"THE BASIC NEEDS OF AMERICAN ARCHAEOLOGY"—
A COMMENTARY

In *Science* for December 8, 1939 there appeared a timely article by Dr Carl E. Guthe on "The Basic Needs of American Archaeology." This article followed upon the deliberations of a committee in the Division of Anthropology and Psychology of the National Research Council. Apparently Dr Guthe's article is a consensus of these deliberations.

In substance, the "basic needs" expressed by Dr Guthe may be summarized about as follows:

1) Field operations should be conducted by trained field men with academic as well as field experience; this should insure the keeping of accurate records, and include "clear comprehension of the problem under attack."

2) Supplies such as surveying instruments, cameras, tools, and storage and transportation facilities must be available. The staff must be given time to experiment, cross-check records, and to do other such work.

3) Laboratory units should be conducted by trained technicians and pertinent allied sciences be brought to bear in order to insure the most complete and accurate analyses of material.

4) Accurate mapping of sites and environment, of vertical and horizontal records, and planned site nomenclature, are indispensable.

5) Methods of excavation "must be conducted in a manner which promises to disclose all available evidence on formation and structure, and to preserve all data on stratigraphy, age, time succession, cultural and historical activities and cultural processes."

6) "All of the foregoing activities are nullified unless followed by prompt publication."

Certainly every archaeologist will agree that all these are basic needs and indispensable to the serious attack of any problem in American prehistory, and that Dr Guthe has stated them clearly.

However, upon reading the article I was struck by the fact that there is another basic need for which there is not as yet any satisfactory solution. This is suggested by Dr Guthe in the sentence about the "satisfactory applications of laboratory methods . . . to facilitate definition of types, cultural synthesis, and broad cultural comparisons." It may be of moment to elaborate upon this point.

Is it possible for American archaeology ever to solve any broad problems in "time succession, cultural and historical activities and cultural processes" until it develops a common trait terminology, a common language concerning artifact types and the criteria by which they may be determined? Can cultural synthesis and broad cultural comparisons be achieved unless there is a general system by which the many workers in this field can be sure whether or not they are dealing with similar or dissimilar culture traits? Considering the time, energy, and money continually expended in excavating, in gathering greater and greater masses of specimens and more field data of every sort, it is a source of wonder that so little attention has been
given to the comprehensive determination of (1) types, traits, elements, or whatever the recoverable culture units should be called, (2) their precise distribution in space, and (3) their relative distribution in time over broad areas. I believe it is virtually impossible for any excavator to interpret correctly the true significance of his finds until he has some means of knowing whether his particular specimen types, once defined, are: individual or special (confined to a single site or part of a site); local (a few sites or a small geographic area); areal (widespread enough to be identified with a "culture area" or perhaps extending over several states); more or less general, perhaps essentially continental. In the last case it may be more important to know where a trait does not occur than where it does.

In the same vein, it seems to me that an accurate knowledge of distribution is at least as important in historical reconstruction as any stratigraphic situation or "geological" dating of cultural deposits. A case in point is the channeled "Folsom" artifact. Many archaeologists find it somewhat difficult to swallow the antiquity pill on this readily recognized object, despite its occasional faunal associations, because of its extensive surface distribution outside the High Plains. Again there is the example of the corner-tang knife of central Texas. This was believed to be distinctive of, and confined to, this one area until Professor Patterson\(^1\) sought information on it from surrounding states. Before long, unquestionable occurrences were reported from nearly all the Plains states and even central Canada.

Above all, we need to know what constitutes a type, in order to discover its derivative, a culture trait. Further, we need to know how to distinguish a type, which is a fairly definite and stable combination of particular features, from a variation in detail within a type. In practice, there is either a tendency to describe in too general terms (e.g., knives, scrapers, projectile points, coiled basketry, incised pottery, et cetera), or to plunge into lengthy description of every small detail of form and size. While such details are naturally necessary for comparative purposes, there is as yet no generally accepted means for determining where variations end and new types begin. This is, of course, no easy problem, but it is a basic need and must be dealt with if we are ever to devise acceptable terminology. Anyone who has mulled over a few thousand or even a few hundred specimens can readily appreciate the difficulties of such determination on a broad scale, with the accompanying dilemmas of devising simple yet graphic terms.

It is true almost anyone can devise some sort of classificatory system for particular collections, but to devise a system that will enable two or more persons to

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\(^1\) J. T. Patterson, *The Corner-tang Flint Artifacts of Texas* (The University of Texas Anthropological Papers, Vol. 1, No. 4, 1936), pp. 1–54. This was followed by *Supplementary Notes on the Corner-tang Artifact* (same series, Vol. 1, No. 5, 1937), pp. 32–39, giving a greatly extended distribution. Between Patterson's two papers Hans E. Fischel wrote *A Note on, Corner-tang Artifacts*, (American Antiquity, Vol. IV, 1938), pp. 152–154, but since this appeared after Patterson's second paper, Fischel followed with a second short note (American Antiquity, Vol. V, 1939), pp. 276–277. One wonders how much further the distributional and historical aspects of this one type would change if research on it were pushed to completion.
sort even the same collection in the same way is often extremely difficult. There are always a number of overlappings and border-line cases in which one person decides the specimen belongs in one type, another person in another type. These cases are often important in testing the validity of the typology itself.

