of the site (the 'locative' characteristics of an artifact): the vertical location and the horizontal location;

vii). an archaeologically recovered artifact found on or in an historical site cannot be dated based only on the fact of its being found at that site, nor can an historical site be specifically dated by the artifacts found within the matrix of that site;

viii). the discipline of historical archaeology must be architectural in orientation and reconstructive in both purpose and scope;

ix). a considerable and basic dissimilarity exists between archaeological evidence for structures at an historical site and such evidence for structures at a prehistorical site, and certain archaeological field techniques, if used at an historical site, can actually destroy important historical evidence of an architectural nature;

x). it is a function of the historical archaeologist to find and present evidence, develop hypotheses, and establish facts regarding both site identification and site authentication.

Ladies and gentlemen, you have my appreciation for having to put up with a mechanical speaker instead of a human speaker (although I sometimes wonder which of the two is really the better), and my thanks for your interest in this paper. I wish you success in your conference and work and I will be looking forward to seeing you in person at some later time in the future. If anyone would care to make a comment on this paper, please feel free to use both the remainder of this and the reverse side of the tape. Thank you and good luck.

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Prologue to the Debate

Clyde D. Dollar

2

On the 9th of October, 1967, I received a call for papers notice from Stan South, Chairman of the Conference on Historic Site Archaeology, for the eighth annual meeting of the group. I accepted this invitation, and, for reasons rather complex, determined to verbally explore certain thorny paths of theory and method in historical archaeology -- a subject which has been of interest to me for the past several years. When it became clear that the press of new duties (I had assumed my present duties with the Rosebud Sioux Tribe only since October 1) would prevent me from making the trip to Macon, Georgia, to personally present the paper, I completed what was to be the first draft, taped it (in the very early hours of a new morning), and air mailed the tape to Stan South's residence in Wilmington, North Carolina. The tape arrived just hours before Stan's departure for Macon, Georgia, the Conference site. In a very real sense, then, the paper was almost not presented! Even as the tape was being played in Georgia, I was revising sections of it in order to better clarify certain points. The paper, in my opinion, still needs a bit of revising.

Within a few days after returning home, Stan inquired if I would object to copies of the paper being circulated to various other members of the profession for their comments, criticisms, remarks, etc. Stan also included a list of suggested names of persons to be contacted for this purpose. I replied that I had no objection to

this procedure, and requested only that I be given an opportunity to reply to these critiques in order to stimulate a worthwhile discussion of the subject. The choice of the persons to be contacted I very willingly left to the judgment of Stan, and the wide diversity of the critiques is a credit to his knowledge of the personalities and backgrounds of the members of this profession. Stan was supplied with the requested number of copies of the revised paper, and the verbal battle was on.

Fourteen individuals wrote thirteen critiques. In broad terms, the professional backgrounds of those writing the critiques are as follows: eight anthropologists, one ethnologist, four historians, and one 'unfinished' architect currently practicing anthropology. With the exception of this latter individual, all have had direct supervisor responsibility for excavation/research projects of considerable magnitude. And, with the exception of this same individual, all are highly competent, well respected, and have made significant contributions to their fields and/or professions. Whether they, individually, were 'pro' or 'con' in their reaction to the "Thoughts" paper has not altered my own personal high opinion of their stature.

These critiques are the work of men busy with the responsibilities of their various professions, and, as I understand the situation, the only restriction on the length of their critiques was the amount of time they themselves wanted to spend on the matter. Some critiques are therefore short and others quite lengthy. Length,

by itself, is of course no criterion on which to judge the quality of any research, much less a critique of another's research, but it does pose certain pragmatic problems when it comes to making replies to these critiques. I am sure that neither the budget nor the patience of Stan South, as editor of The Conference on Historic Site Archaeology Papers, would withstand a lengthy and drawn-out series of replies to each of the critiques. I have therefore tried to limit my replies to only those major points or statements I felt necessary to correct, defend, or refute. In certain of the critiques, the content of which justified the procedure, I took the liberty to adopt a 'point by point' type of reply by inserting numbers (thus: (4), (19), etc.,) into the body of the critique itself. This method both saves lengthy replies and also serves to bring the argument to bear on specific points. If the reader will make use of this opportunity to closely follow the discussions by referring to these numbers of the reply at the appropriate time in his reading of the critique, it should considerably increase his comprehension of what is going on — that is, assuming there actually is some comprehension to what is going on...

The arena of the verbal give-and-take is no place for the timid or fainthearted, and I was delighted to find that none of the critique writers exhibited these characteristics. However, there is a difference between disagreeing and being disagreeable, and certain of my colleagues chose to be the latter rather than the former. Haec olim meminisse juvabit...but here and today is quite

a different matter. I trust that there will be no cause for complaint if certain of these more vitriolic passages are answered in kind.

Now, on to the business at hand.....

COMMENT ON
 "SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY"
 BY CLYDE DOLLAR

Stanley South
 The North Carolina Department
 of
 Archives and History

In this paper the view is presented that the use of typology and seriation is not able "to produce totally distortion free information when tested at an historical site." (1) I do not think that the seriation technique has been thought to be totally distortion free on any type of site, Indian or historical. The fact that the seriation technique is not totally distortion free, however, has not prevented impressive results being obtained by use of this technique in Indian site surveys. (2) As to its use on historical sites, it is possible that under certain circumstances, carefully controlled data could produce information of value on historical sites, though I know of no specific instances where this has been tried with sufficiently controlled data. (3)

Dollar's Reply: (1) Misstated and quoted out of context. This mis-interpretation can be corrected by a careful reading of Thesis #2 - especially the first paragraph. I quote from a portion of this: "... seriation processes...have not as yet been proven to produce totally non-distorted historical data and therefore must not be used in the construction of historical hypotheses..."

There follows one qualification to the above quoted statement. Note that typological processes are not included within the framework of the sentence. For purposes of clarification in this Reply, I have underlined what I consider to be the key to the understanding of the quoted statement (i.e. the word "proven").

(2) How can such results on any site be 'impressive' if they cannot be verified?

(3) Stan looks at the literature of the profession in a somewhat different light than I. In my opinion, a list of such "instances" would be quite long, if not too impressive.

South's Rejoinder: The fulcrum point of difference here lies in our definition of the word "seriation"; I see this as a tool useful within narrowly qualifying criteria; Clyde's is apparently a much broader use.

I would not like to see a blanket rejection of the seriation technique as applicable to historic sites until it had definitely been proved invalid through trial. (4) Throughout this paper I get the feeling that the word "seriation" is used to apply to attempts to arrive at temporal date from typological patterns without the limiting criteria for use of the seriation studies that must be applied to produce valid data from the technique. (5) In fact, the buttons from Fort Smith were said to have been "typed and analyzed serially in several different ways, and the results of this seriation showed pronounced and unmistakable evidence sufficient to suggest that the greatest inhabitation concentration at this site occurred three years prior to the fort's having been built!" This statement clearly points to a misuse of the seriation technique, since the technique is designed to be used with data distributed over a number of sites under specifically qualified conditions (such as a site survey within a river basin). (6) Significantly the definition of "seriation" as presented in this paper omits reference to the fact that seriation data is areal in nature. (7) Another point to be made here would be that using the seriation technique in a valid manner (i.e. utilizing the limiting criteria of its design), and coming

Dollar's Reply: (4) The work with the buttons of the First Fort Smith produced information that was obviously invalid when compared to the known documented history of the site (see my reply to Moore, section No. 8). While this does not constitute grounds for a "blanket rejection" of all seriation techniques at historical sites (nor do I advocate as such), the situation does raise the question of the validity of results obtained by such means at other sites. It also suggests that the proof of the validity of results derived statistically lies with the researcher and must not be considered an inherent characteristic of the technique being used.

(5) Both 'typology' and 'seriation'--as I have used these terms in the "Thoughts" paper--are defined within the text of that paper. Any criticism of these terms, therefore, should be based on their definition, not just on a 'feeling.'

(6) The seriation techniques were not 'misused' at the First Fort Site--not even by the criteria contained in the second part of Stan's sentence.

(7) The areal nature of the seriation technique is inherent and therefore not necessarily in need of specific definition.

within three years of the known date, would be quite acceptable for the technique. (8) Of course one would not use such a technique designed to arrive at broad generalities for the determination of specific historic dates. The implication in this paper is that such mis-use of the seriation technique has been extensively used by historical archaeologists. (9) Impressive percentage relationship studies have been made with historical site materials, however, and valuable data recovered; these should not be referred to as "seriation" studies. (10)

There are a number of points made in this paper with which I would certainly agree. The duo-disciplinary (or multi-disciplinary) nature of historical archaeology; archaeological data as a statistical sample of a statistical sample; the subjective nature of the data recovered; the greater significance of the variant through more intense encounters with individuals as a characteristic of historical sites; the concern with specifics in historical archaeology, resulting in a decrease in reliance on some tools designed to produce generalized data; the greater reliance on written references for specific temporal determination of sites and artifacts; the importance of horizontal position of artifact types on historical sites as significant in interpretive value, are all points obvious to the historical

Dollar's Reply: (8) This statement by Stan is based on a misreading of my text. The error at the First Fort Smith was in fact a bit more than seventy (70) years, and for a site only 140 years old, this plus/minus factor is quite considerable indeed!

(9) Prudence prevents me from attaching a bibliography of such uses of seriation.

(10) Percentage relationship studies are indeed included in my definition of 'seriation', and I again raise the question of just how "impressive" can such results be when they cannot be (or have not been) proven to be totally distortion free.

archaeologist. (11)

This paper points out the generalizing nature of archaeological techniques as used by anthropologically trained individuals, and emphasizes that these techniques alone will not produce the specific data desired in the interpretation of historical sites. This is true, however, any archaeologist whatever his background, would surely utilize the specific historical data available to him in his interpretation of his archaeological materials, without a total reliance on the generalizing data accumulation techniques. (12) The critics of the anthropologically trained archaeologist frequently speak as though such archaeologists totally ignore specific historical data at their command, when to do so would, indeed, be the extremely short-sighted approach to historical sites. Such ignoring of specific historical data in total reliance on generalizing data collecting techniques, should indeed, be criticized, but how many historical archaeologists act in such a totally insular manner? This is not a matter of either/or; the point is, where the generalizing techniques functionally serve in the absence of historic references, then they can very well produce useful data; where historic references are available as to the site and the artifacts, then of course, these become a primary tool for temporal

Dollar's Reply: (11) I am glad that Stan recognizes these points as obvious; however, even the briefest glance at some of the other critiques contained in this dialogue should be sufficient proof that such points are by no means universally recognized as 'obvious'.

(12) I only wish that Stan's statement were true! Again, prudence...

placement of the site and the artifacts. (13) The element of common sense is a significant one in the excavation of historical sites; where history serves, use it; where a more generalized tool is called for, use it; where historical references pinpoint the artifact utilize this information; where references are absent utilize provenience, context, stratigraphy, horizontal position and any other technique that has proved or may prove to be a valid approach in the recovery and understanding of the data. (14)

It does not seem wise, therefore, to reject categorically the generalizing techniques to the extreme reliance on history, for too often historical documents do not answer the kinds of questions we are asking on historical sites, or they are absent entirely. (15) In such cases archaeology must be called upon to supply both the general and whatever specific interpretations

Dollar's Reply: (13) I agree. Unfortunately, the danger all too frequently encountered (and not recognized) is that the generalized information too easily becomes the basis for hypotheses revolving around historical explicitness. This is one reason why I wrote the "Thoughts" paper.

(14) I am torn between answering with a resounding NO! or a qualified yes. I have the initial impression that Stan is advocating a policy of report writing wherein the end product justifies the means of obtaining it, and yet, I know Stan's work to be above this approach. Rather than go into a detailed discussion of Stan's statement, I will instead make one of my own: use any method possible to obtain information about an historical site and its occupants - but use this data very carefully and very explicitly. In other words, if this data can be measured against the standards of accuracy for use as historical data then use it as such. If it cannot, then explicitly say so.

(15) The first part of this sentence is an overstatement; the second part is only too painfully true.

that come from the site. I am thinking specifically of John Goggin's study on the Spanish Olive Jar,* and similar studies that use both the generalizing techniques of anthropological archaeology and the specific knowledge of history to recover data of value in the study of the evolution of ceramic forms over a period of three centuries during the historic period. Goggin's study is an excellent example of the anthropological combined with the historical approach to archaeological data of the historic period, and although refinement of his interpretation will continue to take place as more information becomes available, his work stands as a valid monument of the study of material culture. I cannot see that the approach used by Goggin is invalid for use on historical sites because of its anthropological orientation! (16)

In thesis #1 the large number of nail manufacturing factories and ceramic factories of the early nineteenth century are seen as producing a confusing picture. For someone not familiar with the means of handling large numbers of artifact forms from archaeological contexts in order to derive the generalized data therefrom, this might indeed present a confusing picture. If Indian site archaeologists treated each pot as his working unit,

* John Goggin, "The Spanish Oliver Jar, An Introductory Study", Yale University (1960).

(16)

Dollar's Reply: I am not familiar with Goggin's treatise on Spanish Olive Jars and will therefore reserve comment on it until I have read the work. However excellent it might be as an "anthropological combined with the historical approach to archaeological data", it might still be an exercise in futility when it comes to using this data for specific dating problems found on historical sites. The historian, as well as the anthropologist, recognizes both the existence and importance of broad generalized trends in material (and other) culture. It is when these two specialist descend from the general to the specific planes and begin applying their knowledge to specific problems (of dating or what have you) that the credibility gap between what each will accept as evidence begins to show most. We dare not - at this stage in our historical archaeology research - use locomotive tools to take apart a watch!

they too, would be faced with a phenomenon difficult to handle and interpret. Although each of his pots may have been made by a different Indian, he treats them as types and forms and in this manner derives valid data as to their evolution. (17) The products of a large number of factories in England between 1820 and 1850 would be similar enough so that statements as to the evolution of the ceramic types and forms being made by them could be made. (18) If the historic data indicate that a particular variation as determined by a specific mode was manufactured by a certain factory, then the archaeologist would surely use this information. If this information is not known by him, however, he still can derive certain clues of significant value as to the temporal position of the site and the artifacts through a study of the evolution of forms through time. (19) True, this would not allow him to pinpoint the site to within three years of its date of occupation, but would allow him to arrive at a generalized period involved. (20) It is obvious that to ignore the historically known data which would allow specific dating of a site through artifacts for the sake of generalized techniques would be a mistake.

Dollar's Reply: (17) While this sounds good, and is a basic tenet of prehistoric archaeology, it has never been proven in an historical usage sense.

(18) Evolution in English ceramics made between 1820 and 1850 is detectable only on such a broad and generalized scale as to be unreliable (I am tempted to say worthless) for any dating of historic site artifacts (other, of course, than to "the 19th century"). I am writing these words in July of 1968; it is my hope that additional research into the field of such ceramics will bring about detectable and usable temporal characteristics. Until such time as this work can be done - with enough historical accuracy to be historically usable - I cannot consider ceramics as reliable specific dating tools.

(19) I ask Stan for proof of this statement.

(20) What happens when the information thus derived must be accurate to within three years if it is to be usable at an historic site?

In Thesis #2, typology is said to be "a grouping of artifacts based on similar or like observable physical characteristics," with historical 'validity' not being a consideration. In the creation of the artifact type historical validity is not a consideration, but in the concept is the assumption that there must be some valid correlation between archaeological types and those created by the manufacturers of the objects; and that types represent only an archaeologist's selection from a continuum. (21) Therefore, mottled-glazed creamware can be seen to fit the continuum from the 1760's through the 1770's as far as its appearance in matrix is concerned. It will not be found on sites of the 1740's or likely seen on sites of the 1790's, not fitting the continuum in quantity other than at a particular temporal range. (22) Historical evidence, of course, is utilized along with the archaeological data to establish this fact, providing the unique challenge of historical archaeology; the correlation between historical data and archaeological data to produce information of feed-back value in the excavation of other sites, and in the interpretation of the site and material being studied.

Dollar's Reply: (21) I would want a definition of the terms "valid correlation" before I would accept this statement at face value. There is a correlation, of course, but not of the same type, or perhaps even intensity, as that assumed as a correlation between prehistoric artifacts and prehistoric manufacturers. Here is an area in need of further exploration.

(22) While Stan is no doubt referring to an hypothetical situation, I would nevertheless still express doubts as to the universality of such a situation.

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Thesis #4 states that "the finding of a certain type of style of artifact at an historical site is not valid historical proof that that certain type or style of artifact's dates of manufacture have any relationship to the site in question." It seems to me that if an artifact is found on a site it most definitely has a connection to the site in relation to its date of manufacture. (23) It may be a bottle cap, a hub-cap, a mass of nineteenth century artifacts, or a single fragment of mottled-glazed creamware, but its date of manufacture does have a relationship to the site in question. All such artifacts have a relationship to the site, whether dropped by an Indian, a colonist, a Civil War soldier, or by the archaeologist. How did it get on the site? Why was it in a particular provenience? Was its presence the result of individual, or group activity? Was it out of context relative to its date of manufacture, and the dates of manufacture of the objects found associated with it? These and other questions would tend to indicate that any object found on a site is in a definite relation to that site. Such an archaeological context in relation to the site is not, of course, "historical proof"; but are archaeologists required to submit to the fact of "historical proof" to interpret an artifact's significance in relation to the site on which it is found? Of course he utilizes such

Dollar's Reply: (23) I am not talking about a specific artifact's dates of manufacture as having no relation to the site (of course it does!). If Stan would read a bit more carefully the phrase which he has quoted from the "Thoughts" paper, he would discover that I am talking about dates of manufacture of a type or style of an artifact having no relationship to the site at which a specific artifact of that type or style has been found. In other words (using an hypothetical case with mythical designations and dates). Ceramic Type XYZ was manufactured from 1782-1896; does the finding of a piece (or pieces) of Ceramic Type XYZ at a site date that site to the period of 1782-1896? I think not, and before anybody makes the rejoinder that such a proposition is self-evidently incorrect, I would advise that the literature of the profession be skanned to see how many times this error has already been committed!

proof if it can be found in historical references, but should we require that archaeological data furnish "historical proof"? (24)

Thesis #5 describes "provenience" as a period of time, My understanding of provenience is that it refers to the origin or source of a particular artifact or group of artifacts within the matrix of the site (25) The provenience relationships might represent temporal relationships, but not necessarily so; the determination of temporal data relative to artifact proveniences seems to me to be an important aspect of interpretation of an historical site, and to re-define provenience as "a period of time during which any significant cultural expression can be discerned", would seem to be clouding the issue. Obviously, a site that was occupied until the time the archaeologist began his work would have a long span of cultural materials accumulated on it. But should we re-define provenience to refer to that entire time span? The use of careful methodology designed not only to fix the position of artifacts in their vertical position, but to fix them within their matrix or horizontal provenience, can provide data for the separation of these objects in time and space. It is on this point that some archaeologists become bogged down in the excavation and interpretation of data from historic sites. They become involved with the pinpointing of an object within one inch of its vertical and horizontal position in the plowed soil or a mixed soil zone, which may prove impressive to students as a demonstration, but can seldom be demonstrated to have significance

Dollar's Reply: (24) Yes!

(25) The word "provenience" can also be used to refer to an event, or a chain of events, that occurred at a site in relation to the total historical 'temporal matrix' (if you will) of the site.

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commensurate with the effort put into such pedantic exercises relative to the interpretation of an historic site. It is the artifact types in matrix within a dated ruin level that are of significance, not only for the ruin being excavated, but for comparison with artifact complexes from ruins excavated at a later time. It is this process with which the archaeologist on historic sites concerns himself as far as his methodology is concerned. The process is not the goal, but merely a means whereby the understanding of archaeological remains is more successfully accomplished toward the end of interpretation of the broader events and processes of history and culture relating to the site, thereby increasing our knowledge.

The concern with changing forms in time and space through archaeological methods woven with the specific information of history, is a major challenge to historical archaeology at this particular time. Through the recovery of artifacts in matrix within dated sites we have contextual relationships between artifact types representing a span of time, the boundaries of which can be relatively assigned through comparative archaeology and historical research. (26)

When enough controlled site excavations with closed dates have been studied, and the data therefrom fed into the general pool of knowledge of those active in the field of historical archaeology, the fixing of occupation dates of historical ruins can be more easily accomplished through archaeology than is possible in these dawning days of historical archaeology. In years to come, through this approach to historic site artifacts; this combination

Dollar's Reply: (26) The data thus derived will be date of deposition data, and not necessarily either date of manufacturing or use period data.

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of archaeological with historical data; archaeologists will be able to utilize glass beads to fix the date of a site, they will be able to narrow the temporal range for ceramic groupings in context more accurately than we are now able to do, they will utilize bottles, glass seals, buttons, kaolin pipes, and other objects more effectively than is now the case; and this information will come through this process of combining archaeological and historical methods toward a fixing of artifact forms in time and space.⁽²⁷⁾

Thesis #6 states that "It seems to be an almost universal characteristic of historical sites that the artifact assemblage is a thorough mixture of historical and alter period artifacts." (28) Part of the challenge of a particular historical site is the discovery of depth deposits where layering has occurred through deposition on that site. Of course when the site is shallow throughout, such as short occupation nineteenth century fort sites might be, then attempts "by the researcher to make use of the depth of artifacts in order to arrive at relative dating usually dissolves into utter chaos." Who would try to stratigraphically study a shallow mixed nineteenth century stratum? Generalizations based on experience with such shallow sites as to the value of stratigraphy on historic sites generally would seem to be a mistake, for many historic sites yield stratigraphy and superposition, both of considerable value in the interpretation of the site

Dollar's Reply: (27) I agree, and can only look forward with expectation to that day! Right now, however, we are faced with the possibility of too many premature conclusions being jumped to (!) and this information then being used as part of the basic information for any 'feed back' which in turn will then be used to fix artifact forms "in time and space".

(28) The question to be considered here is not why should this mixing not occur but rather did it occur.

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and the temporal relationship of the artifacts found within it. (29)

Thesis #7 revolves on the word "specific", and makes the point that archaeological sites do not produce specific historic dates for the occupation of the site. Since the pivot of understanding for this statement hinges on "specific dates for a site," then the truth of this statement is obvious, since it is seldom that archaeology can be called upon to independently pinpoint calendar dates for a site. Who expects archaeology to independently supply specific historical dates? (30)

Thesis #8 emphasizes the reconstructive aspect of historical archaeology, and I assume by this that interpretive reconstruction through drawings, sketches, etc. would be within this definition. I seriously doubt, however, whether many archaeologists would be able to construct drawings that would be acceptable by an architect who was charged in restoration of an historic structure. His work would surely be utilized by the restoration architect, but restoration architecture is such a specialized area it would appear unwise for the archaeologist to attempt to make literal reconstruction drawings for the architect. This thesis also states that in this reconstructive aspect the historical archaeologist takes on a far weightier responsi-

Dollar's Reply: (29) The study of shallow and mixed nineteenth century sites, contrary to Stan's opinion, is an excellent testing ground for the generalization represented in Thesis #6. Consider this proposition: if nineteenth century, when excavated today (in the twentieth century) appear to be a thorough mixture of the artifacts, how would these mixed strata appear if they were left undisturbed (after deposition) and not excavated until the twenty-second century? Now apply this answer to seventeenth century sites not excavated until the twentieth century.

(30) I do, for one, if the archaeological data is to be used as specific historical data. Obviously, there are times when this is not possible, but this situation does not negate Thesis #7. For a discussion of my use of the word "specific" in the "Thoughts" paper, see my reply to Jelks, section #6.

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bility than the anthropologist who excavates a prehistoric site, implying therein, I suppose, that there is a greater element of reconstruction involved in historical archaeology than in Indian site archaeology.

However, reconstruction of Indian sites throughout the Southeast is being done in the form of dioramas, paintings, models, physical reconstructions, such as the earthlodge at Ocmulgee National Monument in Macon, Georgia, and the ceremonial center at Town Creek Indian Mound State Historic Site in North Carolina (where archaeology for the purpose of interpretive reconstruction as well as literal reconstruction, has been going on since 1937). On such projects there is a strong element of reconstruction involved, and since this type of interpretation is an integral part of the work of these anthropologists, we cannot properly claim that in the reconstructive element the historical archaeologist has a more encompassing responsibility. The reconstructive aspect is a function of archaeology, not historical archaeology alone. (31)

Dollar's Reply: (31) I agree that a reconstructive aspect is a function of archaeology, and not historical archaeology alone. However, and the Ocmulgee and Town Creek Indian Mound sites notwithstanding, the percentage of structured sites that fall under the heading of historical archaeology is vastly greater than those that can be called prehistoric. This situation alone should be sufficient justification for Thesis #8. As for the discussion in the "Thoughts" paper of the relationship between the archaeological report and the architect's use of it, I was describing the ideal conditions (and I specifically stated so in the "Thoughts" paper). Stan knows, as I do and many others, that these 'ideal' conditions are more often than not less than ideal, but this does not let us out from under the responsibility of making the most of what we have to work with. The restoration architect's use of the archaeological report will be limited by at least two factors: 1), the amount of architectural data recovered during the research, and 2). the historical archaeologist's ability to translate his architectural findings into architectural terms. Thesis #8 was certainly not meant to imply that the historical archaeologist must also be an architect (although this would be a definite asset!) in the sense that he draws the final plans for any reconstruction (as for my own ability in this aspect of reconstruction, it would take a great deal of persuasion, perhaps even pushing, to get me to enter any building for which I alone drew the plans!). However, be that as it may, the significant point of Thesis #8 is that the historical archaeologist must be the one to supply the architect with the facts of the situation (both archaeological and historical) and then the architect adds enough of the architectural 'unknowns' to allow the building to safely stand.

Summary

In this paper on theory and method in historical archaeology, archaeology was seen as a field technique only. As a field technique archaeology can be carried out by the anthropologist, the historian, or the classicist, or the interested layman. Walter Taylor is referring to this when he says:

There are also competent archaeologists who have had no specialized academic training, even no "higher" education at all. And these are often among the most capable. While it is probably true that the man with the broadest background of specialized training will obtain the better information, yet it is often the case that the non-academic "field man" with broad practical experience and less formal training will produce the better data. *

Notice that he qualifies his statement with "broad practical experience". There are a number of examples of this type of craftsman who have made contributions to the field of archaeology. However, I would agree also with Willey and Phillips who said:

Acceptable field work can perhaps be done in a theoretical vacuum, but integration and interpretation without theory are inconceivable.**

It is here that even the most experienced field man with a lack of formal training will most often fail to obtain the better information from his data. I would disagree, therefore, with the statement that archaeology is a "field technique only". I would say, perhaps, that archaeology as a field technique is a field technique only. There are field technicians and field technicians, some can interpret their data effectively and some cannot. Those with a

* Walter W. Taylor, "A Study of Archaeology", American Anthropologist, Vol. 50, No. 3, Part 2, July, 1948., p. 44.

** Gordon R. Willey and Philip Phillips, Method and Theory in American Archaeology. (Chicago:1958), p.1.

theoretical base underlying their knowledge of technique are archaeologists who can interpret their data most effectively; those without theory are practicing the ritual of archaeology as a field technique. Historical archaeology theory must be a fabric with a warp of sound common-sense archaeology woven with the woof of history.

This paper has pointed out certain generalized approaches that are not seen as valid when applied to historic sites, and has concentrated on other aspects traditionally associated with anthropology. As has been indicated, there are many points with which I agree, and these have been listed. Other points, however, have seemed to be over-stated, based on an apparent lack of understanding of the anthropological theory involved, or on a lack of experience with a wide variety of sites of the historic period. The impression is almost one that exists when an individual not thoroughly familiar with the Bible attempts to refute the theologians. This comparison however, could be said to be invalid in that what it does is to criticize the author of the paper for not being an anthropologist, and this would be an invalid argument.⁽³²⁾ However, the author may have been able to more successfully challenge certain anthropological concepts as applied to historical archaeology if he were able to demonstrate a greater familiarity with the concepts he has undertaken to criticize. It also appears that a wider background and experience with historical sites might have allowed the presentation of more generalized theoretical statements that would have appeared stronger in their broader

Dollar's Reply: (32) Thank you.

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applicability. As it stands, however, the paper is an interesting statement of ideas, many that are basic to historical archaeology, and would be little disputed by historical archaeologists, regardless of their background; others however, are as the author has said, "Some Thoughts", and these will stimulate other thoughts from colleagues. (33)

It seems to me that the archaeologist is concerned with the recovery of his data under controlled conditions from the matrix of the site he is investigating, arranging the data through typology and taxonomy relative to existing contextual relationships, and determining their dimensions and relationships in time and space. The historical archaeologist utilizes historical reference sources in this search for clues to the understanding and interpretation of patterned human behavior, as well as idiosyncratic behavior as reflected in the artifacts and other cultural remains of the communities and individuals he is studying. The historical archaeologist is concerned with the process that is history, "a temporal sequence of unique events, [and] with the process that is evolution, a temporal sequence of forms... [History] deals with phenomena as unique events, with reference to specific time and place; [evolution]...deals with classes of phenomena without regard to specific time and place. The one particularizes, the other generalizes."^{*}

Dollar's Reply: (33) How "...little (these ideas are) disputed by historical archaeologists, regardless of their background..." will no doubt come as a surprise to Stan.

^{*} Leslie A. White, "Kroeber's 'Configurations of Culture Growth'". American Anthropologist, Vol. 8, No. 1, (1946), p. 82.

As archaeologists, it seems to me that we are concerned with the identification and interpretation of data reflecting patterned human behavior. As historical archaeologists we utilize historical data, and in so doing we can often deal with the unique events of history as well as the generalized cultural patterns. We should not, however, discard all the tools designed for obtaining generalized data merely because some of these may not apply when dealing with specific historical sites; nor should we fail to utilize the wealth of specific historical data that is available to correlate with archaeological discoveries. We should, rather, utilize any approach that will allow us to add to our knowledge in the most effective manner; through the many faceted discipline of historical archaeology.

The concern with artifact types and forms as a means to an end is for the purpose of arriving at interpretations more accurately reflecting the developmental sequence that occurred on the sites we are investigating; and the pivot of this understanding lies in the determination of contextual relationships in time and space.

Archaeology as a technique can be practiced by craftsmen from a variety of backgrounds, but the interpretation of the data so recovered must be based on a firm theoretical base. The fabric of this base must be woven utilizing those concepts and methods that are found useful in answering the questions historical archaeologists are asking, regardless of the origin within a professional discipline of these concepts and methods. Useful concepts, methods and tools from Classical archaeology, architecture, physics, chemistry, biology, zoology, anthropology and history should be

utilized as they are found to prove helpful in our search for knowledge through historical archaeology. Any one approach must not be over-emphasized at the expense of warping our understanding of our archaeological data in our expositions on method and theory in historical archaeology. (34)^{*}

Dollar's Reply: (34) In the above four paragraphs, Stan has written an excellent statement on historical archaeology, and his thoughts are deserving of much serious study. In my opinion, Stanley South, in his Critique of the "Thoughts" paper, has added considerable depth to the professional discussions on the subject of theory and method in historical archaeology, and our entire profession should be indebted to him for his interest, energy, and time expended in getting these discussions underway.

*
In order to avoid being influenced by the other critiques this paper was written in December 1967, with the final draft completed January 15, 1968, and submitted to Clyde for rejoinder before any other critiques were received by the Forum chairman.

CRITIQUE OF THE PAPER BY CLYDE DOLLAR,

"SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY"

Merrill J. Mattes, Historian
National Park Service
San Francisco, California

1. This seems to me to be a document worth serious consideration, setting forth perhaps for the first time, however imperfectly, a set of principles that might govern practice in the field of historical archaeology.
2. If in fact there exists previous papers which attempt to do this same thing, then the element of originality is lessened; however, I know of no such papers.
3. While archaeologists who have been trained in anthropology rather than history may resent the suggestion that their techniques and their thought processes, even, are inadequate to the requirements of historical archaeology, I would say that sulking is not the answer. Rather, I would think that they should either rebut Dollar's manifesto in detail, item by item, or concede that some refinements of technique and thought-processes are obviously needed, then go ahead and adjust. As a non-archaeologist who has nevertheless been exposed to a lot of sophisticated earth-moving projects, it scarcely seems debatable to me that historical archaeology does have peculiar and different problems. I presume there are some courses and credits in this esoteric, much-orphaned, much-maligned and in any event experimental field; but there should be more of same.

4. Dollar may well be in error on several points, from the professional digger's point of view. Perhaps he exaggerates the limitations of the usual archaeological techniques. Perhaps he has coined some phrases like "distortion-free" which may confuse rather than enlighten. I think his paper could be re-written to make it more compact (although I like his summary at the end) and it could be toned down to make it less bumptious, so to speak. But, I think Clyde Dollar is to be commended for coming up with some incisive observations about the limitations of this alleged craft, and a tentative set of principles. The fact that there are deficiencies and debatable points does not weaken the merit of this paper as a thoughtful challenge.

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Reply to Mattes

Clyde D. Dollar

Merrill J. Mattes is uniquely suited to be able to see the 'trees within the forest' in this historical archaeology theory controversy. He is one of the most respected of the American West historians, and his approach to history is that of a critical researcher, not merely a 'story teller'. Mr. Mattes brings a keen intellect and a level head to the arguments so far presented in the other critiques, and he should be listened to and his remarks weighed with this in mind.

I am not the first to refer to data as being 'distortion free'; for those who would learn more of such phenomenon, I suggest that they read Plato's Republic, Book VII.

...and in the future, Merrill, I shall try to be less 'bumptious'...

COMMENTS ON CLYDE'S PAPER

Jackson W. Moore, Jr.

National Park Service

It is ironic that less than ten years ago several of us made the observation that historical archaeology needed some bona fide historians in the field. Most of us were in full agreement at the time; straight prehistorians expressed general agreement as often as polite indifference. Today, with more "cross-over" of non-specialists from the prehistoric field who find historical archaeology "fun" there seems to be some resistance to historians wielding trowels. Why? Is it that, because of an historical accident anthropologists feel uncomfortable breaking ground with others than their fellows? This is a situation Europeans have always lived with comfortably! What is so different about Europe, or for that matter about North America? Clyde is correct when he states that archaeology is a technique, not a sub-field of anthropology (prehistory, in North America, IS a sub-field of anthropology, of course). It has always been applied by geologists, paleontologists, classicists, and (in the Old World) historians. Since each has its own body of problems, each naturally has its own body of methodology. It also follows, I suspect, that they each have their discrete traditions of smug conceit and condescension toward other disciplines! The major legitimate concern that I would recognize is where two or more multi-disciplinary problems would occur, juxtaposed, on the same ground. Again, this is a situation which Europeans have long been adjusted to. Since we have only now had to recognize

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the existence of the problem, I would recommend some dialogue with the Old World. (1)

I do not feel that it is necessary to develop a separate new discipline, nor does Clyde explicitly recommend. His introduction of new terms where satisfactory terms now exists is suggestive of an incipient jargon for a new discipline, however, and this leaves the door ajar. (2) With all due humility as an anthropologist, I suspect that most of us have studied more history, American and otherwise, than historians have studied anthropology. (3) (Wilfred Logan has observed that the historian, focusing his attention on unique, non-repetitive events, and/or individuals, does not seek the same kind of regularities in the universe that are sought by the anthropologist. This makes for a credibility and communications gap. One historian referred to Toynbee and to anthropologists and sociologists as verbal "web-spinners," whose theories had no more substance than a cob-web. With this attitude,

Dollar's Reply: (1) Some dialogue is in the process of taking place. See Iain C. Walker's Critique of the "Thoughts" paper and my Reply.

(2) Every discipline has coined its own terms to meet the problems encountered by those working in that discipline. As early as 1950, anthropologists had already coined a name for the archaeological research on historical sites (the term, by the way, was "garbage can archaeology"). Beyond the fabrication of this succinct description (no doubt tempered with fondness) of historical archaeology, most anthropologists chose to ignore what must have appeared to them to be a somewhat brazen approach to the Study of Man. Hence (in general), no new set of problems was recognized to exist (by most anthropologists) and therefore, no new terms were coined to adequately describe the problems that were in fact being slowly recognized (Smokey should know; he and I frequently discussed these problems back in 1962/63, and not a few of the terms used in the "Thoughts" paper date from those discussions). This is not to say that many terms already in use within the field of anthropology could not be, or have not been, used when describing various facets of historical archaeology; it is to say, however, that I think additional terms might be of considerable value in describing new concepts in historical archaeology. One particular point that comes to mind is the historian's approach to an artifact (see Thesis Nos. 1 through 7).

(3) This is, unfortunately, quite true.

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most students of history would not be stimulated to expose themselves to anthropology courses.) This is undoubtedly changing now that anthropology has become an "in" instead of a "what" subject on our campuses. In the event that such a new discipline does evolve it will necessarily have to include both history and cultural anthropology. Then, as now, it will still be necessary to call on specialists (including antiquarians) for advice. (4)

Dollar's Reply: (4) As a bit of an aside, I would like to call the reader's attention to a work that will be of considerable value to all of us who are involved in historical archaeology (unfortunately, this work was not available to me when the "Thoughts" paper was written). I am referring to Appendix A, "Historic Objects as Sources of History," in Carl P. Russell's Firearms, Traps & Tools of the Mountain Men, Alfred A. Knopf, New York, 1967. In this section, (which should be required reading for U.S. history and anthropology undergraduate survey courses), Dr. Russell quotes a Dr. Carl R. Fish as follows:

The first duty of the archaeologist is to discover such material (artifacts) and to verify it; the next is to secure its preservation. . . Then comes the task of studying it; classifying it, arranging it, and making it ready for use. At this point the function of the archaeologist ceases and the duty of the historian begins; i.e., to interpret it, and to bring it into harmony with the recognized body of information regarding the past. It is not necessary that different individuals in every case do these different things. . . nearly every historian should be something of an archaeologist and every archaeologist should be something of a historian. . . When the archaeologist ceases from the preparation of his material, and begins the reconstruction of the past, he commences to act as an historian and has to call up a new range of equipment, and a new set of qualifications (underscore is by Dr. Russell).

This quotation is taken from an essay by Dr. Fish entitled "Relation of Archaeology and History" which was first read before the Wisconsin Archaeological Society at Madison, Wisconsin, on July 29, 1910! Yes, that's right, 1910. And more than fifty years later, most historians don't even know what the techniques of archaeology are, much less how to apply them!

The thesis that anthropologists "extend" archaeological techniques when they apply statistics (Clyde applies them very well, incidentally) and that such extensions distort the data is difficult to accept. (5) There is little purpose in recovering cultural material if you don't do something with it. I do agree that, if such material will not solve a problem or otherwise expand your knowledge of the site or at least corroborate your conclusions, it is futile to apply statistics to them. (6) If you begin a project with few unknowns, your responsibility is primarily to those unknowns, not to demonstrate that you can play the game! In other words, if you have documentary evidence which tells you who occupied the site, when he came and left, what he did there, and the artifacts are also known, then the problem is probably architectural. The main value of the artifacts are to confirm the inventory or the inventory sequence. In this case, only a field school situation would justify the application of statistics. On the other hand, if some of the artifacts are inadequately known, such "extended" techniques should assist in fixing their temporal range. As for distortion, I must take umbrage. The buttons at Fort Smith may have demonstrated the lack of need for seriating them, but I do not agree that any data was distorted. (7)

Dollar's Reply: (5) Not the "data" but rather the results.

(6) On the contrary; such a situation might offer an excellent opportunity to test the validity of results obtained using statistical methods. This is at least one way in which all researchers in this field could learn to verify some of their research techniques.

(7) Smokey Moore is one of the few Critique writers with whom I have had the privilege of discussing both the "Thoughts" paper and their critiques. The following sentences of his critique have been discussed with him, both by letter and in person, and while this does not imply full agreement between the two of us on some of these points, we do at least understand a bit more of what the other is trying to say. A summary of my points will be presented in the next numbered paragraph of this Reply.

This was a site with known dates, personalities, units, and drawings (inexact). The button inventory indicated that members of the 1st, 2nd, and 3rd Artillery Regiments occupied the site during 1812-14, with smaller artillery units present by 1805. There were, of course, NO artillerymen at Fort Smith I! A company of the Rifle Regiment began construction in 1817 and were absorbed by the 7th Infantry Regiment in 1822. Both of these latter units brought in buttons of the 1810/16-1821 periods. An 1821 sketch by Seymour depicts two men in a frontier adaptation of the uniform of 1810-12. Direct observation strongly suggested that these troops were attired in hand-me-downs from surplus stocks. Even so, a seriation of button types was charted. According to the chart we should look for some significance within the following dates:

1760-1784 (none), 1784-1800 (none), 1800-1812 (none), 1812-1820 (construction in 1817), 1830-1840 (construction of Fort Smith II begun in 1839). When Ed Bearss' historical report was completed, it revealed a complaint by Colonel Arbuckle, 7th Infantry, that his troops were provisioned at New Orleans with uniforms which were ". . .completely inadequate. . . ." This, together with what was already known, confirmed the direct observations. The seriation served no purpose but practice. But, did it distort? If we had been without documentary evidence, I believe this seriation would have been invaluable. (8)

Dollar's Reply: (8) One fact that should be called to the attention of the reader is that my First Fort Smith Report (the synthesis of my own historical research, the historical research of Mr. Ed Bearss, National Park Historian, and the 1958-59 and 1962-63 archaeology) was not written until more than three years after Smokey finished the field excavations at Fort Smith and wrote his field report. Smokey's above statements, therefore, were written with this time lapse as a background and also without a thorough

(8 cent.'d.) study of the First Fort Smith Report (which contained more detailed studies of the research problems at Fort Smith, including the buttons). In summary, the 'button research' on the First Fort Site suggested the following:

A. The site could have been occupied (during historic times) as early as 1760, and probably was occupied as early as 1784. Probably the greatest number of men were assigned to the fort during the 1814-1815 period, and the occupation ceased probably as late as just prior to the Civil War.

B. In contrast to the above seriation results, historical data showed that the fort site was first occupied (during historic times) on December 25, 1817, that by far the greatest number of men were assigned to the fort during the 1822-1824 period, and that the fort was finally abandoned in 1838.

C. In addition to the erroneous data supplied by the statistical 'button research', as outlined in A above, the same techniques suggested that a sizable proportion (probably as much as 25 percent) of the fort's total assigned troops were from artillery units (with the 1st, 2nd, and 3rd Artillery Regiments, plus a goodly smattering of other light artillery units, being represented in the button artifacts), and that other units, such as the 1st, 2nd, and 3rd Rifle Regiments, various Infantry units, General Staff personnel), and even several Naval units were also present at the fort during its historic occupation period.

D. Historical sources show that the fort was actually occupied only by troops of the Rifle Regiment (in 1815, all four of the Rifle Regiments were reduced to a single Rifle Regiment). Evidence for personnel from the other units (as suggested on the basis of the buttons alone) is not only totally lacking but even denied (through the absence of such unit information on the official personnel returns from the fort).

The reader can readily see that a discrepancy of considerable degree does exist between results of the statistical 'button research' and the known historical data. There are (as Smokey points out) historical reasons why this wide gulf between statistical research and actual fact do exist (in 1822, the new commander of the fort arrived with a sizable increase in personnel strength--all apparently equipped with considerably outdated uniforms), and the discrepancies actually serve to point out this fact. However, (and this aspect of the situation is particularly disturbing to me), what conclusions could have been drawn from the statistical research on the buttons if the historical data had been incomplete or unknown? The answer to this question is something to think about. . .

Clyde points out correctly that it would have thrown us off at least three years. (9) If the prehistorian could believe that this indicated the reliability of his own seriation charts, what elation he would feel! Even documents, written forthrightly and witnessed by others, can be mistaken. Historians frequently resort to a "consensus" for verification!

The complexity of historical artifacts is every bit as great as described, but I disagree that it is as debilitating. Clyde again referred to Fort Smith, which was a very badly disturbed site; in a full decade of historical excavations I judge it so bad as to be unrepresentative. (10) Between the written records and the physical remnants of the foundations, our knowledge was sufficient, without the artifacts, to solve most of our problems. (11) The artifacts did suggest a sequence of inventories for the different periods of occupations. For the problem which the artifacts could help to solve it is not necessary to know all of the things which Clyde bewails the ignorance of. If we know that a certain ceramic type was manufactured between 1819 and 1840, that its origin was in Leeds and that it appealed primarily to the lower-middle income groups, then we know quite a lot! (12)

Dollar's Reply: (9) Incorrect. The error was more in the magnitude of 70 years.

(10) I agree with Smokey: Fort Smith was 'unrepresentative' (I sincerely hope!!). However, Thesis Nos 1 through 7 were certainly not based on observations made at a single site! Or even a handful of sites, for that matter.

(11) I disagree. See the last chapter of the First Fort Smith Report.

(12) Correct. We would know probably more than what the historian, using historical sources alone, could probably have discovered--at least at this stage of research on nineteenth-century ceramics.

The fact that it could have also been manufactured in Staffordshire, Kent, Scotland, or Brittany is not germane to the problem as defined at Fort Smith.(13) If, on the other hand, less were known about that ceramic type than is the case, then one of the problems would have been to make the circumstances of the site shed light on that type. One might even set up a series of site excavations selected to trace the type to its point of manufacture, or even to determine which ports it was carried through in order to establish a pattern of commercial "flow." I cannot foresee problems of this type ever becoming a concern in the National or State Parks, but they might very well provide problems for university field schools and research grantees.

Without wanting to "nit-pick" too much, I would say that the statements about central and peripheral culture areas is out of date. (14)

Dollar's Reply: (13) . . .but this information is of major importance when constructing a ceramic seriation at any given site (or group of sites). If the alpha and omega dates of each variant of a ceramic type (to be included in the seriation) that was manufactured in each pottery of each of these districts were known, and if each of these variants that were manufactured in each of the potteries of each of the districts could be unmistakably recognized, then, and only then, would the ceramic seriation produce historically valid results.

(14) I am not in the least talking about geographical areas, and this admonition was stated twice within the paragraph discussing this concept in the "Thoughts" paper. For obvious reasons, this is an historian's view of an anthropological concept (not, however, based on a total ignorance of anthropological writing on the subject), but to pass it off as "out of date"--without presenting 'newer' information or at least 'newer' reference material--is a somewhat dubious and pedantic approach.

When he lists those "odd-balls, kooks, etc." who don't conform, he is listing the thinkers, the explorers, the inventors, etc. who will ultimately and profoundly influence the cultural expression of the surrounding centers and peripheral areas! (15)

Anthropologists have differed from their kindred in the social sciences for scores of years on the question of cultural determinism and the "great man." Regardless of who is most nearly right in his assumptions, it is futile to expect an anthropologist to discover a previously unknown great man, although he might well be able to infer the occurrence of a great event!(16)

The fifth thesis is weak, as stated, because it is incorrect.

"Provenience" is used here to mean "association." Occasionally I do this myself, and perhaps other historical archaeologists slip on this mental shorthand also. Such association must follow an analysis of the provenience. From here we go on to real trouble. It should not be necessary to coin a new jargon to express the problems encountered in a multicomponent site. He presents the situation in the form of a historical site which the investigator needs to excavate in order to solve his own research problems,

Dollar's Reply: (15) Smokey, and others, apparently read this section of the "Thoughts" paper with the idea that I was being, 'snide' or sarcastic. It certainly was not my intention to be so interpreted. To me, the 'beatnik,' and the other colloquial designations given to those members of our society who do not for one reason or another conform to what is expected to be conformed with, are as much a rightful (and contributing) member of our society as anyone else in that society (or culture, if you will). The historian, and the anthropologist (in general) certainly recognizes the contribution the non-conforming element makes to the whole, and I personally would not deny this cultural element either their right to do this or their recognition for having done it.

(16) Correct--partially. Does the anthropologist have the academic 'right' to superimpose his own anthropological concepts on a site that is historical in nature and for which techniques (and standards, if you will) of research have already been worked out? Is the 'anthropological concept' of an historical site concerned with individuals and their actions or only people and their cultural changes?