Under past and present conditions a conscientious archaeologist, to see his excavated materials in their true perspective, must go into exhaustive research, study all the literature and illustrations available which may bear on his problem. While from one viewpoint such study is very desirable for background, it also happens that with the steadily increasing masses of available data, most archaeologists find that lack of time limits the thoroughness of their outside research. The result of this is that one is more than likely to devote whatever time can be given to distributitional study of a few traits in which he is especially interested. I have yet to meet anyone who has been able to present complete distributitional data on the traits found in a single site, yet how can true perspective be attained by anything else?

Pleas have been made before this for type determination, classificatory systems by name or symbol, even for agreement on the very simplest of archaeological terms.² While some of these ideas appear quite adaptable, the profession as a whole has made little progress. This may be adduced to lack of attention of American archaeologists to the activities of any particular group or institution. A powerful and stubborn provincialism prevails in terminology that militates against general acceptance of any particular system, perhaps because most archaeologists are preoccupied with the details of certain areas or states, not with broad and ultimate distributions.

To answer these further basic needs, I suggest the following:

A board should be established of, say, ten persons thoroughly familiar with the archaeological materials and literature of as many different sections of the continent. They should be prepared to spend not less than five years at the task, and should be given facilities to work together as a unit in one of our archaeological institutions. Their work would consist mainly of two kinds of research. At least two years would be necessary to determine types and traits, list them and the criteria of determination, and devise terminological schemes. This done, the enormous task of plotting distributions could proceed, making use of whatever unpublished data is available, as well as the published.

Such a board would find and make use of the spirit of cooperation that exists in most archaeologists. This spirit has so far been too diffuse to solve the problems here mentioned. If and when a central body is established, upon which attention can be focused, we can begin to effect the synthesis which American archaeology so sorely needs. Once the ball is rolling, there is little doubt that excavators in all parts of the

country will readily give information to the board on whether such and such traits occur in their areas, and under what conditions of space, frequency, and succession.

Trait determination should include more than artifacts. Features of house construction come to mind, as do grave types, burial orientation, and the placement of property or gifts with a body. It is highly important that the infinite details of pottery form, paste, temper, modeling, painting, incising, et cetera, be reduced to a digestible list of types and expectable variations. Painstaking detail is a necessary function of local research but to see our historical problems in a broader light some way must be found to wade through endless variation in detail and size. While the judgment of any individual will never be accepted by other workers in the field, it is not at all unlikely that a representative board can accomplish results worthy enough to draw the attention of the entire profession.

It may always be argued that synthesis is impossible or premature until we have more field materials, or have heard from some of the lesser known areas. This has always seemed a particularly hollow objection to me, since there are such enormous numbers of specimens already on hand in one institution or another. Added quantities will avail us little until we devise some methods of treatment for what we have. Any maps based on present collections can always be added to, if and when new discoveries extend a particular distribution or create a new type.

These suggestions are made in supplement of Dr Guthe’s recent article. I believe that the questions raised cannot be answered by a convention meeting for a few days, but that they must be tackled in a serious and persistent manner in the way suggested. Any comment will be welcomed.

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NOTES ON AN OPELOUSAS MANUSCRIPT OF 1862

At Opelousas, in September, 1862, after a residence of approximately twelve years in Southwest Louisiana, an anonymous writer completed a monograph entitled *Essai sur Quelques Usages et sur l’Idiome des Indiens de la Basse Louisiane*. The original manuscript has not been found; nor can an exact date now be assigned to the typewritten copy, which may be seen at the Howard Memorial Library in New Orleans.

The manuscript contains a brief *Preface* dealing with the author’s life in Louisiana, an *Introduction* setting forth the general plan of the study, and eight chapters on the grammar and vocabulary of the Choctaw language. The author comments on various phases of the vocabulary, which he distributes under such headings as animals, trees, plants, maladies, numerals, and geographic names. He adds two lengthy glossaries, the one French-Indian and the other Indian-French.

The language that the author presents is virtually pure Choctaw, very few words being drawn from other Indian sources. Indeed, I have noted only Mobilian *Ino* for Choctaw *āno*, “I,” and Chickasaw *fouché*—that is, *fushi* or *foshi*—for Choctaw *hushi*, “bird.” His orthography is of course typically French,—so much so that some