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which underlies another occupation, is superimposed on a prehistoric site and may even overlap another site. Here we are confronted with the situation which is a common occurrence in the Old World, especially Great Britain, but makes our blood run cold. Should a historian who is not knowledgeable of the principles of ethnology or social anthropology damage a site which can throw light on the processes of acculturation? Is it any different for an anthropologist to rip through an experiment in communal living or a little-known battlefield because it is obviously "late?" On the other hand, could any of us adjust to a system which might require us to submit an application to some sort of interdisciplinary tribunal in order to investigate a multi-component site? Or would we negotiate "gentlemen's agreements" with known scholars interested in other aspects of such a site? (17)

Thesis Six is really part of Thesis Five, since the "locative" characteristic as described is really part of its provenience. What Clyde says here is defensible to some extent, assuming that all historic sites are as badly disturbed as was Fort Smith I. (18) I repeat that, in my experience, most historic sites are not that badly disturbed. The reappearance

Dollar's Reply: (17) Smokey is discussing Thesis No. 5 in terms which, quite frankly, leave me (to use a term perhaps native to my region) 'buffaloed!' I am trying (in Thesis No. 5) to suggest a terminology for use in reports and other means of communication and I am NOT even remotely suggesting that one period is more 'important' than another at any given site. Nor, in this thesis, am I trying to define a 'new' concept—other than to suggest that the historical period of an historical site is really only a portion of the 'temporal provenience' in relation to the total 'temporal provenience' of the site. The multicomponent site is something we all have to live with as best as we know how; perhaps we can at least begin to learn this living by defining the basic aspects of the problem in terms most of us (and hopefully all of us) can one day understand. The phrase 'alter period' is indeed awkward (see my Reply to Walker), but I feel that we need some designation to highlight the fact that the various periods at any given site other than the 'historic' period are just as important to the research on the site as the 'historic' period. Does anybody have a suggestion for a word to replace 'alter'?

(18) Incorrect assumption. I didn't assume this, and Smokey should not have assumed that I did.

of the term "alter" suggests that he is going to stay with his postulate that only the portion of a site which is germane to his problem will be considered as "culturally expressive." (19) I cannot give support to this contention. (20) Where the occupation level is too shallow to allow for stratigraphic comparison or by arbitrary level, as is often the case in historical archaeology, you simply don't do it. One must, of course, consider the horizontal locations of artifacts if they are to be used to indicate probable use patterns. One may illustrate such patterns by one of several ways (I don't like histograms either), such as isometric drawings, plan drawings with clusters indicated. Such an approach was used at Fort Frederica to indicate three separate work areas in a single room associated with the King's Magazine.

Dollar's Reply: (19) I have no idea where Smokey got this idea. In refutation, I quote from the "Thoughts" paper:

The recognition of these two separate but interrelated periods (the historical and the alter periods) at an historical site by the historical archaeologist is vitally important to the validity of the archaeologically recovered historical data and artifacts from that site as it means that most, or all, of the recovered artifacts must be presented as having come from both the historical and provenience periods of the site being researched—unless the researcher can present valid proof of the fact that these artifacts can be assigned to either of these two periods. (Quoted from the discussion of Thesis No. 5)

In other words, I am suggesting that, barring a rare and verifiable situation, the researcher cannot assume that artifacts found from the site's provenience period belong only to the historic period of the site. Let's stop this nonsense of presenting page after page of drawings/photographs labeled "Pipes (or whatever) from the Fort Fearless Site (1842-1867)" if in fact the site is known to have been occupied by someone—not necessarily troops—from 1830 to 1912 (Fort Fearless, by the way, is a mythical site, and needless to say, so are its dates).

(20) The contention is incorrect, and therefore I cannot 'support' it either.

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Thesis Seven concludes that, for reasons described in the first six theses, artifacts are inadequate for use as dating tools at sites where documentary evidence is lacking. (21) My responses to the first six theses were largely refutative. I believe that artifacts can throw a great deal of light on a site. And if documentary evidence is indeed lacking, then such light as the cultural material can shed is all of the illumination which that site will receive! The archaeologist is still left with the responsibility of interpreting his site to the best of his ability and in accordance with such data as he was able to recover (22)

It may be another accident of history that most historical archaeological projects have been oriented toward architectural reconstruction/restoration. The field was pioneered by the National Park Service, several state agencies, and some well-endowed private foundations. Unlike most prehistoric projects, the goals were pragmatic. Scholarly considerations were secondary. I see nothing in this to indicate that goals more ephemeral than architectural or restorative should not be undertaken. To be sure, the fullest possible grasp of the historical expression of the site would be an integral part of any goal or problem. The inference of relative simplicity for prehistoric sites is unfortunate, because it is so incorrect. A prehistoric site with multiple occupation can be of formidable complexity; rock shelters are

Dollar's Reply: (21) Or, for that matter, at sites where documentary evidence is not lacking.

(22) To be unable to specifically date a site by the artifacts does not necessarily restrict the use of these artifacts in obtaining other data, does it?

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notorious for their complexity. In some areas aboriginal structures were also multi-storied and much more substantially built than a historical frame or log house. (23) Any historical archaeologist of whatever parent discipline will, indeed, need to be knowledgeable of basic architectural and carpentering principles. Unless he is a trained architect, like Pinky Harrington, he will still need to seek the counsel of one. Anthropologists don't pretend to omniscience (anyway, not the NICE ones!) and neither should others. (24)

As historic archaeology is carried out in the Park Service, complete historical research ideally precedes the excavation. (Historians in the Service are not in universal agreement with this principal, but no matter!). Clyde is suggesting that a near-omniscient non-anthropologist will complete this research in dove-tail fashion as he finished his archaeological report, with the final synthesis being the basis for a reconstitution of the site and event. Wil Logan has asked, in this regard, whether he sees any validity to historical archaeology as a means of studying human behavior, values,

Dollar's Reply: (23) Which is more complex: an apple or an egg? The answer, of course, depends on the point of view of the observer making the judgment (not to mention the fact that apples and eggs are incompatible for most comparative analyses). Smokey apparently misinterpreted my remarks concerning this thesis, and I therefore will quote for the perusal of all:

In this reconstructive aspect, at least, the historical archaeologist takes on a far weightier and more encompassing responsibility than the anthropologist does in the excavation of a prehistoric site (I am certainly not suggesting that the one is more 'important' than the other). (Quoted from the discussion of Thesis No. 8; the underline is contained in the original text.)

(24) Based on this criterion, quite a good case can be built for there being few NICE anthropologists. . .

etc., or does he see it only as a tool of the State Park (or other) developer? (25)

I agree with Clyde that the traditional goal of historic archaeological projects is, if not to reconstruct physically, to at least do it figuratively (for interpretation to public and scholars) or to make it possible for it to be done later. This does not mean that literal reconstruction is always the best means, nor that other ends may not take precedence.

Clyde's historian-oriented bias shows when he judges the responsibility of the historical archaeologist far weightier and more encompassing than that of the anthropologist. He is evidently penalizing the latter for dealing with savages who are not in the line of European descent. Would he make a similar judgment regarding Tikal, Woodhenge, or Stonehenge? (26)

Ideally, no one would argue that an incomplete excavation of a good site is not a partial depredation. This is true of any kind of site. It is, nowever, a complete depredation to watch it bladed or washed away with no investigation at all. Neither should you run your sponsoring institution into debt because the site is more complex than was estimated. A large part of any archaeologist's responsibility is mature judgment. Heroic, stubborn idealism can do more damage than an irresponsible 'dozer blade!'

Dollar's Reply: (25) I will answer Wil Logan's question with another quote from the "Thoughts" paper:

The purpose of historical archaeology must be to achieve, insofar as possible, the goal of complete understanding of the history of any given site, and the scope of such research must be to include the recovery of all evidence of historical cultural expression at that site, including all architectural evidence. (Quoted from the discussion of Thesis No. 8.) It would seem to me that "evidence of historical cultural expression" would include human behavior, values, etc. etc. . .

(26) The answer to this question should be self-evident.

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When the situation changes more time and money are to be requested, but if the purse is light, you do what you can with what you have. (27)

Clyde is quite right in noting that prehistoric archaeologists frequently (not always) will use a technique which obliterates architectural data on a historic site. This error is usually realized before they get to a second such project, but it is true that it is a pattern among young archaeologists and even old prehistorians taking a crack at a historic site. This is because, to them, it presents a new set of problems.

Thesis Ten is well taken.. I feel that most present investigators are aware of the problem and try to deal with it as fully as the available resources permit. I say again, no breed of archaeologist will ever know enough to be completely independent of counsel!

I agree with Clyde that it is a propitious time for historical archaeologists to ask ourselves "Who are we?" We know that we use the archaeological technique to uncover remnants and acquire data. We then write our reports which contain description and interpretation. The notes, records and artifacts (hopefully) are then processed and filed, stored, or exhibited. Most of this will be done by the investigator, but certain specialists will have been consulted regarding some aspects of the project.

Dollar's Reply: (27) Smokey and I have disagreed on this point (not about the dozer blade, however) for years, and I see no reason to change the situation now. Obviously, the researcher has to be pragmatic about such affairs as budget and time limitations, but certain other considerations should have at least equal priority. Among these latter considerations is that the sponsoring institution should be made aware of the excavation requirements necessary to complete the research (please recall that excavation is a destructive process), and that the sponsoring institution (and I would include in this category certain federal agencies currently involved in archaeological research) must be prepared to share equal responsibility with the researcher for the completeness (or incompleteness) of the site's excavation and related research. Less than this is to be a party to institutional irresponsibility.

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No two investigators will follow identical patterns on what they elect to seek assistance with. This will be determined by his initial training, subsequent experience, and the interests which develop, or fail to develop. Bones, human or animal, elicit no response in some of us despite our training in physical anthropology. Architectural reconstruction may fail to interest others of us if only because of an inadequate understanding of it. Ceramics, even in sherd form titillate some archaeologists quite as much as whole vessels do keepers and curators of museums. Most of us at present will farm out the initial historical research and the subsequent pollen, textile, and certain artifact analyses.

We are not historical architects, polynologists, nor are we museum curators or preservators. Yet we must be something of all of these. Could we be project managers? When we can agree on who we, as historical archaeologists, are perhaps we can then consider what we should become. Then, too, we can turn to the prehistoric archaeologist and ask him, who he is. (28)

Dollar's Reply: (28) . . .and I would like to be around at that moment to see the fight that will inevitably develop.

COMMENTS ON CLYDE DOLLAR'S PAPER

Stephen Williams
Peabody Museum of
Archaeology and Ethnology
Harvard University

After reading Mr. Dollar's paper several times, I find that some of it is quite good, some of it belabors the obvious, and some of it is so archaeologically and anthropologically naive as to set one's teeth on edge. His conceptions of how archaeology is done and its limitations indicate a slight acquaintance with the field, and apparently with archaeologists whose proficiency appears to be questionable, judging from his paper. His notion, for example, that archaeology can only deal with central entities, not regional variance, is just downright wrong. Nevertheless, I feel that the paper makes a contribution in outlining some of the archaeological difficulties one encounters on the historic time horizon, and hopefully it will generate some point-by-point answers by practitioners in the field.

Reply to Williams

Clyde D. Dollar

The time spent by Mr. Williams in reading the "Thoughts" paper is appreciated. His reply, while very general, at least indicates his interest in furthering the discussions. I would like to raise two minor points:

- 1) What parts of the "Thoughts" paper are so "archaeologically and anthropologically naive as to set one's teeth on edge..."? To raise such a question of nescience and then neither support the contention nor further discuss it smacks of a debate technique normally used on the high school - not Harvard - level.
- 2) Where does Mr. Williams get the idea that I have a 'notion' that archaeology can deal only with central entities, not regional variance? I was shocked to find that I am supposed to hold to such a concept. It would be more correct to say that I hold to the 'notion' that historical archaeology must be made to deal with regional variance, and not just central entities.

A REPLY TO

"SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY"

Bernard L. Fontana
Ethnologist

Arizona State Museum

"Critical acumen is exerted in vain to uncover the past; the past cannot be presented; we cannot know what we are not. But one veil hangs over past, present, and future, and it is the province of the historian to find out, not what was, but what is" (Stapleton 1960: 7)

This maxim propounded by Henry David Thoreau might with profit be engraved on the wooden handles of archaeologists' shovels. If the sole aim of our endeavors is to bring back an extinct past, our efforts shall indeed be in vain. The bed in which George Washington slept shall never again contain his bones; the noise of the shot heard 'round the world has long since parted irredeemably for the realm of silence.

Whatever else might be said about restorations is that they are restorations. They are not recreations of the life of some remote time; rather are they the modern and sometimes distorted reflections of the physical trappings with which someone's forebears surrounded themselves. When a lad has been at work in a colonial-style printing shop for eight months learning to set old-style font by hand, and he proudly explains to me he is still an apprentice but that he looks forward to becoming a journeyman, I presume it means he has worked for the corporation for two-thirds of a year and has yet to get his first raise. I further presume that even as a master printer the present-day demand for his hard-earned

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talents shall always be limited. He is very much in danger, in fact, of becoming a company man. Surely it was different with his 18th-century counterpart. Come to think of it, he has no 18th-century counterpart. The 18th-century printer was not a product of restoration.

Or let us consider Brigham Young's house. Whatever purposes it served that mighty Morman it shall nevermore serve. Let each square cut or hand-wrought nail be carefully pounded in place; let the structure's footings stand where they stood more than a century ago; and set the table with dishes identical to those whose pieces were thrown into Brigham's well. This will not bring the religion of Latter Day Saints back toward its beginnings; this will not effect the politics of 19th-century Nauvoo or rekindle the harsh judgments of Nauvoo's neighbors. It becomes instead a modern monument, however faithfully restored, which pleases Elders of the 20th-century church, which attracts tourists, and which stands to remind the modern Morman concerning some of his origins.

Lest anyone think I am being cynical, let it be understood that I am second to no one in my admiration for and love of restorations -- and the more accurately restored the better. I think that such restorations serve a variety of very important present-day functions: economic, social, educational, personal, and, in some cases, religious. Let us not, however, delude ourselves into thinking that because we have copied a building, a ship, or a whole town we have somehow brought the dead back to life. An archaeologist is not Jesus; Old Sturbridge is not Lazarus.

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If anything divides archaeologists into classes it relates but little to their academic training as anthropologists or as historians. Far more basic is the question, "Are we interested primarily in human beings or in things? Are we people oriented or object oriented?" Among the ranks of anthropologists, especially among those who have specialized in archaeology, we find practitioners of both leanings, even as we do among historians. All of us have known academicians with an antiquarian turn of mind. These are the chaps who pester us at cocktail parties, answering questions no one has asked or is likely ever to ask.

I cannot agree that "historical archaeology must be architectural in orientation and reconstructive in both purpose and scope." This view is so narrow that certainly it rankles anyone who has ever spaded a trash mound, cleaned out an ancient privy, salvaged the sawed bones of animal remains from a field where they were thrown, dug a cemetery, gathered tin cans from a cave where a prospector slept, or dived beneath icy waters to regain the cargo spilled by voyageurs at a portage of some Michigan river. What architecture? What kind of reconstruction? Nor are these sites the "very few exceptions!"

Let us consider instead that the orientation of archaeology, historical or otherwise, be humanistic. Let restoration be on this basis; let us only then take up the matter of architecture should it be appropriate. "Nothing," says Thoreau again, "so restores and humanizes antiquity and makes it blithe as the discovery of some natural sympathy between it and the present. Why is it that there is something melancholy in antiquity?"

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We forget that it had any other future than our present. As if it were not as near to the future as ourselves. "...The heavens stood over the heads of our ancestors as near as to us" (Stapleton 1960: 8)

As for the exposition of anthropological, historical, and archaeological concepts, theories, methodology, and methods in "Some Thoughts on Theory and Method in Historical Archaeology," there is nothing to be said concerning it in a short reply that will help. When the statements are not in error, confused, obscure, or ambiguous, they are either unduly contentious or painfully obvious. It is clear, in any case, that the attack is launched from a platform of ignorance rather than of knowledge. It is also launched in an aura of blind devotion to the written word. Is a documentary reference per se better evidence than that afforded by other kinds of data? I have heard it remarked that literary historians seated atop an exploding volcano would not be inclined to believe it was happening until someone committed the event to paper for them. A few of the remarks in the essay under consideration come precariously close to exemplifying that uncomfortable position.

The advancement of the causes of historical archaeology, whatever these may be to different people, will not be promoted by ill-considered debates between historians and anthropologists or by accusations that others are doing mayhem to their sites because of their departmental brand of training. We are joined together in the early growth of an exciting venture. Let us move ahead with the biblical aphorism in mind: "Old things are passed away; behold, all things are become new" (II Corinthians 5:17).

Reference

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Reply to Fontana

Clyde D. Dollar

Bernard L. Fontana's elocutionary morass has the earmarks of having been written to elicit twitters of smug laughter from his undergraduate students. I would sooner quote Captain Kangaroo on Ethnology than Henry David Thoreau on Historical Archaeology (the Walden Pond philosopher must be enjoying a brief revival among the student body at Arizona State). Fontana has managed to contribute some sound and smoke to the general argument, but this only serves to give the battleground the sham appearance of a Hollywood movie set.

Perhaps Mr. Fontana should give a bit of heed to those cocktail party questions "no one has asked or is likely ever to ask"...(?!?). Perhaps he should begin to think in architectural terms about those ancient privies, salvaged bones, tin cans, and spilled voyageur cargos (he might learn more of what 'architecture' actually implies in its relationship to the history of material things.) Perhaps he should have paid more attention to the written word before making a 200 year error (as at Johnny Ward's ranch). And perhaps he should never have written his critique, for we all now have a most interesting basis for evaluating his future work in the field of Historical Archaeology. I, too, have access to the Bible, and would quote a passage from it for Mr. Fontana's meditation:

"When I was a child, I spake as a child, I understood as a child, I thought as a child: but when I became a man, I put away childish things..."

The First Epistle of Paul to the
Corinthians, xiii, 11

CRITIQUE OF DOLLAR'S "SOME THOUGHTS ON THEORY AND
METHOD IN HISTORICAL ARCHAEOLOGY

Edward B. Jelks
Smithsonian Institution

The following comments relate to Dollar's major points as stated near the end of his paper.

1. Dollar states: "It is time to give serious thought to the recognition of historical archaeology as a distinct socio-scientific discipline with a methodology designed to cope with the unique problems encountered during the excavation of historical sites;"

It is way past time: recognition of historical archaeology as a separate discipline with unique problems was made by J.C. Harrington, in print, more than 15 years ago. (1)*

2. Dollar states: "Two of the major research methods used by the historian are the logical processes of deduction and tests for validity, and both of these aspects must be a property of the research at an historical site if such research is to be ... considered historically valid;"

I can find no fault with this statement, and will simply add that deduction and tests for validity are standard research methods used by anthropologically trained archaeologists too. (2)

3. Dollar states: "The techniques of archaeology are field techniques only, and any 'extended' use of these techniques by the researcher is grounded on a statistical basis too far removed from the possibility of verification to be usable in historical research processes;"

* Dollar's reply to these numbered points can be found at the end of this paper.

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The first clause of this statement is erroneous (whether the parenthetical clause which I have left out in the interests of clarity be included or not): Archaeology has numerous techniques that are employed for ordering and interpreting data after they have been collected in the field. The latter part of Dollar's point has no real meaning to this reviewer. Archaeologists do not "extend" their field techniques. Field techniques are designed to produce completely objective observations of physical phenomena. By "extended field techniques" Dollar evidently is referring specifically to typology and seriation, which are neither extended field techniques nor objective observations: they are comparative, statistical methods designed to order the observations made in the field into units representative of the cultures of whatever people or peoples occupied the site or sites being studied. They are used at several levels of abstraction, but never (as with field techniques) at the level of observation. (3)

4. Dollar states: "The concepts of anthropology are oriented toward macroscopic inductive processes and inferential verification, and training in this field frequently does not prepare the anthropologist to cope with the problems faced when researching an historical site;"

The first clause of point 4 is true but not inclusively true. Archaeologists (at least anthropologically trained ones) employ inductive reasoning and validation through hypothesis-testing as standard procedures. They attempt to view problems both macro- and microscopically. As points 2 and 4 ostensibly contrast the methods of historians and archaeological archaeologists, it should be pointed out that historians customarily use both inductive and deductive logic, and also have been known to rely on

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inference in reaching conclusions—perhaps even more so than archaeologists (4)

5. The ten theses:

i. "Since the late 18th century (in America), the number of ... artifact forms ... has increased to a point where the subject is extremely complex;"

I would be the last to disagree with that thesis as stated in Dollar's summation. Previously, however, in his discussion of the thesis, Dollar added that because of the complexity, artifact typology is of dubious value for "specific dating purposes." Again, I heartily agree with Dollar, but submit that occasionally specific dating can be done accurately on a typological basis and that, within limits, some chronological ordering can be achieved with accuracy for any randomly collected sample of artifacts of appropriate size. Since the term "specific" is a relative one in the present usage—and since time is infinitely divisible—any dating could always be made finer if the requisite data were available. (6)

Of course, nobody can date a sample of 19th century nails or ceramics like those referred to by Dollar precisely (say, within one year, or five years) on the basis of their physical attributes alone.⁽⁷⁾ One of the inherent limitations of the typological method is that the precision of its results depends on the precision of the data plugged into a particular typological model. (And this applies to prehistoric archaeology too.) But gross distinctions can often be made. Many persons, for example, (including Dollar, I suspect) can readily distinguish a statistically adequate sample of English-made ceramics dating from the 1790's from another dating from the 1860's. And even if one could not make that distinction, what about

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the typological differences between 17th century and 19th century ceramics and nails. Surely, even Dollar will agree that distinctions that gross can be made. And if so, the method has utility for dating in a general--if not in a "specific"--way. If a method will produce information not obtainable elsewhere, should it be thrown out solely because it cannot produce more specific information--which no other method can produce either? (8)

ii. This thesis states that seriation of artifact types should not be used alone in the construction of historical hypotheses on the grounds that it has not been proven to produce "totally non-distorted historical data". Certainly, seriation should not be used if more accurate techniques are at hand--and I never heard anyone suggest that it should be. No technique should be used if more precise ones are available. But suppose--as is often the case--that seriation is the most precise technique available: What then?

I submit, furthermore, that there is no process of any kind that can be proven to produce totally nondistorted historical data. What are the nondistorted data--produced by eyewitness observations, by deduction, induction, inference, or any other method--relevant to A. Vespucci's role in the exploration of the New World, or to the true causes of the Civil War, or to L.H. Oswald's role in the assassination of President Kennedy? If "totally non-distorted" data are required before historical archaeology can properly be done, we might as well lay down our trowels and forget it. (9)

Typological seriation is a statistical technique, and its results are an expression of probability, not of fact. The same is true of radiocarbon

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dating, of Gallup polls, and of the pastime of roulette. Anyone who possesses the barest knowledge of statistical principles is aware that a single radiocarbon date expresses only probability, not fact; that polls taken by Mr. Gallup do not necessarily reflect the "non-distorted" temper of the American constituency (ask Tom Dewey); that every spin of the roulette wheels at Las Vegas brings more profit to Howard Hughes' bulging pockets. But the validity of the statistical method, within the context of its limitations, has been, in my opinion, firmly established.

Properly trained archaeologists are aware that the results produced by typology and seriation are expressions of probability, and they would never attempt to make them produce "totally non-distorted" data in the first place. A knowledgeable archaeologist with anthropological training who found himself dealing with data like that from Fort Smith or Nauvoo would simply plug the newly acquired data on buttons and ceramics into the previously existing typologies—a procedure that would enhance the precision of the typologies next time they were used. (10)

iii. All artifacts recovered archaeologically from any site, historic or prehistoric, have an infinite number of inherent dates, at least four of which are of concern to the archaeologist: (1) date of manufacture, (2) date of use, (3) date of discard, and (4) date of deposition at the spot where found archaeologically. The last two are not necessarily the same. These matters involve the very essentials of archaeological theory. And outside of direct historical documentation (which, naturally, is preferable when it exists but, unfortunately, is all too seldom available for this kind of problem), I do not perceive any means by which those dates

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can be established other than through the conceptual methodology of anthropological archaeology: that is, through the principles of (a) contextual association and stratigraphy that underlie field excavation techniques, and (b) the concept of historical typology that is the basis of comparative analysis. (11)

iv. The "alpha" and "omega" manufacturing dates apply, of course, to prehistoric as well as to historic artifacts, and they are—as Dollar says—difficult to establish. But it is better to establish them on a statistical basis within relatively broad parameters than to not establish them at all.⁽¹²⁾ "It is a matter of ... frustration" not only to Dollar but to all archaeologists that "more historical [and prehistoric, too] artifacts ... do not as yet have an omega date!" But is not a matter of wonder to those who are knowledgeable about the nature of archaeological data. (13)

v. Dollar uses the term "provenience" in a singular way to refer to what most archaeologists would call "period of occupancy," or "date of component," or some such terminology. This thesis is concerned with the inevitable problem of separating one occupation period of a site from another, or "ordering the field data into culturally significant chronological units," to use the jargon of the trade. I fully endorse Thesis #5, with the addendum that it is one of the ubiquitous problems of archaeology—prehistoric as well as historical.

vi. This thesis states that every artifact has two locations within the site matrix.⁽¹⁴⁾ This is not worth belaboring, but an artifact really has only one location, not two, inasmuch as it patently can be in only one place at a time.⁽¹⁵⁾ Sometimes archaeologists measure the position of an artifact

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with respect to the surface of the ground and to some kind of horizontal reference system in order to record where it was found. (To fix a spot on a horizontal plane, incidentally, requires two readings of some kind, so that three measurements are necessary to record the exact location of an artifact.) But there are other ways than simply plotting on a three-dimensional scale for recording relationships between artifacts and the physical components of a site. The statement that every artifact in a site has two "locative characteristics," in all events, is sheer nonsense.

The utility of horizontal distribution analysis is well known to anthropologically trained archaeologists.

A somewhat irrelevant question: If the physical attributes of artifacts after the late 18th century are too complex for chronological ordering on the basis of stylistic criteria (as maintained in Thesis # 1), then what are the grounds for the statement that vertical distribution of artifacts at Fort Smith reflected incorrect relative dates?) (16)

vii. The following comment is offered with regard to Thesis # 7.

It is rarely possible (but not always impossible) to date a specific artifact confidently by the physical context in which it was found. If the physical context is discrete and shows no signs of having been disturbed after its formation, then there is every reason to conclude that the deposition of the artifacts within the context dates from the time that the context was created. If the date of the context can be established, precisely or approximately, then the same depositional date can be assumed for the artifacts. (17)

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viii. The recovery of architectural data is an important aspect of archaeological excavation anywhere--at both historic and prehistoric sites. Dollar's statement that "Historical archaeology must be architectural in orientation and reconstructive in both purpose and scope" [*italics mine*] is perhaps too strong. Taken literally, this says that the only legitimate purpose for excavating historic sites is the study and reconstruction of their buildings. In that case, all the previous discussion about typology (except as it applies to the typology of buildings) is irrelevant as non-architectural artifacts such as ceramics are not legitimate subjects for a historical archaeologist's attention anyhow. Furthermore, nonarchitectural features at a site--trash heaps, for example--should not be excavated if this policy be adopted. (18)

Architecture and reconstruction are practical considerations of prehistoric as well as of historical archaeology.

ix. The statement that "a considerable and basic dissimilarity exists between archaeological evidence for structures at an historical site and such evidence for structures at a prehistorical site" is meaningless if prehistoric architecture of the Meso-American and Southwestern United States are excluded. Or does Dollar truly mean to say that problems of architecture at prehistoric sites, are different from problems of architecture at prehistoric sites, except for those prehistoric sites where the problems are basically the same as for historic sites?⁽¹⁹⁾ Dollar must have never excavated a Spanish colonial jacal, either, if he thinks they were more massive than prehistoric Indian earth lodges. (20)

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Architecture provides a good example of typological variation within the historic period. Eighteenth century English colonial architecture, for example, is different in many respects from 18th century Spanish colonial architecture in the New World--not to mention 18th century Indian architecture, which is also generally considered to be within the purview of historical archaeology. And these differences reflect the respective cultural traditions of English, Spanish, and Indian peoples of the period. Similarly, differences between the architecture of different prehistoric cultures obtained too. And it is just as important that an archaeologist be familiar with the architecture of a prehistoric culture whose remains he is excavating as it is for him to be familiar with the architecture of a historic site he is excavating. The desirable architectural knowledge in a particular instance is a function of cultural affinity, not of whether or not a site happens to be historic or prehistoric. (21)

x. Thesis # 10 appears to be at odds with Thesis # 8: at least this reviewer does not see how identification and authentication (My dictionary does not list the word authentication.) are "architectural in orientation and reconstructive in both purpose and scope"⁽²²⁾. Problems of authentication and identification, in any case, as well as separation of "come-down" periods are important ones in archaeology. The latter two, by the way, apply to prehistoric as well as to historic sites and traditionally have been major concerns of anthropologically oriented archaeologists. They are, in fact, two of the important problems that typology and seriation were devised to help solve. (23)

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Dollar has shown a lot of perception in singling out some of the major problems that relate to the ordering of archaeological field data into temporally and culturally significant units. But he has treated the problems as though they had not been recognized previously by anyone. Actually, these same problems are normally taken up at the advanced undergraduate level at any university offering a full curriculum in anthropological archaeology. Furthermore, they are normally in the forefront of every archaeologist's mind as he digs any site—historic or prehistoric.

Dollar maintains that there is a major difference between "concepts of methodology used in excavation of a prehistoric site and the concepts necessary for use on an historical site." This could only be true if there were some essential difference in the nature of historic and prehistoric sites.⁽²⁴⁾ After having personally excavated dozens of sites in both categories, I cannot agree that there are any fundamental differences.

The basic procedure for properly excavating any archaeological site is (1) to identify the physical components that constitute the site (this is done by various techniques, depending on the requirements of each individual site) and, (2) to dissect each component as a separate unit (again, by whatever technique is best suited to the situation). The spatial relationship of each component to every other component is observed and recorded, and the cultural content of each component is observed and collected as a unit. This is done with complete objectivity, but with varying degrees of accuracy depending on the skill and experience of the excavator.

The nature of the individual components may vary greatly from site to site; but there is often more difference in structure and content between

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two historic sites, or between two prehistoric sites, than there is between many historic and prehistoric sites. The standard definition of a historic site is one for which there is direct or indirect historical documentation. The question of whether or not there exists historical documentation relative to a particular site has nothing whatever to do with the site's structure or cultural content.

An adequately trained archaeologist is expert in recognizing anomalies resulting from human occupation in the natural matrices of a site. He should, under normal circumstances, recognize a filled foundation ditch, or a post mold, or any other culturally produced anomaly that is visible; if he does not, it is because of a lack of skill on his part. He may not know the cultural significance of an anomaly, but he should see it and record it if it is clearly visible.

Some anomalies are quite conspicuous, others are exceedingly subtle; and no archaeologist, however experienced, is going to see them all. Naturally, the more familiar he is with the culture represented at the site he is digging, the fewer he is going to miss. But this matter has nothing to do with any disparity between historic or prehistoric cultures per se: it is simply a function of how much the archaeologist knows about the particular culture being investigated, regardless of its age or degree of historical documentation.

* * * * *

In summary:

1. With respect to methodology of excavation, historic sites do not differ basically from prehistoric sites. The differences in structure and

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content between sites are functions of cultural variations and natural processes, irrespective of whether a particular site is classified as historic or prehistoric.

2. The most precise applicable techniques should always be used for dating as well as for other kinds of data ordering. But sometimes, owing to the nature of a set of data, the more precise techniques cannot be used. In such cases, there is every justification for employing typology, seriation, or other statistical methods: One always should do the best he can with the data and tools at his disposal. Typology, incidentally, serves purposes other than chronological ordering, a point that was never brought up by Dollar.

3. I can see no valid reason for restricting historical archaeology to the study and reconstruction of buildings. And--unless it should be demonstrated that they are pointless--such problems as acculturation, ecological adaptation, diffusion of cultural elements, diet, technology, pathology, historiography, and other nonarchitectural aspects of historical communities will continue to be pursued by historical archaeologists.

4. Dollar's contention that "the techniques of archaeology are field techniques only" is naive. And the implication that one can learn to be a competent field archaeologist without intensive grounding in archaeological method and theory (including typological theory), in the pertinent areas of geology, and in cultural theory--as well as in history and historical architecture where historical archaeology is involved--is not only misleading but dangerous. (25)

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5. Historical archaeology has indeed become a separate field of study, and there can be no question that competence in the field requires specialized training. It seems as obvious as all get-out to this observer that a fully competent historical archaeologist must be well grounded in the applicable areas of historical method and theory, of archaeological method and theory, of culturological method and theory, of architectural method and theory, of geological method and theory, of statistical method and theory, and of other fields. He also needs to be as knowledgeable as possible about all the different aspects of the material culture of the people who occupied the site he is investigating—not to mention their social organization, their political organization, their religion, and the other nonmaterial aspects of their culture. And he needs practical training in scientific excavating and in laboratory analysis of data.

I am sure that there is no one person on earth today who has all the qualifications to be a fully competent historical archaeologist. But historical archaeology is a new field. The need for developing training programs to produce qualified historical archaeologists is glaring. When such training programs are organized, it would be a major mistake, in my estimation, if the curricula do not include methods and theories from all the fields listed above, and others. (26)

I am personally distressed by a recent trend toward factionalization among those working in the field of historical archaeology. Two camps seem to be forming: one maintaining that historical archaeology is essentially history and that anthropological methods have no legitimate place in the field, the other maintaining that historical archaeology is

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really anthropology, supported by historical documentation. It is neither, for Pete's sake: it is a field of such inclusive demands that it needs to draw to the fullest extent from the methods and theories of both history and anthropology, as well as of other disciplines. Let's stop bickering, adopt what is useful from all possible sources, and proceed with the business of producing competent technicians—not in history, not in anthropology, but in historical archaeology. (27)

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Reply to Jelks

Clyde D. Dollar

(1) The "Thoughts" paper was, of course, not written in a vacuum of awareness to the printed statements of other professionals regarding this 'recognition.' Nor was it written to imply a 'first proclamation' in this area. My hat is off to J. C. Harrington for his early pioneering work and to Dr. Jelks for mentioning the subject. However, it was (and still is) my opinion that very little professional attention has been paid to this 'voice in the wilderness' of some years back, and probably both Jelks and Harrington will be shocked (as I was) to discover the intensity of the evidence of this situation as embodied in many of the critiques on the "Thoughts" paper.

(2) Acknowledged.

(3) Dr. Jelks appears to be using his own definitions to replace those given in my paper; hence the confusion. If Dr. Jelks were to use my definitions within the framework of the paper, he might discover that there would be few areas of disagreement between the two of us on this point.

(4) Excellent point, and one that escaped the attention of many of the critique writers. The referenced section from the "Thoughts" paper is a summation drawn from the first part of that paper—which was described (within the Introduction) as ". . .being a very brief discussion of general concepts and limitations in the fields of history, archaeology, and anthropology." Too frequently, to 'briefly discuss general concepts' is a dangerous tack as it often is interpreted as a shallow and overly rigid presentation. My statement, as quoted by Jelks, is nonetheless generally true and was therefore used as a background to highlight other statements.

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(5) If the results of these processes can be subjected to verification, then by all means use them. If they cannot be subjected to subjected to any type of verification, then, while these results might be of considerable interest to the anthropologist—and perhaps even the historian—they must not be used as historical data within the framework of historical research processes.

(6) Correct. I deliberately refrained from defining the word 'specific' because (a) of its extreme relativeness to any given situation or site, and (b) I was curious to see how many would question this omission of terminology. The research historian deals in dates and facts, and from these he builds his hypotheses. This, in my opinion, must be the pattern for research on any historical site, and regardless of how interesting the results obtained otherwise might be, they are not history in a critical sense. The fact that so few critique writers failed to note this point might suggest a preoccupation with data other than 'specific.'

(7) See Thesis No. 7.

(8) What is this 'general' data, arrived at through what I have termed 'extended' (statistical) techniques, were at variance with known historical (factual) data? The answer would be to accept the historical data, of course. But, if there were no such historical data to be used as corroborative evidence, what then?

(9) Oh no! The loss of Dr. Jelks' trowel to the archaeological profession would be a major loss indeed! More than 260 words (two paragraphs) were expended in the "Thoughts" paper to show that the historian works in a subjective atmosphere while constantly attempting to achieve objective findings. I trust that Dr. Jelks was not implying that,

after saying this, I was then suggesting that all historical data must be "totally non-distorted" in order to be usable by the research historian. The term 'distorted' data, when used by a research historian employing critical research methods, implies data from any level of source material (primary, secondary, or tertiary) that is at variance with or irrelevant (under certain circumstances) to verifiable facts. Frequently, of course, the very condition of data being 'distorted' is of significance to the researcher and does not preclude his use of it within the context of his level of generalization (and/or acceptance), providing, of course, that it is presented as 'distorted' data. The very term itself ('distorted') implies some means of knowing of its veracity (comparative and/or critical analysis of specific facts). Data derived from statistical methods (note my definition of 'extended' archaeological techniques, more specifically seriations) cannot help but be based on a generalized statistical situation, and is therefore not verifiable (hence, not usable) on a specific level of critical historical research.

For example, consider the so-called 'sherd count' (if not sherds, then nails, buttons, etc.), meaning the listing and/or grouping of such artifacts found on any given site, group of sites, etc. Such a count tells us only of the number of pieces of certain items (I will use ceramics as an example) found within the defined limits of the study. It does not tell us: (1) the number of whole pieces used on that site, (2) when any given type of ceramic was in use, or indeed (3) if the finding of any given piece of ceramic (or sherd) has any relevance to the historic period being researched. I would say that this is 'distorted' data in that it has no verifiable relation to a specific level of critical research.

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Since my acceptance level for statistical information is quite low, I feel that if such data can be made verifiable (because of the possible existence of factors unique to a given site), then this verification must be of such a nature as to be totally verifiable, or, in other words, totally non-distorted. On the other hand, a sherd count does yield information that is relevant to the temporal 'provenience' period (or, entire history) of a given site, and it clearly is of value to that level of study (or, if you will, generalization). The very existence of the ceramic sherds comprising the count is its own verification (they were found on the site and therefore must have arrived there at some time during the site's history). On this level at least, the sherd count produces totally non-distorted information; on any other more critical level, precluding the existence of unique and happy circumstances, it does not. As for my own approach, I will continue to use sherd counts and other statistical methods, when applicable, but only on a level where this information can be verified as being 'totally non-distorted' data.

(10) I get the impression from reading these two paragraphs that Dr. Jelks is not using 'typological seriation' in the same sense that I defined these two words (not once, but twice—same definition, however) in the "Thoughts" paper. I knew that there would be semantical difficulties involved in the "Thoughts" paper, and I therefore went to some lengths in order to present definitions of words in the context in which I used them. Whether the reader agrees with these definitions or not is no basis for these definitions to be ignored and the reader's own definitions substituted in their place. For example, I draw a clear distinction between "typology" and 'seriation,' (both defined within the "Thoughts" paper); Dr. Jelks (and other critique writers) on the other hand, blandly ignore this

distinction (perhaps on the basis that it does not agree with their own definitions), and proceed to blast away. Gentlemen, whatever else this tack might be called, it is not critical reading! I trust that my discussion following Number 9 (immediately above) has cleared away some of the distortion surrounding 'non-distorted' data.

(11) I concur, and would like to point out that Thesis No. 3 does not limit the inherent dates of an artifact to only the stated two, i.e., the date of manufacture and the date of deposition. In the last sentence of this paragraph, Dr. Jelks seems to be limiting the principles of contextual association, stratigraphy, and the concept of historical typology to use only by 'anthropological' archaeologists. If he is, in fact, suggesting this (which I rather doubt), then he is probably in for some rather pointed discussion from geologists, paleontologists, historians using archaeology as a research tool, and (hopefully) even a few anthropologists.

(12) What statistical technique could be used to establish the alpha and/or omega manufacturing dates for use in dating artifacts found at specific sites?

(13) Then this might be because ". . .those who are knowledgeable about the nature of archaeological data. . ." are not knowledgeable enough about the nature of historical data.

(14) Incorrect. The thesis states that every artifact has two locative characteristics in relation to the matrix of a site, not two locations.

(15) Dr. Jelks is right: this point is certainly not worth belaboring. Dr. Jelks sits in a chair (while reading this) in relation to the room he

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occupies; at the same time, he is in a building in relation to the Southern Methodist University campus; he is located in Dallas, Texas, in relation to the continental United States, and he exists on Earth in relation to the Universe. Yet, while he physically occupies only one given area at any time, he nevertheless has at least these four locative characteristics in relation to the matrix of his existence. An artifact (as well as Dr. Jelks) has a number of locative characteristics in direct proportion to the number of points of reference for that artifact (or Dr. Jelks). If this is "sheer nonsense" (as Dr. Jelks calls it), then what is Dr. Jelks?

(16) This implicative statement is an exclusive disjunction as the antecedent is both fallacious and overstated. Thesis No. 1 does not state that artifacts are too complex for chronological ordering; it does state that the problem is quite complex. Furthermore, the real point of Thesis No. 1 is the statement that

This phenomenon has very important implications in the matter of using certain artifacts for specific dating purposes at an historical site. (I have added the underscore.)

The consequent phrase of Dr. Jelks' implicative statement can be clarified if he will allow me the privilege of textual condensation. At the First Fort Smith site, the Prehistoric Period, Phase I (of the site's total temporal provenience) was from ca. 500 B.C. to 1000 A.D.; the Historic Period was from December 25, 1817, until mid-July, 1834; and the Coke Hill Period was from ca. 1890 to September, 1958. For the sake of illustration, allow me to designate certain characteristic artifacts as being somewhat representative (and temporally identifiable) to each of these three periods: (1) Projectile points for the Prehistoric, (2) early nineteenth century military uniform buttons for the Historic, and

(3) wine bottle corks/caps for the Coke Hill Period. More than 12,000 square feet, or approximately 90 percent of the living area within and without the walls of the fort were archaeologically investigated. For most of the fort's area, the average thickness of the artifact 'bearing' area (from sterile soil to top soil grass line) was between 1" and 1½". With distressing frequency, wine caps or corks would underly early nineteenth century buttons, which would in turn be overlain by prehistoric projectile points or vice versa, etc. While the above sentences represent a considerable condensation of the precise situation, perhaps the idea will come through that even Dr. Jelks would have a difficult time escaping the conclusion that the vertical distribution of artifacts at the First Fort Smith site relected incorrect relative dates. As an aside, it should be noted that this very disturbance was a discernible and important clue in unravelling certain historical actions occuring on the site. For further data, I would refer the reader to Jackson W. Moore's The Archaeology of Fort Smith I, National Park Service contract report, 1968.

(17) There are quite a number of "ifs" in this paragraph (three to be exact). I agree with Dr. Jelks when he states that it would be "rarely possible. . .to date a specific artifact confidently by the physical context in which it was found." In fact, I think that it is such a rare occurrence that it becomes the exception to the general rule; hence, Thesis No. 7. The core of Dr. Jelks' argument (which, by the way, is quite valid) is the scope of his acceptance of data as expressed by the phrase:

. . .then there is every reason to conclude that the deposition of the artifacts within the context dates from the time that the context was created. (I have added the underscore.)

As regards my own personal approach, I regard archaeological data, particularly locative data, as secondary source material (not primary), and hence accept it only as prima facie evidence to be critically used as such.

(18) Overstated, no doubt based on Dr. Jelks' admitted 'too strong' impression of the thesis. He, and a number of other critique writers, have interpolated the word 'exclusively' in place of my phrase "in orientation."

(19) An interesting syllogism, but unfortunately, syllogistically as well as factually incorrect.

(20) Misinterpreted; the thesis does not state this.

(21) Agreed, but this does not negate the thesis. Whether or not a site is historic or prehistoric is in fact a function of cultural affinity, is it not?

(22) I do not see that Thesis No. 10 is "at odds" with Thesis No. 8. The definition of architectural features and the reconstruction of historical events at any given site are at least two of the major tools available for authentication (or, if this word bothers Dr. Jelks, authentication).

(23) Typology, perhaps, but not seriation (at least, as I have defined it in the "Thoughts" paper).

(24) There is an essential difference. This difference lies in the nature and type of the information derived from these sites.

(25) I wonder if Dr. Jelks realizes just how 'exclusive' and pedantic this paragraph sounds. What he seems to be saying is that only those who are expertly trained in anthropological techniques should research history archaeologically.

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(26) I heartily agree!

(27) And, again, I heartily agree! And at the same time, I would like to express my grateful thanks to Dr. Jelks for the writing of his Critique. He has brought to bear an impressive amount of knowledge and experience on the myriad of problems facing historical archaeologists now and in the future, and, in my opinion, he has suggested problem areas which will, in the future, help coalesce our ideas into something more firm than just thoughts on theory and method.

COMMENT ON CLYDE DOLLAR'S PAPER

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I think it is unfortunate that a work of this caliber will attract the attention of the Conference membership that it will. I'm quite sure that there will be reams of rebuttals and counter positions. The unfortunate part of it is that there is nothing wrong with interest that is stirred by controversy, but in this case I feel that the interest has been stirred simply through indignation, indignation wrought through the presentation of a rather amazing mass of mis-information. Dollar's sophomoric work simply does not acknowledge the work of others in the field, he misapplies anthropological and historical theory, and in spite of the rebuttals that may be printed along with it, can be quoted without mention of the rebuttals, a not too happy situation for the Conference membership. In discussing this paper with a colleague, a formally trained historian now historical archaeologist, I have found that he finds it as insulting to the discipline of history as I did to archaeology. I do not feel that this paper is worthy of criticism or publication, revealing as it does that a little knowledge is a dangerous thing.

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Reply to Demmy

Clyde D. Dollar

Mr. Demmy, who is neither an historian nor anthropologist, has, on a number of occasions, adequately displayed his lack of theory comprehension in these two fields. It is discouraging to note that, in replying to my paper, he not only continues with his flamboyant display of pseudo-knowledge but also adds sarcasm to his character trait list as well. No doubt his peers will take note of the shallowness of his remarks and will recall these when critically evaluating any future archaeological work Mr. Demmy might be allowed to do.

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COMMENTS ON CLYDE DOLLAR'S

"SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY"

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Historic archaeology may fairly be said to have come of age in the New World within the last year or so, and it was for this reason that this reviewer felt compelled to attempt to state in the first issue of the publication of the Society for Historical Archaeology (Walker, 1968) what he felt should be the philosophy of this new field. It is therefore extremely interesting to be able to consider a paper such as Dollar's which has been put forward with much the same aim in view. (As I was unable to attend the conference at which Dollar's paper was presented I had not seen its contents until after my own paper had appeared. (1)

Inevitably, a great many of Dollar's points can be related either directly or indirectly to my own paper, so that I repeat to some extent the views stated there, but I should like to consider Dollar's points in relation to my own experience in the field.

Historic archaeology is the excavation and interpretation of post-Columbian sites of European or European-derived occupation and the study of associated material. What Dollar means by "a distinct socio-scientific discipline (even duo-disciplinary in nature)" I have not the faintest idea (2)

Dollar's Reply: (1) Nor did I see the contents of Walker's paper until after mine had already been presented.

(2) A). "distinct": see my reply to Cleland and Fitting.
B). "socio-scientific": is History, Anthropology, and/or Archaeology an art or a science?
C). "duo-disciplinary in nature": a combination of the research methods of both History and Archaeology.

For heaven's sake let us use English and not Taylorese. Archaeology, whether historic or not, is the excavation and interpretation of sites and their material. I agree that excavation may be a technique, or rather a combination of techniques, in that it can be applied to a number of different fields, but I cannot emphasize too strongly that excavation without interpretation of the evidence, stratigraphic and artefact, constitutes the rape of a site and the irretrievable loss of irreplaceable source material. To define archaeology as a technique only makes it very easy to define an archaeologist as a technician only: an archaeologist is one who applies his excavation data to whichever field his training has fitted him - British Medieval or Post-Medieval, New World historic, Egyptology, Assyriology, Mayan, European prehistory, or anything else. Unless he knows or is prepared to learn the history of the period to which the site he is excavating belongs, he should never as much as set foot on the site.(3)

Dollar's Reply: (3) My statement about archaeology being a 'field

technique only' obviously needs clarification in order to correct Walker's misinterpretation. First, I am speaking only of the 'data gathering' stages of the research. Second, (and this point I thought would be so obvious as to not need specific mention), I am referring to methods of data control, whether it actually be at the bottom of an excavation pit, trench, square, etc., or in a laboratory situation (whether this be located ten feet from the excavation, at a university, or on the planet Mars). The main point of my statement is I feel that there needs to be a distinction between the processes of data control and the manner in which this data is interpreted.

Walker apparently took my statement to mean I was advocating that data only be controlled and not interpreted (a situation extremely difficult for me to comprehend as my own personal approach to field excavation requires me to make some interpretations of the archaeological data even as it is being uncovered). As far as Walker's implications that I have advocated "excavation without interpretation of the evidence stratigraphic and artifact" I would like to point out that all ten of the theses refer to the various aspects (and limitations) relative to which I feel data must be interpreted. Obviously, if I felt that the archaeologist must not make an interpretation of his data, a discussion of the manner in which this should be done would have been unnecessary.

For these reasons, then, I cannot accept Dollar's definition of archaeology. (4) Nor can I accept his apparent limitation of extended archaeological techniques to statistical and seriation studies. Interpretation goes far beyond such obvious techniques. (5)

Dollar's definition of an historian as one who uses the logical processes of deductive reasoning and the research processes of verification seems peculiar, not because an historian does not use such methods, but because Dollar appears to say that an historian uses these methods in contradistinction to other people involved in research, including archaeologists and anthropologists. Indeed, his definition of an anthropologist as one who uses "macroscopic" processes as opposed to the historian's "microscopic" approach, and is "inferential" while the historian is "legalistic" could be construed to mean, in broad terms, that historians write accurately while anthropologists waffle. (6) While it certainly appears to me that many anthropologists make full use of the fact that their field contains far fewer accepted certainties than does, say, history, in order to indulge in flights of pseudo-scientific nonsense, it is rather startling to read that archaeologists and anthropologists apparently do not use logical processes of deductive reasoning and verification of facts: surely all researchers must include these techniques. (7)

Dollar's Reply: (4) I hope that the above paragraph will clarify this point.

(5) There is something very confused here: this is not what I was talking about at all.

(6) Walker said it -- I didn't!

(7) I would slightly revise this last statement of Walker's to read: ...surely all researchers should include these techniques.

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However, it does seem true to say, as Dollar does, that anthropological studies tend to formalize generalizations which are difficult to prove. All research, of course, does involve such factors, but in anthropology it seems related to sociological studies which often use mass-statistical surveys to make conclusions which may be valid as generalizations; but being generalizations, are often inapplicable to single instances. It is not coincidence that anthropology and sociology are almost always united in the same university department. It is when these generalizations cease to be fluid and to be useful inspirations on which to base original work, and become instead neat chest-of-drawers categorizations into which everything must be placed (answer either "yes" or "no") that the subject becomes intellectually sterile and reaches a dead end. It is this position which I feel is prevalent in much anthropological thought, at least that part of it which has imposed itself on New World archaeology. Anthropology certainly seems to me to need the stiffening discipline of historical philosophy; if the latter can be applied successfully to Old World prehistory it can certainly be applied to New World prehistory.

Considering for a moment the technique of archaeology, it seems to me that in very many cases in the New World these techniques are about two generations behind those in Great Britain. Dollar in his ninth thesis notes that certain archaeological field techniques used by American prehistorians, such as trenching along a wall line, will destroy vital information on an historic site. In Great Britain, calling an excavator a wall-follower is what calling an excavator a pot-holer is in North America. I have seen carnage reminiscent of the London blitz called scientific excavation, and

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seen students on excavations working with about as much discipline as a herd of water-buffaloes. A tidy, organized excavation is not necessarily a good one; but to be good an excavation must start by being tidy and organized. Only on such excavations can complex stratigraphy be recorded, and people who excavate without regard to stratigraphy are simply not archaeologists. Further, to those who argue that because a site is shallow and thus has no meaningful stratigraphy there is Dollar's salutary comments on the significance of horizontal or 'locative' position of material.

When I quoted with slight sarcasm Willey and Phillips' apparently facile statement that in the past the "assumption of a more or less unvarying rate of cultural change in a spatial-temporal continuum has been overdone" I did not realize that the authors were proposing a major departure from accepted principles. It was not until I read recently in a new study of Iroquois prehistory the statement that in the light of recent C14 dates obtained from various cultures we must now see these are overlapping rather than lying in simple linear progression that I realized that such ideas still seriously exist in North America.

As long as ideas such as unvarying rates of cultural change and simple linear sequence of culture continue in print, and as long as the techniques of opencast coalmining continue to be tolerated as part of the methodology of American archaeology, and as long as text books blithely state that excavation in six-inch levels is better than excavation by stratigraphic layers, North American archaeologists are going to be regarded as incompetents by their British counterparts. This reflects on me as much as on any other North American archaeologist; and while North American prehistorians need not be troubled by what British prehistorians think of them, it is not possible

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to be isolationist in historic archaeology, for this field is going to involve close connections with work on sites of similar age in Great Britain. If one tenth of the effort expended on writing the theoretical philosophical works, typified by those of Binford and Taylor mentioned in my previous article, were to be expended on outlining the basics of sound excavation practice and interpretation of recovered information, North American archaeology would be in an infinitely healthier position.

In view of Dollar's suggestions that anthropological training is not suited for the stricter discipline of historic archaeology, I can say that I know of one recent successful Ph.D. thesis in anthropology dealing with historic archaeology, done for a prestigious university, which is the absolute proof of dollar's contentions. If its contents really reflect what the average anthropologist thinks is good enough for historic archaeology then there is a crying need for historic archaeology to break away from anthropology immediately. If "anthropology is to become history or to become nothing" then it has certainly become nothing. (8)

The ten theses put forward by Dollar constitute some good points with which to start listing some of the basics of archaeology. However, most of his points can be applied either directly or indirectly to prehistoric archaeology as well as to historic. Theses 3 to 5 deal with what superficially are the most obvious differences between the interpretation of material from historic sites and that from prehistoric sites - the preciseness of thought necessary when one deals with sites and material associated with hard and fast documentation (whether accurate or not) as to use, date, and source. And yet surely all items, whether prehistoric or historic, have alpha and

Dollar's Reply: (8). It took great courage on Walker's part to write these past five paragraphs. I only hope that his words will be given the attention they deserve.

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omega dates, and all sites have historic and alter dates. In prehistory it is incredibly easy to ignore these considerations because the differences involved - relative to a site which may be datable only to a period of several centuries - are too small to be significant, or, indeed to be measured. Here a British training is an advantage, for in prehistoric Britain from c. 2,000 B.C. onwards most sites can be dated with a precision unknown in New World prehistory outside Maya sites, not by C14 or similar means, but by ultimately historic evidence. Thus it is frequently possible to date to within a century in the first two millenia B.C. and often to a half-century. As a result, problems of heirlooms and hoards affecting chronology cannot be ignored. Further, recent investigations into the process of weathering and silting of earthworks have produced important evidence for the chronological interpretation of stratigraphy, revealing problems even in simple-looking deposits.

The terms alpha and omega for artefact dates are certainly simpler and easier than the terms used in Britain for the same idea - terminus post quem and terminus ante quem - and I feel deserve to be tried by writers with a view to their becoming standard usage. The provenience date concept (or as I prefer to call it, provenance) is extremely useful, but the terms historic and alter for site dates are less satisfactory, partly because all dates are "historic" and partly because alter may be misread and mispronounced as the verb to change. Perhaps the terms 'subject' and 'non-subject' period could be used for 'historic' and 'alter' respectively - one could then use convenient sub-divisions of the latter such as 'pre-subject' and 'post-

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subject'. (9)

There is no doubt that historic archaeology forces those involved in it to think of dating problems with an intensity hitherto unknown in the field of archaeology. This is complicated by thesis one: the tremendous amount and variety of artefacts, particularly in the later historic period.(10) This thesis reflects perhaps the greatest problem with which the historic archaeologist is faced: the attempt to master enough information on material generally very inadequately known to enable him to adequately study his site. I find my own specialist field a subject which requires my full-time attention to keep abreast of work in the field. It is extremely unlikely that historic archaeologists can become polymaths - indeed the days of pan-European prehistorians such as Gordon Childe are probably over and even prehistorians are having to specialize in either areas or periods simply because of the overwhelming and continuous increase of knowledge. This,

Dollar's Reply: (9) In my opinion, the word "alter" is awkward, and I felt so even as the "Thoughts" paper was being written. Walker's suggestion (that the words "subject" and "non-subject" be substituted for "historic" and "alter" respectively) is apparently already in use, and if this is the case, lends weight to their being put to use in our professional geographic area. However, these words ("subject" and "non-subject") have a semantical connotation that implies one is worthy of the research because it is the 'subject' of such study while the other is not. We need to do some more thinking on this matter before arriving at what perforce must be a generally acceptable set of terms.

(10) I will take the liberty to point out that the New World use of the word 'complicated' has a somewhat different connotation than what I assume to be the meaning which Walker signed to his use of it. According to The Oxford Universal Dictionary on Historical Principles, Third Edition, revised with addenda, Oxford University Press, at the Clarendon Press, 1955, the word 'complicate' means:

"1. To fold, wrap, or twist together - 1691. 2. To combine intimately - 1691. 3. To mix up with in an involved way 1673. 4. To compound - 1707 [and the example of this fourth meaning is:] 4. Ideas...complicated of various simple ideas (Locke)." page 356.

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however, must not mean that historic archaeologists develop into nothing but a number of "experts" in various fields: the primary responsibility for the interpretation of a site must always lie with the excavator, however much he may have to rely on others for specific points of information.

Thesis 2 presents a peculiar but not unexpected problem to those who are entering a new and largely unstudied field. I do not feel particularly worried that in two instances Dollar found that typological evidence indicated a date contradicted by the historical evidence. Typological evidence - and I use the word in its British sense of meaning both typology and seriation in American usage - is, as I stated in my previous paper, the starting-point of a study, not the end result. Few if any typologies will ever produce "totally distortion free information". Initially, a model or concept has to be postulated. The historic archaeologist may have to postulate a typology on the evidence of material from two or three excavated sites and a preliminary historical search for dates. He may well have no idea of how the sites were excavated and thus have to take the excavation reports at their face value; the documentary evidence may be a late 19th century book on ceramics full of unfootnoted assertions. Basically, all he may be able to say is that on the evidence of his preliminary work the particular artefacts appear to have been current at the time indicated by the dates of his site. From this preliminary study, to quote my previous article, theories can be set out, all, some, or none of which may be true. The evidence from each subsequent piece of work will amplify or modify the initial theory.

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Many variables of which the archaeologist may be aware simply have to be set aside initially because their effect cannot be judged. If the deposits which contained the buttons on which an initial button typology was based were in fact redepositions of earlier material then it scarcely matters that the material was found sealed between two floor levels in each of which was an unopened casket containing a sworn affidavit giving the date the floors were built. Only if it is possible to find evidence that the layer is redeposited material is it likely to be recognized for what it is, and the best of excavators may be unable to tell this, particularly when our knowledge of so many types of artefact is so scanty. If a site produces a layer in a position similar to that suggested above, and the two floors were dated to 1880 and 1890, and the only marked pipes produced in the deposit had their stems marked MURRY/GLASGOW then I should say that on the pipe evidence the layer was a redeposition because Murray's was taken over by an apparently new pipemaking firm in Glasgow in 1862 or 1863. But even this seemingly straightforward statement involves several assumptions. I assume the new company - Davidson - removed the old company's names from any of Murray's moulds which they took over and that anyone else who obtained moulds from Murray's at this time did likewise (which is reasonable though there is no direct proof) and I assume that such fragile items as clay pipes would have such a sales turnover that the stock of pipes made by and marked by Murray's would have been sold within a year or two of 1863 at the latest. (This is extremely likely, but it cannot entirely remove the possibility that some Murray pipes could have been stored away and forgotten for 15 or more years.) I also assume that my information on the firms of Davidson and

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Murray is correct - as I have done research on this point myself I can say that all the evidence which I could find available (though this may not include all the available evidence, still less the complete truth) points to these facts being correct. (I have also assumed for this particular example that no other artefacts were found whose evidence was at variance with the pipe evidence - obviously such variant evidence could easily be found among ill-studied classes of artefact.)

However, if the only marked pipe fragments produced had their stems marked McDUGALL/GLASGOW then I could not say (on pipe evidence) that the layer was a redeposition, because McDougall's, on evidence stronger than that available for the date of Murray's closure, commenced pipemaking in 1846 and did not close until 1967.

Obviously then, an archaeologist however good is limited not only by whether stratigraphic evidence can physically be found but by whether he can use the artefact material he has found.

Diffusion is another completely unpredictable variable. Because government departments in general, and military mentality in particular, demand uniformity, issue dates for military buttons or cartridges may well enable a definite chronological line to be drawn for the introduction of such items; no such clarity is likely to exist with items in civilian use unless fierce competition breaks out between rival firms or some external event impinges on an industry such as the publicity given to the cancer-producing properties of cigarettes resulting in a sudden rise in the popularity of filter tips. Again, a change involving the military will be

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enforced countrywide at the same time; (11) amongst civilians, local preferences and suppliers are likely to alter the variants of material introduced and the times they appear. At a national level, coinage in Britain remains in circulation longer than does coinage in Canada. Such permutations and combinations are almost endless - this is, to borrow the phrase from E.H. Carr again, the "continuous process of interaction between the historian and his facts." Brose, at the Custer Road Dump site datable to 1876-95, was able to accurately date eight layers by studying coins, campaign pins, patent dates for glass bottles and metal artefacts, and documented dates for military adoption of equipment. This is an outstandingly successful example of dating without involving any real typology at all, and suggests the way in which archaeologists involved on such sites may be able to achieve a break-through on the welter of mass-produced artefacts which appear on such sites. True, Brose was helped by datable horizons from introduced military material, which would not occur with such clarity on a civil site, but it seems that his methods are the most promising so far in this field.

As I have stated in my previous article, the primary responsibility of archaeology, as with all research, is to knowledge. As Dollar says, ideally one should be able to reconstruct a site either in theory or actuality from the research done, but how often - if ever - is this ideal reached? It is only when one attempts such an ideal that one realizes how inadequate the information available from even the most careful archaeological and historical research almost invariably is. From Dollar's remarks it is

Dollar's Reply: (11) Not necessarily, especially if the site being studied was an outpost. It has been my experience that many 19th century American military forts, especially if they were frontier or outpost forts, were often several years behind times in certain stylistic changes-including the first appearances of new armory and ballistic weapons.

clear that he has worked on sites where restoration has been based on external factors intellectually and perhaps morally incompatible with ethical research.

The problem is that once committed to a reconstruction, even one run on the most enlightened of lines, with no financial, political, or personal problems, and no deadlines, one can never say "I don't know". If no evidence can be found for the number of windows on the second floor or for the pitch of the roof then guesses have to be made. In these examples comparative evidence may well be available, but one controversial feature on a restoration that comes to mind and which is a good deal more speculative is the 'navigation locks' at the Jesuit mission site of Sainte-Marie I (1639-49) near Midland, Ontario, now a major tourist attraction. This site has been excavated twice, first (W. and E. McL. Jury 1954, re-issue 1965). Kidd, excavating the feature which the Jurys later claimed (p. 71, 1965 ed.) to be "undoubtedly the first artificial waterway with locks to be built in the New World", suggested (1949: 79) that "The moat system very likely served the triple purpose of water road, defence, and drainage".

The Jurys noted that there was skepticism among some at first because such an intricate construction seemed beyond the capabilities of people in the backwoods of 17th century Ontario. The problem, however, is not that a locks system should have been built at this time and place, but the purpose for which it was built. As an Ontario archaeologist, Frank Ridley, has put it, it is difficult to believe that voyaguers coming 400 miles from Montreal with hundreds of pounds of goods, crossing 30 portages and miles of

virgin forest and swamp needed three locks to assist them with the last 50 yards. Ridley suggested that the trickle of water in question was dammed for water supply and that the watercourse would also be used for sanitation purposes as was the case with many European monasteries. Father William A. Russell, another Ontario archaeologist, has suggested (Russell 1965) that the 'locks' were in fact part of an undershot waterwheel-operated mill, and he presents various pieces of evidence, including the presence of a miller at Ste-Marie; he also presents evidence against the interpretation of the remains being a lock.

Clearly, all this means only one thing: there is not enough evidence to interpret the wooden remains which both Kidd and the Jurys found in their excavations. As long as findings, deductions, and opinions remain on paper the interpretation is never closed; once they have been committed to construction materials they cannot be altered - the excavator cannot tell those who are doing the reconstruction that he does not know what his finds represent. Yet how often, if we are honest, should we say this. Historical reconstruction is a hard taskmaster. Public money committed to such a restoration is an even harder taskmaster, for no public servant who has committed cash to a restoration is likely to admit that there are any mistakes in the work.

If one is going to do total restoration, then it is difficult to avoid demanding total excavation, with its attendant lengthening of research time. To take an example from my own experience, a cutstone feature was known to have existed on a masonry rampart but its size was unknown and no example had been found during the excavations. Accordingly, an arbitrary

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but reasonable figure for the stone's thickness was decided on. Then, totally unexpectedly, one of these stones, with a different thickness to that decided, was found built into a fireplace base elsewhere in the complex and shortly afterwards two others were found rebuilt into the rampart itself.

However, I should agree with Dollar that one has to undertake every excavation (and in my view prehistoric as well as prehistoric) with the conscious realization that a re-creation - the term I should prefer to use - of the site and its life is the ultimate goal of the work. While much work may be salvage, or part of routine site maintenance, or may be undertaken to ascertain a single point for some non-archaeological reason, the ultimate goal must always be the re-creation, as far as human ability allows, of the site. We are, as Dollar says elsewhere, dealing with persons, not people. As I quoted in my previous paper, we cannot understand the history of prehistoric man unless we become in our own mind a prehistoric man - so also with historic man. Each site excavated is individual to some degree, and the product of individuals.

Dollar's tenth thesis also brings up a point which is certainly more likely to affect historic archaeology than prehistoric archaeology than prehistoric archaeology. Local societies, councils, and individuals do not always take kindly to a treasured local site being declared unauthentic, yet as Heidenreich says (1966: 125) "It is simply not good enough to pick the nearest known archaeological site to one on the map locations as has been done in the past" - he was talking specifically about Contact period mission sites in Huronia, Ontario, but his strictures can be applied very widely.

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There is no point, even if we go no farther than the crassly mercenary and consider it in terms of public money wasted, in hiring an archaeologist to apply his knowledge and skill to a site if local custom, or political or religious pressure, is allowed to over-ride his conclusions.

Related to this problem is another of Dollar's points, which also affects historic archaeologists much more than prehistoric archaeologists. This is the difficulty - in some cases the possibility - of convincing those who are not historic archaeologists but are associated with work in the field - administrators, engineers, architects, and historians - that research on a site cannot be confined to the 'subject' period but has to include the 'non-subject' as well. The lack of comprehension of non-research people can perhaps be understood; but the equal failure of some historians is sheer tragedy. The history of more than half the period of known human occupation on a site may be missing because the historian supposed to work on the site feels that his job ends when the last soldier marches through the fort gate, the last settler packs his waggon, or the last miner drifts on to the next lode.

There are, of course, people who when excavating ignore 'non-subject' material; but a genuine archaeologist records all of his material, in sequence of excavation. Historians, on the other hand, invariably seem to think of the site in terms of what is assumed to be its most important period - its 'subject' period - only. This in turn leads to another remark of Dollar's, in effect that partial reconstruction is worse than none. If

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this involves doing only partial research then I should agree. (12)

Allied to this is the desire by some to improve on history - perhaps the fort had only two cannon, but it will look more impressive, and give the tourist more to look at, if twelve are set up.

Finally, let us not over-emphasize the contribution of archaeology to history. As Harrington has said (1955:1134), the excavation of historic sites has contributed relatively little to history, but added considerably to historical data.(13) Archaeology, as I stated in my previous article, rarely proves anything - its primary purpose is to discover not facts so much as facets. In his paper, Dollar has given us a number of extremely useful points towards defining both the strengths and limitations of historical archaeology: it is up to each of us to establish this new field on a foundation of relevant, practical philosophy, and sound research techniques. (14)

Dollar's Reply: (12) I quote from the "Thoughts" paper: "If an historical site is only 'sampled' and then all additional work neglected, or if a project is terminated prior to completion (such as the General Custer House Site in North Dakota), then the validity of the results obtained is brought into serious jeopardy. An historical site can no more be halfway researched or excavated than can a structure be halfway built, and I think that it is high time that we stop using prehistoric site time/work experience factors to establish the duration of an historical site excavation." (discussion of Thesis #8, last paragraph)

(13) I take some issue with Harrington's statement paraphrased by Walker. No historic site which I have researched and/or excavated has yet failed to reveal previously unknown but discernable historic actions of importance on the part of the occupants. Is this not History?

(14) Walker's Critique, coming as it does amidst the din of scathing bombardments by some critique writers of the "Thoughts" paper, is like a refreshingly calm breeze from across the ocean. One might even be tempted to think that the British -- or Canadians (Walker) and the Indians (Dollar) have again teamed up against the Yankees...Walker's grasp of the theoretical problems involved in historical archaeology is impressive, and he has done an excellent job of presenting his thoughts on this subject. His views will have a pronounced and stabilizing effect on the future development of theory in historical archaeology, and will also no doubt have a sobering effect on the more rabid objectors to the "Thoughts" paper.

(In view of my remarks in my previous article (Walker 1968) on jargon I should like to protest good-naturedly about the phrase "spacial control" being attributed to me (second-last paragraph p. 32). Originally photographs were to have been used to illustrate the points I was making and when lack of finances prevented their publication this section was rewritten.)

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THE CRISIS OF IDENTITY: THEORY IN HISTORIC SITES ARCHAEOLOGY

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Archaeological method must not merely be technically excellent; it must express good archaeological theory. Good archaeological theory demands a conjunction of methods, conjoined on a rational basis of good logic. History and Science have not to be segregated, but identified together.

Charles Frances Christopher Hawkes
1954

The Society for Historical Archaeology was organized in Dallas, Texas in January of 1967. While those assembled agreed on the need of such a society, not all were in accord on its direction. Some alterations in the proposed statement of purposes were strictly political; Noël-Hume's suggested name for the society was accepted because of his concern for what "politicians" think. The real crisis came when the problem of defining historic sites archaeology arose. An attempt was made to accept what seemed to us a very narrow, self-limiting, self defeating definition. This definition which was drafted by Larrabee, Cotter and Noël-Hume proposed that "Historical archaeology is the application of archaeological method to the study of History." After a very narrow vote of 32 to 30, the matter was wisely dropped and the problem of definition was left to the individual members.

While the Society for Historical Archaeology avoided imposing limitation on itself in open meeting, a number of its more vocal members have recently made statements which are as self-limiting and self destructive as those initially proposed at the Dallas meeting. It is the attitude taken by such

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scholars as Noël-Hume (1961), Walker (1967) and more recently Dollar, (this volume) that we view with alarm and dismay. We believe that such a rigid position will severely limit the potential contributions of historic sites archaeology.

The crisis of identity in historic sites archaeology is not unexpected. When several disciplines approach a new body of data, there will undoubtedly be a confusion of paradigmatic theories developed in these different disciplines. Questions dealing with the application of theory developed on one set of data to a new set may be logically raised. The question of whether anthropological theory, developed from the study of primitive cultures, is applicable to contemporary society was raised by Leslie White in his presidential address to the American Anthropological Association in 1964 (White 1965) and has been the subject of a major review article by Leo Despres (1968). The question recently asked is not how do anthropologists dealing with complex societies differ from sociologists, economists and political scientists but rather if they differ at all. The answer has been a resounding yes and the contribution of anthropological theory to the study of complex societies has been demonstrated to be complimentary to, not mutually exclusive of, the bodies of theory which define other disciplines.

This crisis parallels that which is faced by historic sites archaeology today. Historic sites produce a body of data which may be studied in a number of ways and these approaches are also complimentary. To define historic sites archaeology as "a technique of history" or as a separate field of study to itself is to actually limit the extent of its importance.

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Where the authors dealing with historic sites mentioned above have seen fit to identify their academic backgrounds it has been, in every case, history. In contrast the senior author's initial training was in the biological science with later specialization in cultural ecology. The junior author was a communications research student who shifted to a social science program. Our unified approach is through anthropological theory which we have adopted through choice, not by default.

We are very much aware of the differences in the theories of different disciplines and are equally aware that no discipline can exist without theory. Even Walker's (1968) denial of the need of theory has a theoretical base which he either does not realize or can not accept because of his antitheoretical stance.

Walker's article needs special mention since he makes some points which are well taken. However, he is not the first archaeologist to find Taylor illogical or Binford incomprehensible. It is unfortunate that he either was not aware of, or did not see fit to cite, any of the dozen or more better sources for anthropological theory in archaeology.

The paper by Dollar presents a somewhat different problem. The arguments which we find objectionable are as follows:

- (1) Historic sites archaeology should be a distinct discipline with its special methodology designed to deal with its unique problems.
- (2) The artifacts collected from historic sites are the result of industrial processes and are therefore so complex that the analysis of these artifacts can not be based on an objective appraisal of their attributes.

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- (3) The research techniques and methodology employed by specialists engaging in historic site research, principally historians and anthropologists, are so distinctive that they are incompatible.

It is unfortunate but these three suppositions have gained wide acceptance among a large body of scholars engaged in historic sites research and are frequently stated in a circular self supporting argument which can be entered at the point best suited to the bias of the particular observer. Thus, Noël-Hume (1961: 256) views anthropology as the poison in the pudding.

Colonial sites do not, as a rule, commend themselves to most amateur archaeologist or state archaeological societies. The former often fall into two classes, pot hunters and anthropologists, the latter being most interested in the broad culture trends that are to be gleaned from archaeology.

He goes on to brilliantly observe that an anthropologist who digs a colonial farm site will write that the artifacts he finds indicate a barn-like cultural orientation (which is what would be expected if a barn were being excavated). Walker (1967:32) supports Noël-Hume's distrust for anthropology and adds that it's not too late to save historical archaeology from "the confining bounds of anthropology-oriented theory."

From such a base these "colonial archaeologists" seem to argue as follows: Historic sites archaeology is a specialized field of history, while the proper realm of anthropology is prehistory. Because complex historic sites offer different problems than simple prehistoric sites the former sites must be worked by archaeologists who have developed special technical skills designed to meet unique problems of complex

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artifact assemblages. The very complexity of artifact assemblages can be understood only through an intimate knowledge of a particular historic period and not by objective analysis of attributes. As a result, historic sites archaeology should be a tool of history distinct from anthropology. This argument exactly parallels our introductory comments concerning the Anthropology of complex societies. Anthropologists who study complex societies are historians. The anthropologists who study complex societies, however, can benefit from both the sociologist and the historian and the historian and sociologist can benefit from each other.

The argument can also be entered from the point of view that historic sites archaeology is fundamentally different from other types of archaeology and should therefore be constituted as a distinct discipline. For example, Dollar informs us that "It is time to give serious thought to the recognition of historical archaeology as a socio-scientific discipline with a methodology designed to cope with the unique problems encountered during the excavation of historical sites." (Dollar; this volume). Harrington (1952:343) agrees stating:

I think it proper to say that excavation in this field constitutes a new kind of archaeology, on a par with classical archaeology, American prehistoric archaeology or paleolithic archaeology. Historic sites archaeology involves a distinctive kind of site, develops a distinctive approach, both in field techniques and manner of interpretation, and produces characteristic conclusions and results.

Such arguments again provide entree into the argument that since historic sites differ in kind, they require techniques different from those applied by other archaeologists. These new methods must recognize the complexity of historic sites and historic artifact assemblages, a

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complexity which can not be rendered intelligible by traditional anthropological or historical methods. They would argue that the development of new methods can only be accomplished by the establishment of a new and different historic sites field.

Finally, the complexity argument has frequently been evoked by those engaged in historic sites archaeology. Thus, Dollar informs us that "It should be sufficiently clear by this time that variants in the manufacturing techniques of historical sites artifacts totally destroy their value as specific dating tools for the historical archaeologists." Elsewhere Dollar, as well as many others have noted that because a particular category of artifacts were manufactured in hundreds of different factories that it is impossible to observe discrete attributes which could possibly lead to more definitive categorization. Similarly it has been argued that such "complex" artifacts are not amenable to statistical manipulation.

This argument again leads easily into the logical vortex which we have already described. The adoption of this type of theoretical position represents more than a harmless personal bias bred by disposition or training. It, in fact, determines the type of field works, description and analysis which historic sites archaeologists perform. More important, perhaps, this position can determine what kind of a field historic sites archaeology will be; will it be self-contained and self-limiting or will it make substantial contributions to the study of the natural and social sciences.

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The adoption of the theoretical stance which has been described above seems to us to have led to a number of unfortunate and wholly disadvantageous attitudes, methods and conclusions on the part of many historic sites archaeologists. While we don't intend to engage in wholesale refutation of the group, we find the following trends to by and large characterize historic sites archaeology.

(1) The excavation of sites by professional scholars who lack experience in archaeological field methodology. These are primarily historians and "specialist" in various artifact classes, people with interest in art history and architecture or salvage divers.

(2) The excavation of historic sites with the notion that excavation is a simple technical process which may be carried out in a theoretical vacuum partitioned from its analytical or laboratory phase and terminated short of the integrative or synthetic phase.

(3) The analysis of excavation material from a historical bias. The result is dull, unimaginative reports which contribute little or nothing to our understanding of history, cultural phenomena or anything else. These reports become in essence long lists and descriptions of artifacts and excavated building features,

(4) The reluctance to adopt a classification process based on the discrete attributes of artifacts and to use sophisticated analytical techniques in classification has led to the appearance of a cult based on the "mystique of expertise." Thus, specialists can distinguish German from Dutch earthenware on the basis of "experience" or "feel" but are reluctant to set forth specific criteria. We expect that these criteria are either

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nonexistent or are undefinable, untestable, and, therefore, indefensible. The results of this cult is a huge group of specialists, oriented either temporarily (18th Century Colonial), regionally (Great Lakes fur trade) or most frequently topically (weapons, glass or ceramics).

(5) The financial support of restoration programs at historic sites has produced a carnival atmosphere which is hardly conducive to genuine research. Research activities are often seen as peripheral to other activities such as providing evidence for building restoration or entertainment for tourists. As a result, the relatively minor, and certainly preliminary, field phase and analysis of structural evidence is given precedence over solid long term research.

The continuation of these trends will see historic sites archaeology develop as an unimaginative hobby characterized by low level research undertaken by poorly trained technicians who are aided and abetted by hordes of specialists who are, in essence, academic antique collectors.

As an alternative we argue that historic sites archaeology is not a different kind of archaeology but a field which requires the cooperation of a number of sub-disciplines. We argue that the field and laboratory methodology employed by historic sites archaeologists should be objective and rigorous and finally that anthropological and historic phases of research are not only compatible but are complementary and necessary in the understanding of any particular site.

We have thus far presented what we believe are the most prevalent and damaging trends in historic sites archaeology. These are for the most part based on a misunderstanding of the relationship between history,

anthropology and archaeology.

Some years ago White (1945) presented a very instructive matrix to illustrate the differences in temporal and conceptual perspectives which produce differences in the theoretical ordering of natural phenomena.

The matrix shown below is a modification of White's matrix to fit the situation under discussion.

Particularizing	Temporal	Non-temporal
	History	Field Work (Excavation & Artifact Analysis)
Generalizing	Anthropology	Structural-Functional (Sociological) Interpretations

Here we see the major conceptions of primary concern to historic sites archaeology. Field work is done in a non-temporal particularizing framework. In historical archaeology this is field excavation and simple laboratory description. All too often historic sites archaeology does not get beyond this point. The non-temporal particularizing frame, however, is basic for all types of scientific research in which things or events are observed within some context to produce basic data. Higher level analysis can only be as good as the data produced at this level allows.

The data may then be ordered in this paradigmatic form in three ways; temporal-generalizing, non-temporal-generalizing, and temporal-particularizing. The latter is history (specific things and events ordered in time) while the non-temporal generalizing frame provides sociological and social

anthropological conclusions and the temporal-generalizing frame provides the basis for cultural anthropological interpretations. It is important to note that the same data produced by field and low level (laboratory) research can be interpreted in Historical, Sociological and Anthropological frames of reference. Most of the confusion over the role of history, anthropology and archaeology in historic sites archaeology has been the result of confusing these frames. The difference between archaeology and anthropology, or prehistoric archaeology and anthropology, or even the differences between history and historic sites archaeology have been clouded by the lack of reference to such a paradigmatic framework.

Harrington received some criticism when he wrote that excavations on historic sites contributed considerable historical data but results in relatively little history (Harrington 1955:1124). Here Harrington was correctly recognizing the distinction between a non-temporal-particularizing and a temporal-particularizing frame of reference. Archaeology is definitely not, as Judson put it (1961:410), "...a historic subject which reconstructs history from objects."

Once the above distinctions are clear, it is possible to proceed to a consideration of the methods used in the excavation of materials. It is our contention that historic sites do not constitute a unique phenomena in this regard, that they are no simpler or no more complex than at least some prehistoric sites and that they require no field techniques that may not be applied on other sites (see Powell 1967:36 for a similar position). Thus, the central question becomes not who excavates but how well they excavate. We recognize that prior knowledge of historic records and

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documents is as important to the proper excavation of historic sites as the ability to distinguish between trench fill and potholes. Any technically competent archaeologist who has thoroughly researched the history of a site is qualified to dig, any historian who is thoroughly versed in history but lacks competence in archaeological field procedure is no more qualified than an archaeologist who is ignorant of a site's history.

The analysis of excavated materials is not unrelated to the way in which field excavation is undertaken. The computer programmers adage, "garbage in - garbage out," sums up this point of view. Excavations designed simply to collect a sample of artifacts no matter how excellent in execution will not produce meaningful data. Excavations must be problem-oriented and oriented at a high level—locating a specific building known to have existed on a site is not high level imaginative research (see Harrington 1955:1121 and 1126). Designing field research to generate data which can produce significant statements about technology, style, or function in a social, political or ideological context is a worthy goal of field research.

Laboratory analysis of excavated materials must be more than a descriptive process in which each specimen is intimately described (see Noël-Hume 1966a, 1966b). Instead, description should lead to well-defined classificatory systems which account for variation in terms of stylistic, functional or structural realities (see Witthoft 1968: 12-49, South 1967: 33-59, Marwitt 1967:19-26 and Brose 1967). Such systems do not take

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refuge in expertise and therefore may run the risk of revision and obsolescence, yet they provide a systematic and useful framework for spatial and temporal comparison. The analysis of historic artifacts must be based on the quantification of empirical data.

Finally we come to the interpretation of excavated material. It has been noted that this may be undertaken in either an historic (temporal-particularizing), a sociological (non-temporal-generalizing) or anthropological (temporal-generalizing) context. An excellent site report must contain all of these and must be based on excellent analysis and excavation (non-temporal-particularizing). While it is a foregone conclusion that a historic site must be interpreted in the context of the international, national and local events taking place at the time the site was occupied, we see a stubborn resistance to any interpretation which is thought to be sociological or anthropological. Despite persistent urging principally by Foley (1967a:43 and 1967a:66) and some imaginative and useful cultural interpretations (Binford 1962, Dethlefsen and Deetz 1966, Brose 1967), few anthropological interpretations have been attempted for historic sites. None the less, historic sites are potentially well suited for sociological and anthropological interpretation. We would, for instance, expect that our knowledge of 18th and 19th Century trade, transportation, social stratification, political spheres, craft specialization, and acculturation of native peoples could be tremendously enhanced by data from historic sites of this period. These and many other problems which involve the cultures represented by historic sites should be of tremendous interest to historic site archaeologists. While such problems cannot be studied without regard to historical data, neither can historic sites archaeologists who continue

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to ignore these problems expect to fully understand historic sites. We submit that historic sites archaeology can make but few contributions to history but tremendous contributions to other fields of study.

To use part of Walker's (1967:32) recent statement, "Historic archaeology in the New World is a field which is still in its formative stage. It is not too late to make it a field of distinction..." Rather than freeing ourselves from "the confining bonds of anthropological theory" as Walker suggests, we appeal for objectivity, quantification and the unbiased use of both anthropological and historical methodology and add a plea for more thoughtful orientation rather than a conscious limitation to low levels of interpretation as Noël-Hume, Walker, Dollar and others seem to advocate.

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Reply to Cleland and Fitting

Clyde D. Dollar

Cleland and Fitting have clearly spent a great deal of time in putting together their combined critique on the "Thoughts" paper; I only wish that they had spent as much time thinking of the basic problems set forth in that paper. And it was with a great deal of interest that I learned that I had been identified (albeit with "alarm and dismay") with such scholars as Noël-Hume and Iain Walker. This compliment was perhaps unintended, but I will nevertheless accept the identification graciously.

In my opinion, all three of Cleland and Fitting's objections to the "Thoughts" paper are grossly (sometimes shockingly) overstated. This tack might have been deliberate; if so, it was done in panic and with poor judgment. If these objections actually represent what these two writer's feel are contained in the "Thoughts" paper, then either the paper itself or their comprehension of it are in error.

Objection (1): Yes, I feel that historical archaeology should be a distinct discipline with its own special methodology designed to deal with its unique problems. I do not (as Cleland and Fitting strongly imply that I do) feel that this discipline should be separate from either anthropology or history. This would be as foolish as if someone were to suggest that a person's hand, because it was distinct from that

person's body, should therefore be separate from that body. I detect traces of professional panic on the part of Cleland and Fitting in overstating this point.

Objection (2): This objection, as stated by Cleland and Fitting, should be manifestly absurd. In the "Thoughts" paper, I discussed what I felt are limitations to which use of an analysis of historical artifacts should be put, not necessarily the way in which they should be analyzed. Apparently, both Cleland and Fitting completely missed this point. They are not, however, alone in this error.

Objection (3): This statement, as fabricated by Cleland and Fitting, borders on the ridiculous. Furthermore, their objection cannot be supported from within the text of the "Thoughts" paper. Even the most diverse temporally oriented research disciplines (for example, the fields of Mesopotamian and Meso-American archaeology) have theoretical points in common. But it would be just as ridiculous for a researcher trained in Mesopotamian archaeology to lustily descend on a hapless Meso-American site without first being aware of the special theory and techniques used on such sites. To do so - without this understanding - would be to display a lack of professional judgment and responsibility. It is my contention that similar situations have occurred, and will continue to

occur, in American historical archaeology until such time as this discipline is recognized as having its own unique methodology and theory.

The rest of Cleland and Fitting's critique is marred by an extreme and somewhat scathing personal attack on all historical archaeologists in general and certain ones in particular. Furthermore, the exposition of their own theoretical outlook on the subject contains the seeds of its own destruction - as well as clearly shows why this particular approach is inadequate for application to a 'temporal-particularizing' situation such as history. Their critique has merit (and a great deal of this), however, in that it somewhat defines an opposing pole position in the present controversy.

'SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY,'
A CRITIQUE*

Vincent P. Foley
Historic Bethlehem, Inc.

Dollar's paper is open to criticism on two principal levels: the logic and accuracy of his statements. With the exception of the irrelevant listing of some historic developments in nail manufacture, the paper is undocumented. Statements are made as fact, which are not factual, for which no original proof is offered, but from which argument proceeds. Other valid facts are used as men of straw, which, while non sequitur, are presented in such a way as to support his arguments by inference. (1)

It is unfortunately obvious that Dollar's paper reveals serious misunderstandings of archaeology as a technique, anthropology as a discipline, and the relationship between the two. (2) One is compelled to view with regret and sympathy Dollar's unpleasant and undocumented experiences with individual archaeologists. However, this writer cannot accept them as proper for his type of paper, without proof; nor should these experiences be used to characterize our profession.

*Echoing Stanley South's statement as to his reactions on first hearing Clyde Dollar's taped presentation of his paper, I must add that my feelings were greater on the side of disagreement. Each time I reread his words I became increasingly reluctant to make a reply. This paper is submitted for publication only after a meeting with Dollar, at which time I explained my stand, and confirmed my understanding of his views.

Dollar's Reply: (1) A sterling example of an objective, positive, and pleasant opening statement!

(2) The ease with which Mr. Foley judges the professional qualifications of his colleagues is exceeded only by the frequency with which he does this.

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Anyone attempting an evaluation of Dollar's paper will find himself hard put to follow it. While it is obvious that he believes in his statements, they are not logically organized and the reader finds himself confronted with the task of reorganizing the material. Having reviewed his thoughts with Dollar, this writer feels safe in reordering Dollar's paper so that it can be answered. (3)

As the paper promises to lead to a statement of ten theses, supposedly representing the overall argument, I have chosen to begin with them in outline form.

Thesis #1. A statement of fact as he sees it, with no theoretical proposition being offered. (4)

The outlined problems concerning the difficulty of using nail and ceramic types as temporal markers are indeed valid. Every historic site archaeologist is concerned with these problems, and is working to shed light in this area. No archaeologist would attempt to use these artifacts as index fossils at this time. (5)

Dollar's Reply: (3) My meeting with Foley (in Williamsburg last January) had the profundity of a sophomore carnival held in Grand Central Station. Foley's allusion to this meeting as being a serious and free discussion of the "Thoughts" paper is discourteous.

(4) Apparently, Foley overlooked the discussion of this thesis. This statement, and the ones following, certainly suggest this.

(5) Even I, from the fastness of my Reservation, am aware of at least three such attempts being made by various archaeologists. To be more specific at this time would be to pre-judge these attempts; however, to my knowledge, all three of these ceramic analyses are being based on what I have defined as 'extended' archaeological techniques. While this basis would not necessarily prevent useful information from being brought to light in these studies (see my Reply to Jelks, Numbers 6 and 8). I would certainly approach the results with a great deal of skepticism.

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This does not mean that they yield no information. The very use of the term "cut nail" reveals that some typological observations have been made of such artifacts on morphological-technological grounds. Such designation yields more temporal information than just a reference to "nails." (6)

The problem with these and other artifacts has to be met; whether future site studies are performed by archaeologists (anthropologists), historians or individuals from a new "socio-scientific discipline." A realistic historian would readily admit that such subjects are outside his realm. The archaeologist's career is oriented to the investigation of just this type of difficulty with the interpretation of material culture. What revolutionary methods of artifact analysis are to be offered to replace the traditional approaches?* (7)

*

A certain amount of the criticism directed towards archaeologists by Dollar concerning this point is valid. Not in this writer's opinion, because our methods are inadequate to the tasks, but rather because we have not used our methodology to the fullest. I have maintained for some time, and attempted without much success, to convince my colleagues of the need of a good knock-down, drag-out confrontation with learned ceramicists and art historians. Such a vis-a-vis confrontation or more realistically, a series of them, would enable the erection of a valid ceramic terminology based on the aspects viewed by each of the three fields mentioned. For example, the term "pearlware" could be reduced to its proper classification and definition. Is it partly an art-historian term? Does it have stratigraphic temporal limits; does it have a valid chemical rationale?

Dollar's Reply: (6) True; it defines a manufacturing time span for an artifact type with an alpha date of some 170 years ago. This particular artifact type does not as yet have an omega date (see Thesis #4). Of what use this broad information might be, of course, would depend on the circumstances.

(7) See Thesis # 2,3,4,5,6, and 7.

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Thesis #2. A valid proposition, and self-evident. However, Dollar also implies, without foundation, that anthropologists disagree with the proposition he is restating. It is here that he displays a lack of understanding of archaeological technique and blames the seriation technique for his error in its use. (8)

Thesis #3. Dollar's confusion relates to the formal historian's penchant for assigning specific calendrical dates to specific events. While it is convenient and desirable to be able to date events precisely, their lack does not relegate data regarding the events to the unimportant or insignificant. If it were so, anthropology would never have begun to accumulate new data. Furthermore, the proper scientific alignment of data and its use eventually lead to the assignment of specific dates. (9)

Thesis #4. A restatement of traditional and universally accepted scientific assumption. (10)

Thesis #5. While displaying an archaeologist's theoretical problems with classification, Dollar's use of the word "provenience" is ill-advised in view of its more current archaeological usage. (11)

Dollar's Reply: (8) An interesting twist of logic - and the factual situation.

(9) There is "...confusion..." in the "...penchant for assigning specific calendrical dates to specific events..."?!? If this data is to be used in the historical research process, then it must be datable. The last sentence of Foley's paragraph is naive and unrealistic.

(10) Foley will no doubt be shocked to learn (from his colleague's critiques) just how 'universally accepted' is this thesis!

(11) And what is the definition of this "more current archaeological usage"? See my reply to Larrabee, Number (14).

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In several instances Dollar coins terms in his paper to no useful purpose. Terminology already exists to describe the levels of phenomena to which he refers (i.e. "alter period," "alpha" and "omega" dates, etc.). Of what use is the development of a multiplicity of words to describe the same things? (12)

Also in the paragraphs subsumed under this thesis, Dollar restates a basic archaeological concept to the effect that a site should not be studied in regard only to the component of prime interest. But he omits any reference to his example site's history prior to 1817. Was there no "temporal period" prior to this historical component? (13)

Thesis #6. Additional multiplications of terms to define spacial-temporal proveniences of an artifact. Dollar's difficulty with disturbed strata will be remarked upon elsewhere.

Thesis #7. Non sequitur. (14)

Thesis #8. Essentially quite true; but the statements are also true of prehistoric study. (15)

Thesis #9. Dollar's statements are too extreme; initially, with respect to architectural knowledge, which is relative. Differences, if they exist

Dollar's Reply: (12) See the critique by Walker and my reply.

(13) Yes. There was both a Geological and a Prehistoric temporal period. These were covered in my First Fort Smith Report written for the National Park Service in 1965.

(14) For those of us benighted by a lack of training in Jesuit logic, the words 'non sequitur' can be defined as a 'brush off'.

(15) Foley (again) failed to read the discussion of this Thesis.

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(and Dollar's argument falls apart with his wishing away of important examples -- Meso-America, etc.), are ones of degree, not of kind. The western architect and aboriginal builder both arrived at their knowledge empirically. Did the log building pioneer understand structural stresses any better than the Iroquoian longhouse builder? I doubt it! (16)

The latter part of this thesis, dealing with technique, is spurious. Dollar is stating well-developed technic rules which apply to the excavation of structures with buried footings, cellars, etc. They are but a few examples of a long list of historic site types, and concern just one group of an extensive rubric of empirically arrived at "do's" and "don'ts" for all archaeologists.

Thesis #10. Another series of statements leading nowhere; not a thesis. (17)

A syllogism can be extracted from Dollar's paper and seems to be embodied in the following statements. ". . . there is a major difference in the concepts of methodology used in the excavation of a prehistoric site...." "... researchers in ... historical archaeology are encountering problems the solutions to which seriously strain the ability of traditional anthropological methods to solve" (emphasis mine. Therefore, ". . . the field of historical archaeology is coming of age as a distinct socio-scientific discipline ... we must, therefore, critically examine all aspects of the subject in order to arrive at valid new concepts for what is essentially a new discipline,

Dollar's Reply: (16) I doubt it, too, but this does not negate the thesis. Foley's inexperience with the subject of architecture shows quite clearly at this point.

(17) See the Oxford Universal Dictionary on Historical Principles, third edition, revised 1955; reference the word 'thesis', definition #II, 1; page 2170.

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and not necessarily borrow concepts and methodology wholesale from the existing body of anthropological thought" (Draft page 2).

In support of the above reasoning Dollar states that the historian's work is necessarily subjective, but the historian uses two main tools, ". . . deductive reasoning . . . and . . . the research processes of verification" (Draft page 4). The ". . . research framework is very legalistically and microscopically oriented,..." and this verification is necessary for "... valid and useable...." research (Draft pages 4-5).

Dollar's statements relative to an historian's methodology imply that the latter has research tools more efficacious than those of the anthropologist. Again we are faced with factual and logical fallacy as well as a lack of comprehension of our methodology. (18)

To imply that the formal historian uses deductive reasoning as a tool exclusive to his research is absurd in the extreme. No one can reason deductively without first having erected certain valid general principles which, by definition, are arrived at inductively. This is the scientific method, no matter what the subject under consideration. All science aims to erect "laws" or general principles inductively from observed phenomena,

Dollar's Reply: (18) These two sentences of Foley's highlight a major difficulty that exists to a considerable degree throughout the present field of historical archaeology. Foley, as an anthropologist, has (in this critique, at least) displayed little concept of historical research techniques. This does not prevent him from excavating (destructive research) on an historical site where historical data should be the major research product. He nevertheless masks this lack of historical research technique comprehension by proclaiming that the anthropologist's methodology is sufficient to do the job adequately!

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before attempting to expand our knowledge and improve general theorems by application. This latter practice is deductive in nature. One is very tempted at this point to observe that it is the formal historian who insists that general principles do not exist in history. Any practitioner of that art is doomed to vociferous criticisms should he break that rule of thought. The case of Toynbee is an excellent example. (19)

As to the matter of verification, Dollar admits that the formal historian is perforce subjective. (20) To substantiate the validity of a document he

Dollar's Reply: (19) These sentences betray an almost unbelievable lack of comprehension (or misconception) of the field of historical research, and Foley's tenents are so mis-stated as to almost defy reply! I would therefore suggest that he read a sound undergraduate textbook on the subject of historical research (such as Robert V. Daniels' Studying History; How and Why, Prentice-Hall, Englewood Cliffs, New Jersey, 1966). After mastering this, I would suggest that he proceed to any one or all of the following:

William H. Dray, Philosophy of History, Prentice-Hall, Inc., Englewood Cliffs, New Jersey, 1964.

Herbert J. Muller, The Uses of the Past, Oxford University Press, New York, N.Y., 1957.

The Philosophy of History in Our Time, edited by Hans Meyerhoff, Doubleday & Company, Inc., Garden City, New York, 1959.

The Varieties of History, edited by Fritz Stern, Meridian Books, Inc., New York, New York, 1956.

(20) For the remainder of this critique, Foley discusses various facets of his conceptual approach to historical archaeology. The discussion is of interest in that it presents his own decidedly biased viewpoint on methodology. I will not, however, attempt to reply or rebut any more of his statements as this would require an undertaking which would be impractical (because of length, if nothing else) at this time. In my opinion, Mr. Foley does not have sufficient grounding in historical research theory and method (not to mention his demonstrated misconceptions on the subject) for a meaningful exchange to take place.

must subjectively compare it to other subjective documents, equally suspect. It would be folly on my part to assert that this method is not of value -- especially when, without archaeology, it is the only existing method for some kind of verification. But Dollar commits an error of the same order when he forgets that the archaeologist has excellent laboratories for verification, immensely less subjective than historical documents. This is what archaeology calls the comparative method. A site of a particular temporal period can and must be compared with other sites of the same synchronic level, as well as with sites of other periods. This is inductive research, at least initially. My impression of Dollar's disillusionment with anthropological archaeology is that he forgets that the historical phase of archaeology is still in its youthful, primarily inductive stage. We are all impatient to reach the deductive level, and we can do so to some degree, by applying certain of the general principles that anthropology and prehistory have taught us. Data so obtained can be and is being compared with the documentary record. We would have greater reason for concern if we did not have a vast amount of validated anthropological principles to apply.

On Dollar's Draft page 5, he states that archaeology is not the exclusive property of anthropologists. He uses classicists and European archaeologists as examples of others dealing with "things" in the same way. Here we have another area of confusion that is not limited to Dollar's paper. There seems to be a great deal of misunderstanding of the difference between archaeological "field technique" and archaeology in the traditional American sense. There may be little essential difference in the ways

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modern classical archaeologists and American archaeologists conduct excavations; in the field, recovery of data is by way of the archaeological technique. There is most assuredly a fundamental difference in the ways they deal with "things." The way things are dealt with is inseparable from the underlying orientation of the practitioner -- the raison d'être for his excavations. For example, the approach may be a preservational historian's attempt to find supportive data relating to a particular synchronic level of prime interest. On the other hand, the raison d'être could be a holistic anthropological approach which seeks to recover all the cultural data available at the site, including, but not restricted to, the area of prime interest of an institution supporting such research.

When Dollar criticizes the typological and seriation proclivities of archaeologists, he is doing so without fully understanding their meaning or application. If, by his own definition, the archaeological technique is essentially a way of ordering the accumulation of field data, does it not follow that the next step is the ordering of the data? His impatience with these techniques which have caused him difficulty relates back to his reluctance to comprehend the scientific method. He apparently does not appreciate the fact that, as with all generalized theories, sound typologies are developed after years of research on sites of a particular culture in which traits appear with compelling regularity; are objectively patterned; and, when combined with stratigraphic data, present a picture of morphological and/or stylistic development within that culture. It is not a capriciously-arrived-at scheme, and should not be confused with the attempts of some to apply seriation techniques to the surface collections of an unexcavated or disturbed site. The two, while having the same theoretical

basis, emanate from different poles.

Pages seven through nine present additional thoughts revealing the problems which led him to write the paper under discussion. On page seven he states that sites, presumably historic, are "...almost invariably subjected to previous extensive, and sometimes undiscernable, statistical samplings of various types, accidental or deliberate." While he is right that historic sites seem to have suffered from turning and disturbance more than prehistoric, it is a natural concomitant of all sites in or near present urban or well-travelled areas. It is also true that any site's artifactual inventory represents only a partial sampling of the total at the time of occupation. Furthermore, the type and amount of artifact inventory will be skewed by the type of occupation represented. Those factors are the precise reasons why there are, and should continue to be, persons who specialize in archaeology, and are trained to recognize such problems. His assertion that "samplings" due to disturbance are not discernable is a very indiscreet statement. There are not many archaeologists, nor many sites, of which it can be said that strata disturbance is not recognizable. Furthermore, no archaeologist would attempt to erect an artifact typology for a disturbed site, in vacuo; he could only draw inferences from his material to the extent of valid comparative data being available.

Dollar's statements do illustrate with validity a serious weakness in historic site archaeology to date; a weakness for the most part beyond the archaeologist's control. That is the general inability of American archaeologists to apply the problem approach to historic sites. Most excavations are financed by parties having no interest in the scientific

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accumulation of data per se. Such studies will have to wait, or be limited to our southern and western institutionally-sponsored brethren to first employ this technique, or until there is a greater availability of funds for independent research of all historic site archaeologists.

Dollar's belief that the archaeologist is limited to artifacts and their proveniences in his interpretations is simply not correct -- especially on historic sites. When site documentation is available he must use it before, during and after excavation, being careful not to be overly influenced by them preceding and during digging, because of the admitted subjectivity of documents.

While his concept of what anthropology is, and how it views culture is unacceptable, argument of it seems irrelevant to the main issues. His interpretation of a cultural variant relative to culture centers is erroneous, and it is used incorrectly to defend his historian's view of the "great man" thesis -- equating that view with the anthropologist's culture variant. It would be outside the scope and limitations of this critique to explain the anthropologist's view of the great man hypothesis or the fact that that question and cultural variants are two entirely different levels of abstraction. Suffice it to say, that one should consult White's Science of Culture, especially Chapter 9, for a capsulized treatment of man and the cultural process. It should then be clear that the anthropologist does not ignore the named individual (George Washington, Aristotle, etc.) because such data is not available, but rather because it is an irrelevant deterrent to the proper understanding of "man" and culture.

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Dollar summarizes this portion of his argument by concluding anthropology "...is incapable of producing techniques for the recognition of either : specific actions or single cultural contributions of any given individual within any given culture" In one sense I must concur, but on the other hand, it is because anthropology again does not consider the named individual important, on a general theoretical level. He knows that the individual is a reflection of his culture, not the other way around. When Dollar's type of variants exist, cultures allow them to exist within the prescribed limits of allowed cultural deviation. Exceed those limits and that deviant ceases to exist as a part of that culture.

Basically, it does not matter whether an individual is named or unnamed in a cultural study. It is convenient on a particular historical site limited synchronically and diachronically, if we know the name of the occupant. The name, however, is simply another tool used to extract particularly relevant information from the documentary record. The point here is that the availability of that tool is not essential to the proper study of that site.

In the realm of acquiring cultural-historical knowledge it is irrelevant whether Mount Vernon was occupied by an individual known as George Washington or Joe Smith. While the distinction may be important to the sponsor of such a study, or the school child visiting the site, it adds a subjective element to the site which produces for the archaeologist the exact error that Dollar places on the historian.

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By Dollar's implied definition, historic site archaeology is becoming increasingly narrowed in its scope. It is my understanding that most archaeologists involved in these investigations would agree that historic sites archaeology begins with the period (in the Western Hemisphere) with European contact. This definition would perforce include sites of aboriginal occupation at the time of contact.*

It seems to this writer that Dollar's approach is narrow and national-history oriented, seeing no value in the archaeological study of a site other than to supplement the historical record for preservational purposes. He is blinded to the anthropologist's diametrically opposed non-ethnocentric orientation and his attempts to extract more information from sites and artifacts. The anthropologist is not satisfied simply with the kinds of data preservational interests desire, but strives to find cultural significance in each site, and erect comparative methodology usable on

* Since we are interested in culture, and the effects of alien cultures upon each other, this writer feels that European contact need not be direct or vis-a-vis confrontation. In the absence of contradictory evidence, it can be assumed that trade axes received by an aboriginal group must have caused significant cultural traumas with that group, even though the population may never have seen the European who traded those items. If this contention is valid, "European contact" would have to be viewed relative to a particular site rather than the usual broader geographical areas (Jamestown - 1607; Massachusetts - 1620; Mexico - 1492-1520; etc.).

Mr. Dollar would have us focus our attention exclusively on European settlement sites with nationalistic-historical significance, destined for preservation and display. God forbid the reduction of our science to the level of a technical field supervisor of laborers, carpenters, masons and landscape architects. A new "discipline" would thus be created, probably better called National Shrine Preservationists.

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other sites. (21)

Dollar's statement to the effect that historic site archaeology is beginning to evolve into a separate discipline appears to be quite correct. That is as it should be -- especially when formal historians, technologists, art historians, etc. have so much to contribute. What is called for is a discipline of a cooperative nature. Each must have his own place in the study of a site without an attempt, at least initially, to transmutate all into one individual. It must be remembered that such a discipline would be an amalgam of the arts with the science of anthropology -- a science that has existed a relatively short time, but one which has contributed vast quantities of data concerning the history of man and culture. Furthermore being a science, it operates on a different theoretical plane than the arts. Definition is the basis of all scientific thought. Its value is self-evident when one compares the almost universal communication among anthropological archaeologists with the multiplicity of meanings of terms in the arts. In this writer's opinion the scientific approach has proved

Dollar's Reply: (21) Throughout the critique, Foley (perhaps unconsciously) appears to assume the role of the White Knight of Orthodoxy charging out to do battle with the Differing Dragon of Heresy. One can almost hear sounds of the thundering hoofs of the White Steed (of Righteousness), the whine of the Arrows (of Logic), and the death-dealing blows of the Sword (of All Knowledge). Not until the head of the horrifying Dare-To-Be-Different Beast is severed from its loathsome body (that taints the very air by its presence) is Foley satisfied with his performance and signifies his readiness to receive the thundering applause (and perhaps other things) from the by-standing Damsels in Distress. Come now, Damsels, everybody applaude...

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itself far superior to that of the arts in the acquisition and treatment of data. It therefore behooves others who seek understanding of the apparently inevitable amalgam to be sure they comprehend the scientific method before being ready to discard as useless any part of it. They must also be ready to substitute something more appropriate than the method they are attacking. That, after all, is the way science advances. (22)

Dollar's Reply: (22) It is my hope, that 'science' (and I trust also the 'arts' of which Foley speaks) will indeed advance, and that both he and I will learn more about the subjects in which we each claim the other is deficient.

COMMENTS ON CLYDE DOLLAR'S PAPER

David A. Armour

Mackinac Island State Park Commission

My comments on Clyde D. Dollar's paper, "Some Thoughts on Theory and Method in Historical Archaeology," are those of a professional historian who has drifted into archaeology rather than an archaeologist who has become interested in historic sites. Consequently, I will confine my remarks to the areas of my greatest competence and leave other aspects to be commented upon by my colleagues with an anthropological orientation.

What Mr. Dollar's background is I do not know, but he does not write like an historian. Historians, at least those of a recent vintage, take pride in clear and lucid writing unadorned with impressive sounding but often unintelligible jargon. Mr. Dollar's presentation is on occasion somewhat less than clear. Even his definitions are sometimes incomprehensible.

One of my most serious criticisms of the paper comes at the basic level of definitions. In his title Mr. Dollar uses the term "Historical Archaeology," and I initially assumed that there was a common definition of that term. Perhaps I am wrong. I thought that "historical archaeology" is the archaeological investigation of a site which is historically documented or which contains historically documented artifacts. Apparently Mr. Dollar has a much more restrictive definition. This, however, he does not reveal until Thesis 5, when he defines the historic period as "the period of cultural expression (and deposition) with which the historical archaeologist is most concerned from the standpoint of recovery of historical information." He contrasts this "historic period" with the "provenience period" or the remainder of the site's occupation, which is often later and consequently contains data from documented times.

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Consequently, he narrowly limits his "historic period" to the short period of a site's occupation which particularly interests the researcher. For example, at the first Fort Smith site Dollar claims that the historic period is only from 1817 to 1834 when the fort was garrisoned by soldiers, even though people lived on the site at later times. Thus, for Mr. Dollar the "historic period" is completely subjective and dependent upon the researcher's particular interest at the time of excavation.

Mr. Dollar's restrictive definition and outlook on historic sites has led him seriously astray in other ways. One of these points is his fascination with architectural remains. In Thesis VIII he asserts that "the discipline of historical archaeology must be architectural in orientation and reconstructive in both purpose and scope." True, many historic sites are centered around structures, but not all. Numerous historic sites have little or no relationship to buildings yet are of great importance in providing information about past cultures. Certainly the trade goods retrieved from the swirling waters of the "voyageurs highway" tell us much about the nature of the Indian trade, yet these artifacts are totally unrelated to any structures. Nearly all underwater archaeology is unassociated with building remains unless one considers sunken ships as structures. Furthermore, burials, dumps, refuse pits, and many other types of sites are not basically architectural in character.

Perhaps my most serious quarrel with Mr. Dollar is that he erects a false antithesis between the methodology of history and archaeology. He claims that history as a discipline can arrive at a much closer definition of the "truth" about the past because it is deductive in character and applies some unexplained "tests for validity." The impression created

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is that historical research methodology is basically different from archaeological research. I fail to agree. Archaeologists on occasion are deductive in their approach in working from generalization to the particular, and historians are often inductive in analyzing bits of information to try to form a generalization. Neither discipline can be classed as exclusively inductive or deductive. The major difference between the disciplines is the type of data with which they work, rather than their methodology. The historian searches out and analyzes dusty documents, while the archaeologist works with information retrieved from the soil. Furthermore, both historians and archaeologists work with fragmentary data, and, despite efforts to the contrary, they analyze it from a subjective point of view.

Dollar creates the impression that history is much more accurate than it really is. The documentary evidence about a historic site is often extremely sketchy and sometimes non-existent, leaving large gaps in the time sequence on a site. These only archaeology can fill. When documents do exist they often provide specific information which archaeology could never reveal. However, where only scattered historical records survive, the historian's interpretation may be even more subjective than the archaeologist's.

Both historians and archaeologists provide an incomplete and, despite our best efforts, incorrect picture of the past. Yet by working together and by providing each other with data, it is possible to come closer to the truth than could either discipline working alone. It is this cooperation which is important rather than trying to change archaeologists into some type of pseudo-historians capable of producing the impossible—"totally and non-distorted historical data."

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Reply to Armour

Clyde D. Dollar

Dave Armour's critique is puzzling (to me at least) as it contains the largest number of misconceptions I have so far encountered in this debate. If a student were to submit such caliber work, I would return it (probably with a nasty note) for total re-reading and revision.

I would therefore suggest that Dave read the "Thoughts" paper, and not just scan it, before again attempting to offer his contribution to a body of theory for Historical Archaeology.

A COMMENT ON CLYDE DOLLAR'S PAPER

John D. Combes
Institute of Archaeology
University of South Carolina

I find Stan's introductory remarks interesting because I, too, watched several taking notes during Mr. Dollar's presentation, as well as an especially attentive audience. This reflects a thirst on the part of all of us engaged in historic archaeology for formalized methods and theory. This kind of thirst is only wishful thinking, however, for nowhere will we come up with a "cookbook" for excavating historic sites, thus giving us an "easy way out." The majority of us involved in this kind of research in North America are anthropologically trained and have received little, if any, formal training specifically in historical archaeology. The question is this: Is an anthropologically trained individual ill-equipped to handle the excavation of historic sites? (1)*

Mr. Dollar's first point is a call for "the recognition of historical archaeology as a distinct socio-scientific discipline with a methodology designed to cope with the unique problems encountered during the excavation of historical sites." He is implying a great deal here. Need historical archaeology be a distinct socio-scientific discipline? Are the problems encountered while excavating unique and do they require the design of a new methodology? My answer is no. Fundamentally, I see very little difference between prehistoric and historic archaeology. The differences are few and, I don't think, significant.⁽²⁾ Certainly the historical archaeologist must make use of an historian, or better yet, familiarize himself thoroughly with

* Dollar's reply to the numbered points can be found at the end of this paper.

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the primary source material, but this is much like the prehistorian's use of ethnographic data. Studying the artifacts is also much the same. We look in different places and historic sites yield a much greater range and much more complex array of material items for study, but the ultimate objectives are the same.⁽³⁾ Whether we are excavating an historic site or a prehistoric site, a statistical approach may be appropriate or may not be appropriate, just as a typology may or may not be useful.⁽⁴⁾ Mr. Dollar might argue that the sites are different, and again I disagree. Historic and prehistoric sites are both subject to specialization, short occupation or long occupation, and both may be occupied by one culture or multiple cultures. I am not suggesting that an individual try to do both, for to do so is simply inefficient use of one's time.⁽⁵⁾

It is pointed out strongly by the writer that the main purpose of historical archaeology is the reconstruction of, or interpretation of, the site, and he suggests that if one does not agree with this they are perhaps not historical archaeologists at all. The interpretation of historic sites is important, but in my mind only a by-product of the study. We must go further than that. In addition to supplying data for interpretive purposes, why not study human behavior?⁽⁶⁾ Why ignore a previous or subsequent occupation of the site? Is it not significant that there is a discrepancy, let us say, between the journal of a 16th century observer and the archaeological evidence?⁽⁷⁾ I would like to take the liberty to make some additions to Mr. Dollar's assertions above and say this: If you are engaged in historical archaeology and you are not interested in human behavior as well as public interpretations, you ought not be doing historical archaeology if indeed you are anyway! This kind of difference between us is clearly a reflection

of our different backgrounds and training. (8)

Mr. Dollar also argues that techniques of archaeology are field techniques only. One also occasionally hears that a school boy may be taught the field techniques and do a reputable job of excavating. There is certainly more to archaeology than simple technique.⁽⁹⁾ The field worker, unlike a lab technician, cannot get by with a predetermined set of operations in order to obtain data. The destructive nature of archaeology requires, in many instances, on-the-spot interpretations in order to proceed properly, and the nature of each site may require a different kind of approach. Therefore the excavator must be more than just a technician, and it is imperative that he also be involved in the interpretation of the phenomena encountered. He also points out that archaeological techniques are not the sole property of the anthropologist. This is true, certainly when we go outside North America. However, if it were necessary to choose between an anthropologically trained archaeologist and, say, an historian (both unfamiliar with historical sites), there is no question who is best equipped to excavate the site. (10)

Finally a brief word concerning the then theses. By no means are any of these notions new, or for that matter unique, to historical sites. All ten of these theses are self-evident statements that I would consider elementary concepts and need not be discussed by professional archaeologists.⁽¹¹⁾

Discussions concerned with methods and theory in historical archaeology are for the most part a waste of time, just as are discussions concerned with the justification of doing historical archaeology. A long, drawn-out discussion of Mr. Dollar's paper serves to do nothing but attempt to impress one's colleagues with one's vast knowledge and experience with the excavation

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of historic sites. My personal feeling is that our time may be spent more profitably with discussions dealing with the materials recovered, their analysis, architecture and the many other problems with which we are faced that will concretely aid our research.

Mr. Dollar's paper spells out quite clearly to me why anyone engaged in this kind of research should have at least some background in anthropological archaeology. (12)

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Reply to Combes

Clyde D. Dollar

(1) As John Combes is an anthropologically trained individual already 'handling' the excavation of historic sites, the answer to his self-posed question is not hard to imagine.

(2) Perhaps Combes failed to read the discussion on Thesis Nos. 8 and 9. I would hardly call these differences 'insignificant'!

(3) The objectives, yes, but the methods of arriving at these objectives differ considerably. As I have pointed out, the prime difference between anthropological and historical methods is the degree of acceptability, therefore the applicability, of the evidence derived archaeologically.

(4) I certainly do not deny this. What Combes has missed in reading the "Thoughts" paper is my contention that a statistical approach ('extended' archaeological techniques) is not applicable at an historic site merely because it is a statistical approach. There must be validity demonstrated before the results of such an approach can be accepted for use in the historical research process.

(5) If, as Combes suggests, prehistoric and historic sites do not differ to any great degree, then why would it be 'inefficient use of one's time' to try to do both?

(6) Because all too frequently human behavior on an individual level cannot be discerned archaeologically at an historical site. If it can, and the evidence for such individual behavior is permissive for use historically, then by all means do so. Collective human behavior at an

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historical site is the sum and substance of what is being studied. In order to clarify any possible confusion in the reader's mind regarding my use of the word 'interpret', I would like to quote from a study I made several years ago for the North Dakota Park Service:

There are three basic facets of any historical area which, if the visitor's encounter with the site is to be successful, must be very clearly interpreted. Basically, the visitor wants to know:

1. The historical significance of the site (why was this site here, when, and what happened here);
2. the historical appearance (what did the site look like), and
3. the cultural significance (how did the people who were at this site live). (Quoted from Dollar, Clyde D., A Comprehensive Plan for an Historical Archaeology Research and Development Program for the North Dakota Park Service, Bismarck, North Dakota, February, 1967, page 39.)

(7) The answer to this, and the previous question, should be obvious.

I trust that Combes is not implying that I suggested: (1) That previous and/or subsequent occupation of an historical site is unimportant, and (2) that discrepancies between historical sources and archaeological evidence are both unimportant and uncommon. If Combes seriously thinks that I suggested the above at some point in the "Thoughts" paper, then he has considerably misread the paper.

(8) I repeat: human behavior, as expressed in material artifacts, is the sum and substance of what is studied in historical archaeology. Human behavior, as expressed in written documents, is the subject of the study of History. Presumably, Combes denies these assertions and would restrict the study of human behavior to only the fields of anthropology and ethnology.

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(9) What, may I ask, is 'simple' about archaeological field techniques?

(10) There may be no question in Combes' mind about this point, but there is considerable question in mine. I might add that this question has not been resolved by any argument so far presented by Combes.

(11) It was most enlightening to me, and I am sure that it will be also to the other participants involved in this debate, to learn that Combes considers the Ten Theses ". . .self-evident. . ." and ". . .elementary . . ." (!). This 'I-knew-it-all-along' attitude belies the caustic approach so far exhibited in Combes critique (or vice versa). In my opinion, at no other point in Combes' critique does his lack of comprehension of the "Thoughts" paper become more obvious than here.

(12) It would appear that Combes is of the opinion that "all History is foolish and all Historians are fools. . ." To him, and to others with similar narrow-minded opinions, I would say this: Historical Archaeology is not a subject to be 'dabbled in' by those of exalted positions within the field of Anthropology—unless these individuals know and can apply 'rules of evidence' acceptable to historical research. And, exalted position notwithstanding, John Combes in this critique at least, has failed to demonstrate his comprehension of an historical research approach to me.

CRITIQUE OF THE PAPER BY CLYDE DOLLAR,

" SOME THOUGHTS ON THEORY AND METHOD IN HISTORICAL ARCHAEOLOGY "

Edward McM. Larrabee

Clyde Dollar has nailed his theses (ten in this case) to the door in Macon, and called for a reformation in both method and theory. Without venturing into religious controversy, I will simply observe that one must acknowledge that there is usually both truth and error in such proclamations, whether one welcomes them (as I do Dollar's) or considers them heretical. Such is the case here. However, Dollar's stated purpose was to stimulate discussion, and I am sure that he has done that.

General Comments

Those who have excavated a number of historic sites will recognize and sympathize with many of the problems cited by Dollar, and will agree that some of them are particularly present in our work. However, as our experience broadens, we see that some we thought were unique to our specialty are shared by other professions. Thus my overall response to this paper is that it reflects too narrow a picture and too ingenuous an attitude. Many of the sins which Dollar belabors seem to be peculiar to the Plains. It is not that he is wrong to criticize these regional practices when they are applied wrongly on an historic site—simply that each region has a similar situation, and these particular practices are not the major problem elsewhere that they seem to him. (1)*

The same is true of the theme that Historical Archaeology must be largely structural or architectural. This seems to be an over-reaction to something like some Paleo-Indian kill-site excavation, where the

* Dollar's reply to these numbered points follows this paper.

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investigators found only artifacts. It is a mistake to limit our work to architecture. Some of the best 'pure' Historical Archaeology has been non-structural, in a Virginia garbage pit and some Massachusetts church-yards.* A variety of other non-architectural problems come to mind, such as excavating a battlefield or field fortification or a farming operation, where the evidence is soil disturbance not associated with any building. The spilled canoe-loads that Walter Kenyon is examining in Ontario are another sort of historical archaeology. Detailed analysis of one type of artifact, such as that done for clay pipes by the late Geiger Omwake, is also valid archaeology. (2)

Specific Comments

There are a number of points in the paper which require comment. A major one stems from the attempt to characterize history and anthropology as two symmetrical opposites, the first deductive and microscopic in approach, the second inductive and macroscopic (pp. 4-10).(3) Not only is this too neat, but it is not true. Neither discipline can be pigeon-holed so easily.

* Ivor Noel Hume, Excavations at Rosewell, Gloucester County, Virginia, 1957-1959, U.S. National Museum Bulletin 225, pp. 153-229. Contributions from the Museum of History and Technology, Paper 18, U.S.G.P.O., 1962

Edwin Dethlefsen and James Deetz, "Death's Heads, Cherubs, and Willow Trees: Experimental Archaeology in Colonial Cemeteries," American Antiquity, Vol. 31, No. 4, April, 1966, pp. 502-510

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Similarly, in criticizing the use of techniques which produce only 'general truths' (e.g. seriation)⁽⁴⁾ on a site where "particular truths" are needed (e.g., confirming the association of a particular dwelling with a particular person at a particular time), Dollar misses the point that the "general truths" should still hold, and provide an important check on traditional identification of a site. If they don't hold, they need refinement, or more sophisticated application.⁽⁵⁾ Perhaps the "general" methods applied at Fort Lincoln, Nebraska, would tell us only that it was a U.S. Military establishment occupied for a few decades in the latter nineteenth century.⁽⁶⁾ You say we already know that. However, just that sort of apparently obvious information has been needed to avoid falsely ascribing a feature to the "historic period" of a site, at the Fortress of Louisbourg, N.S., and at Fort Tompkins, Sackets Harbor, N.Y., for example. Furthermore, the general method can be refined and improved only by applying it to the "specific" site, where corrections can be made. This is like calibrating the sights of a gun by using a target of known location.

The argument that the "general" (Dollar calls them "extended") techniques are not sufficiently accurate is mistaken, too, in believing that other techniques yield some different sort of truth.⁽⁷⁾ All knowledge is only relatively true or false. The differences are only of degree. Collecting documentary references from scattered sources is just as much a form of "sampling" as is collecting artifacts from a trench.⁽⁸⁾

Now I agree that the anthropologically trained archaeologist digging his first historic site is facing new problems (p. 11), and I feel that much difficulty has stemmed from precisely this situation.

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However, it is not because Historical Archaeology is a "whole new discipline" which he is facing—the methods and techniques of digging and analyzing are but variants of those he already knows. It is because he is moving some distance into History, which is a different discipline.⁽⁹⁾ In as much as Archaeology is largely a technique, there is a great deal of disciplinary overlap in it.

This basic problem, which has bothered a number of Historical Archaeologists before Dollar, can be stated as follows: Archaeology is largely, if not entirely, a technique, used by many different disciplines to study physical evidence of past human activity for their particular purposes. For example, the Anthropologically trained archaeologist excavating an Indian site is practicing Anthropology with data from the past. Now, if that same scholar steps into another discipline, unless he is master of both, he is out of his specialty. To continue the example, a person does not become an historian simply by finding and reading an historic map or document (i.e., by handling historical data), any more than he becomes an anthropologist by interviewing some Indians. Therefore, when this anthropologist trained to use archaeological techniques applies them to an historic site, more often than not it is only the technique he brings with him, not the approach and theory which are needed to understand this date in terms of his discipline. He is acting only as a technician, not as a fully professional scholar. No matter how good his workmanship nor how careful his techniques, including methods of analyzing archaeological data, his conclusions will be only those of a technical study.

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This has been a frequent occurrence at historic sites, and the result, while it may be technically competent, is neither History nor Anthropology, and therefore probably not even adequate Archaeology. The solution to this is not simple, and involves hard work for everyone. In the first place, trained and practicing historians should learn to apply the methods of Archaeology to their subject. This alone will require a minor revolution. In the second place, anthropologists, who will continue to provide most of the people trained in Archaeology in North America, should do two things. First, they should apply their entire theoretical approach when they work on an historic site, not just the techniques of field excavation. After all, an historic site is also a cultural site, and can be treated as such with profit. ⁽¹⁰⁾ The second thing they must do is treat the historical data, its analysis, and the historians working on it, with full professional respect, which the historians must reciprocate.

Even if a job is so small that one person will do it all, he must keep his roles scrupulously separate, in order to analyze each body of data by its own rules. Then he must combine them, to understand the site. If he mixes his evidence piece-meal, before each discipline has been fully applied to its data, he will only muddy his thinking and misuse the rules of evidence. Recognition of the importance of these procedures would clarify some of Dollar's statements and would have prevented the exaggeration and error in others.

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The "Ten Theses"

Thesis 1 must have grown out of the desperation we all feel at times when dealing with the overwhelming material refuse of industrial production. However, it is naive to assume that we know nothing, simply because we know only when a particular glaze was introduced, but not when any specific manufacturer may have started using it. All we ever know for most artifacts, even a dated coin, is a theoretical date of introduction. (Consider the Kennedy half-dollars issued in 1965 but continuing to bear the 1964 date, to lower the collectors' value of the first ones and keep them in circulation.) The elemental principle of ante quem and post quem are built on this. But it is by compiling this sort of knowledge for a variety of objects that we narrow the date-range. We cannot simply throw our hands in the air and say that the mass of material is "almost beyond comprehension" (p. 14)⁽¹¹⁾. Instead, we must try to find ways of comprehending and using it.

Thesis 2 is unrealistic in demanding "totally non-distorted historical data" (p. 15) from any technique—there is no such thing.⁽¹²⁾ The "early" dates from buttons at Fort Smith and ceramics at Nauvoo probably could be explained by more sophisticated assumptions of retention and loss of objects.⁽¹³⁾ My own experience with buttons would indicate that they can be used only in the most general way.

Theses 3 and 4 are reasonable, if a bit redundant. It is bad writing to take a good word like "provenience," in Thesis 5 (pp. 17-18) and give it a special meaning at variance with the one it has normally.⁽¹⁴⁾ This is especially so when the word is particularly current in our professional

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literature in its normal sense. There are perfectly good words for describing the 'temporal span' of a site. The point about "alter" period (p. 18) is well taken, although the word seems awkward.⁽¹⁵⁾ However this, too, is not unique to Historical Archaeology. Schliemann was looking for just one particular Troy, no matter how many others he had to dig through.

Thesis 6 indicates a lack of experience with deeply stratified historic sites, and with the literature of work on sites where 'micro-stratigraphy' is significant (as in much Romano-British work). Adequate recording should provide the information required. This is a false issue.⁽¹⁶⁾

Thesis 7 is overstated. It may be true that a single object cannot be dated by its provenience (but consider a sealed deposit), nor can a site be dated by a single artifact (which is true in any Archaeology). However, the artifacts definitely are still useful as dating tools, and should be used as such. I might add that no single document gives a safe date for a site either. (17)

Thesis 8 has already been considered, under the discussion of architecture. Again, far more Archaeology (besides ours) is "structural" than Dollar shows--and one of the serious faults of Historical Archaeology is that it is too 'structure-oriented.' Usually that is all our 'clients' are asking for. We must lean in the other direction.⁽¹⁸⁾ It is true that we should learn to recognize the architectural features of the culture we are studying--but that is true of any Archaeology.

Furthermore, all Archaeology is "reconstructive" (p. 24) in the sense Dollar uses. But he misses a most important fact by not

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recognizing the limits of our knowledge. We must always present as full a picture as possible, within the limits of our evidence. Beyond that, we are speculating. Dollar rolls two stages into one when he proposes that the archaeological synthesis could be the basis of architectural reconstruction. (We are slightly off track, since physical reconstruction is not synonymous with Historical Archaeology.) A cardinal rule of clear thinking in reconstruction is to keep each class of evidence separate, and rigorously tested by its own methods, before bringing them together. This process produces a profitable "dialogue" between History and Archaeology. From 1963 through 1965 such a system was used at Louisbourg, and the dialogue was gathered in a "Summary Research Report" for each unit of study. (19)

When Dollar urges caution in physical reconstruction (p.25), he is 'right, for the wrong reason.' The most important reason for not rebuilding some structure is that we don't know enough. The old rule-of-thumb, that there will be trouble if more than about one quarter of the appearance of a reconstruction is based on conjecture or 'the typical,' has been proven numerous times. If the percentage of conjecture is any higher, the risk of being proven wrong by further research rises to a point where full-scale reconstruction becomes a poor investment. Some less specific form of memorializing is indicated. The plan to reconstruct the Thomas Lincoln Cabin at Lincoln Boyhood National Memorial is an excellent case of this.

It can always be debated that some sampling is worse than no digging at all (p. 25). This must be judged in each specific case, as no generalization will hold.

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Thesis 9 suffers from lack of acquaintance with other structural excavation, and misuses "reconstructive" (p. 27). What we seek to be able to recreate (mentally) is past human activity. Structures are simply one manifestation of this. (20)

The reservation expressed in Thesis 10, especially that "any previous historical identification of the site must be considered suspect" (p. 28) are good. However, to claim that "authentication" (why the pleonastic "fi"?) is "usually unique to historic sites" simply ignores most Biblical, Near-Eastern, Classical, and Medieval Archaeology. The principle is an elementary one, although worth restating.

Conclusion

Perhaps the major fault of the paper is overstatement, if this is a fault in trying to elicit debate. Dollar is right to say that we need to refine our tools, and to develop some particularly suitable to our specialization. But he is simply using jargon (which is present elsewhere in the paper) when he says that Historical Archaeology should be "a distinct socio-scientific discipline with a methodology designed to cope with its unique problems" (Point 1, p. 30). None of the things he thinks so peculiar to our work are unique. It is simply a matter of emphasis. (24)

Because Archaeology, whatever other aspects it may have, is largely a method and technique of investigating the physical evidence of past human activity, the problem is how to apply this to the study of historic sites,—how to find what parts of the method are most useful to us. But this must be done in terms of the larger context. Historical

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Archaeology cannot exist as a separate "new discipline" (p. 2) because it is an area of disciplinary interaction or overlap.⁽²⁵⁾ We must accept this, if we are to benefit from this desirable situation. Further, we must realize that we are part of a larger field of activity called Archaeology. Our goals do not differ from those of other forms of this, except in detail. The logical conclusion of Dollar's argument is that we should look inward, but his paper proves that we need more to look outward, and to realize the potential advantage of being part of a larger discipline, with communications to other fields.

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Reply to Larrabee

Clyde D. Dollar

(1) I trust that Ed does not mean this to be as smug as it sounds. The Ten Theses presented in the "Thoughts" paper are certainly not regional in application.

(2) The very fact that Ed Larrabee, one of the more thorough historical archaeologists of my acquaintance, would rise up in righteous indignation against Thesis No. 8 is certainly proof of a need to start thinking in terms of architecture at historical sites. It is just as important to discover how a fortification was constructed as it is to determine its present configuration and who constructed it. And rare indeed is the battlefield that did not have some type of architecture directly associated with it. Thesis No. 8, it should be pointed out, does not exclude all types of research except the hunt for architecture; it does state that more emphasis must be placed on the recovery of this type of data. Larrabee is perhaps limiting the word 'architecture' to a meaning associated only with four walled structures (see among others the Oxford Universal Dictionary on Historical Principles, third edition, revised 1955; reference the word 'architecture,' first definition, page 94).

(3) Because one is deductive and microscopic in approach and the other is inductive and macroscopic is no statement that the two are "symmetrical opposites" (quote from Larrabee). I agree with Ed that such over-simplification would be too neat and not true.

(4) I can find no place in the "Thoughts" paper where I equated seriation with "general truths," and I trust that Ed did not really mean to imply that I did.

(5) Ed gets the cart before the horse at this point, and then expects the poor beast to push it uphill! The results of seriation techniques must be proven to be applicable at a given site before they can be accepted (from the standpoint of historical research) as hypotheses at that site.

(6) This apparently is an error. To my knowledge, there is no "Fort Lincoln, Nebraska." There is, however, a Fort Abraham Lincoln, North Dakota. As Ed has already suggested that I am somewhat lacking in knowledge of sites outside the High Plains area, I shall now return the compliment.

(7) I did not, to my knowledge, state this in the "Thoughts" paper. The matter of 'Truth' and its degrees is best left to the philosophers. For a further discussion of 'extended' techniques and their use at historical sites, see my reply to Stan South, numbers 1 through 10.

(8) Excellent point. This is a major reason why historical and archaeological data must be constantly crossed checked.

(9) . . . "some distance" . . . ??? Why not all the way?

(10) As long as the approach by the researcher is not limited to the Anthropological approach.

(11) What is the difference between describing industrial production material as being "almost beyond comprehension" (my quote) and "overwhelming" (quoted from Larrabee six lines back). It looks as if two pairs of hands are in the air! I agree with Ed that we must find ways of comprehending it and using it, and for this reason, I submitted Theses Nos. 2 through 7.

(12) Interesting commentary on Ed's own standards of research. I can see that both of us have peeped into the darkened room of Philosophical Truth and have both come away somewhat disturbed at what we could not see.

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(13) Perhaps the assumptions of retention and loss of objects were a bit more sophisticated at Fort Smith and Nauvoo than what Ed realizes. Otherwise, we might not know that certain artifact information from these sites was in fact distorted.

(14) On the contrary. It is perfectly acceptable writing to define a word ('provenience') as I did in the first sentence of Thesis No. 5, and then use it in this defined sense. Perhaps I am not acquainted with the literature in which Ed claims this word is used in its 'normal' sense. In fact, I have yet to discover a 'normal' definition of this much used word, and a close reading of each usage of the word in our professional literature suggests a wide variety of meanings, and certainly not a 'normal' one. Stepping outside 'our professional literature,' the Oxford Dictionary on Historical Principles (already cited) states that the word was first used in 1882, is 'common in the U.S.' (page 1608), and is equal in meaning to the word 'provenance' (defined on page 1607). My use of the word is compatible with this latter definition.

(15) I agree: the word is awkward. See my reply to Walker.

(16) Ed's comments suggests a complete mis-reading of Thesis No. 6. The issue discussed in this Thesis is anything but false.

(17) Overstated? No. If it is true that a single artifact cannot be dated by its provenience (and I would not exclude a sealed deposit), then of what validity are dozens more of the same? Isn't this a use of 'extended' techniques? This thesis should have been thought through to its logical applications before it was branded 'overstated.'

(18) Needless to say, I disagree.

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(19) I suspect that the storms at Louisburg have colored the interpretation of this thesis to no small extent. In this paragraph, and the following one, Ed gets considerably off track in his remarks. It is my contention, as stated in the discussion of Thesis No. 8, that Historical Archaeology must be reconstructive in both purpose and scope. I quote from this discussion:

The purpose of historical archaeology must be to achieve, insofar as possible, the goal of complete understanding of the history of any given site, and the scope of such research must be to include the recovery of all evidence of historical cultural expression at that site, including all architectural evidence.

Did I miss the point of the 'limits of our knowledge?' I think not. Did I propose that the archaeological synthesis 'could' (Larrabee's use, not mine) be the basis of physical reconstruction? Indeed, it must, if in fact physical reconstruction is to be attempted. Am I "right, (but) for the wrong reason" by urging caution in physical reconstruction? Did I at the same time exclude the possibility that insufficient evidence might be known in order to attempt an accurate physical reconstruction? This is absurd, and Ed Larrabee should have read more closely before making such statements!

(20) I note that Ed fails to detail the "lack of acquaintance with other structural excavation" from which Thesis No. 9 supposedly suffers (I trust that he has himself had excavation experience with sites other than forts). And, he had taken the trouble to read the discussion of Thesis No. 8, and more specifically my definition of 'reconstructive' (as quoted from the "Thoughts" paper in the above paragraph (No. 19), he would not have misunderstood my use of the word. It will be of interest

(No. 20 cont'd.)

to the reader (and is a bit aggravating to me) to note that Ed gives me hell for what he considers my "misuse" of the word 'reconstructive,' and then immediately follows this with his use of the word—which is identical to the way I used it!

(21) 'Identify' is to 'identification' as 'authenticate' is to 'authentication.'

(22) I trust that Ed has not found too many Biblical, Near-Eastern, Classical, and Medieval archaeological sites situated in the United States.

(23) An interesting statement when compared to earlier statements contained in Ed's critique.

(24) Isn't this in itself a unique problem?

(25) See my reply to Cleland and Fitting regarding use of the words 'separate' and 'distinct.'

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Epilogue

Clyde D. Dollar

The printed word is a wonderful media for the spread of knowledge, but I am convinced that no other invention of the mind of Man has created so much confusion, misunderstanding, or ambiguity. The "Thoughts" paper, its critiques and replies, is a case in point. It was my original intention to write this section of the dialogue by first setting forth the individual theses and then listing under each of these the pertinent remarks taken from the critiques. Unfortunately, this approach has proven to be too cumbersome and lengthy. Perhaps it will make an interesting project for historians of Historical Archaeology in the future. I have therefore modified my approach in writing this section to include only those statements from the critiques which relate to the individual critique writer's opinion of the paper in general. These are presented (in a random order) as follows:

In his paper, Dollar has given us a number of extremely useful points towards defining both the strengths and limitations of historical archaeology: it is up to each of us to establish this new field on a foundation of relevant, practical philosophy, and sound research techniques. (Walker)

While the Society for Historical Archaeology avoided imposing limitations on itself in open meeting, a number of its more vocal members have recently made statements which are as self-limiting and self-destructive as those initially proposed at the Dallas meeting. It is the attitude taken by such scholars as Noel-Hume (1961), Walker (1967), and more recently Dollar (this volume) that we view with alarm and dismay. We believe that such rigid position will severely limit the potential contributions of historic sites archaeology. (Cleland and Fitting)

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. . . my overall response to this paper is that it reflects too narrow a picture and too ingenuous an attitude. (Larrabee)

I agree with Clyde that it is a propitious time for historical archaeologists to ask ourselves "Who are we?" . . . When we can agree on who we, as historical archaeologists, are perhaps we can then consider what we should become. Then, too, we can turn to the prehistoric archaeologist and ask him, who he is. (Moore)

As for the exposition of anthropological, historical, and archaeological concepts, theories, methodology, and methods in "Some Thoughts on Theory and Method in Historical Archaeology," there is nothing to be said concerning it in a short reply that will help. When the statements are not in error, confused, obscure, or ambiguous, they are either unduly contentious or painfully obvious. (Fontana)

Finally a brief word concerning the ten thesis. By no means are any of these notions new, or for that matter unique, to historical sites. All ten of these theses are self-evident statements that I would consider elementary concepts and need not be discussed by professional archaeologists. (Combs)

This seems to me to be a document worth serious consideration, setting forth perhaps for the first time, however imperfectly, a set of principles that might govern practice in the field of historical archaeology. . . . I think Clyde Dollar is to be commended for coming up with some incisive observations about the limitations of this alleged craft, and a tentative set of principles. The fact that there are deficiencies and debatable points does not weaken the merit of this paper as a thoughtful challenge. (Mattes)

Dollar has shown a lot of perception in singling out some of the major problems that relate to the ordering of archaeological field data into temporally and culturally significant units. But he has treated the problems as though they had not been recognized previously by anyone. (Jelks)

Mr. Dollar's presentation is on occasion somewhat less than clear. Even his definitions are sometime incomprehensible. . . . Mr. Dollar's restrictive definition and outlook on historic sites has led him seriously astray in other ways. One of these points is his fascinations with architectural remains. . . . perhaps my most serious quarrel with Mr. Dollar is that he erects a false antithesis between the methodology of history and archaeology. . . . Dollar creates the impression that history is much more accurate than it really is. (Armour)

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. . .I feel that the paper makes a contribution in outlining some of the archaeological difficulties one encounters on the historic time horizon, and hopefully it will generate some point-by-point answers by practioners in the field. (Williams)

I do not feel that this paper is worthy of criticism or publication, revealing as it does that a little knowledge is a dangerous thing. (Demmy)

It seems to this writer that Dollar's approach is narrow and national-history oriented, seeing no value in the archaeological study of a site other than to supplement the historical record for preservational purposes. He is blinded to the anthropologists' diametrically opposed non-ethnocentric orientation and his attempts to extract more information from sites and artifacts. (Foley)

As it stands, however, the paper is an interesting statement of ideas, many that are basic to historical archaeology, and would be little disputed by historical archaeologists, regardless of their backgrounds' others however, are as the author has said "Some Thoughts," and these will stimulate other thoughts from colleagues. (South)

It would appear that the one single point of agreement among us is that we disagree, and not the least of the results of this dialogue will be the discovery on the part of the critique writers just how much they individually disagree with each other—as well as with the "Thoughts" paper. No doubt this will come as a shock to some. I personally feel that this very disagreement is indicative of a healthy, robust, and inquisitive outlook within our profession. I am deeply grateful to all these writers for the time they spent in putting their thoughts down on paper. In doing this, not only did they bring a tremendous amount of experience and knowledge to bear on the multifaceted problems of theory and method, but they also exhibited a certain amount of bravery in doing this so that others could witness and judge their contribution. This, put simply, amounts to a strong leadership. In particular, I would like to express my thanks to Stanley South for his work in putting this dialogue together. I regret that the press of my primary duties with the Rusebud Sioux Tribe has prevented me from finishing this dialogue sooner, and I wish to also thank Stan,

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and my colleagues, for their collective patience while these replies were being completed.

Ours is not a profession dealing with data of mathematical precision, and I do not think that we should ever restrict our research, which is inherently creative in nature, by formulating any but (at this point in time) very general theories and concepts. However, the various critique writers, by their very divergence of opinions and viewpoints, have at least exhibited some proof of a need to create a more cohesive theoretical approach to historical archaeology, if for no other reason but that this procedure would tend to strengthen the credibility and acceptance of our research to other branches of scientific study—and colleagues in other geographical areas. I would therefore, suggest that we think in terms of calling a conference on theory and method in historical archaeology to be held at a midwestern university or college sometime during the early part of 1970. Selected professional researchers might be invited to formulate basic definitions, present papers on topics relative to theory and method in historical archaeology, and then, as a group, attempt to arrive at certain general concepts applicable to our profession. It would be naive, perhaps even undesirable, to expect any but a general consensus to come from such a conference; however, even a general consensus arrived at as a result of a meeting of minds in conference would be better than the disorganized and somewhat chaotic orientation our profession now presents to the academic world.

The 'frontiers of the mind' are always stormy, and sometimes the more severe criticisms are the most useful. In allowing the "Thoughts" paper to be critically analyzed by my colleagues, I knew only too well that criticism would be far from lacking. My own inadequacies, as exhibited in the writing of this paper have been rather adequately discussed, and I submit to my colleagues that when the 'last' word on theory and method in historical archaeology has been written, I will not have been the one to write it. In the foregoing pages of

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critiques and replies, much has been said about the contribution historical archaeology has made, could make, or will make, to the understanding of man's various cultural expression, and with much of this I have heartily agreed. However, it is in the descending from this 'over-view' of Culture and History to the particular plane of determining definitive facts and data that we run afoul of considerable disagreement. The anthropological and/or cultural contributions notwithstanding, I still maintain that historical archaeology is basically historical research being done using the specific techniques of the spade and trowel as well as the time-evolved methods and criteria of historical research. Anything less than this is not historical archaeology, no matter by whom it is done. In short, gentlemen, the name of the game is History, and if you have not played it according to its rules, then you have played in vain.