

## THE METHODS OF PERUVIAN ARCHAEOLOGY

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Editor's introduction. This paper is the original English text of an address which Alfred Louis Kroeber delivered in Spanish to the Faculty of Letters of the University of San Marcos in Lima on April 24, 1942, when the Faculty bestowed on him an honorary doctorate. The Spanish translation was published in the journal Letras, no. 22 (segundo cuatrimestre de 1942), pp. 205-226, Lima, under the title "Los metodos de la arqueología peruana." The English text is published here for the first time with the kind permission of Mrs. A. L. Kroeber.

"The methods of Peruvian archaeology" is Kroeber's only general statement of his views on archaeological method and theory, and it provides a valuable background to his numerous substantive studies. The date when the paper was written should be kept in mind, however. It falls somewhere near the middle of the period in which Kroeber worked on Peruvian archaeology, and it reflects neither his earliest nor his latest thought on the problems of this field. Kroeber continued to think about archaeological problems up to the time of his death in 1960 and to follow the work of others in this field. No doubt he would have modified the views expressed in this lecture very considerably if he had revised it toward the end of his life.

In this paper Kroeber discusses the significance of differential associations, stratigraphy, seriation, and the advantages of studying small sites to establish units of contemporaneity before attempting to sort out the sequence of occupation at large sites. The first and last topics have never been better discussed, but Kroeber's comments on stratigraphy and seriation have less enduring value.

His discussion of stratigraphy is weakened by his acceptance of the assumption that absolute depth somehow reflects relative age, a surprisingly common illusion among archaeologists which has been ably and vigorously protested by R. E. M. Wheeler (Archaeology from the earth, fig. 11, p. 54; Clarendon Press, Oxford, 1954). On the other hand, Kroeber was the first Peruvianist to recognize the importance of fill as an element in the stratification of habitation sites.

Kroeber illustrates his principles of seriation by discussing certain details of Nasca style; he looks for logical principles from which a sequence of style changes can be inferred. This approach is vitiated by the fact that there are equally logical alternatives to each of the principles he lists. I have commented on this problem elsewhere ("Nuevos datos relativos a la cronología del estilo Nasca," Antiguo Perú; espacio y tiempo, pp. 29-45; Librería-Editorial Juan Mejía Baca, Lima, 1960, and "Alfred Louis Kroeber, 1876-1960," American Antiquity, vol. 27, no. 3, January, 1962, p. 406, Salt Lake City). - J. H. Rowe.

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The privilege of addressing this assembly is both an honor and a pleasure. It is an honor to speak in a hall of the oldest University in the New World--older by eighty years than any in my own country. It is a pleasure to express what was noticeable from the first days of my arrival: the enormous progress which has been achieved in the development of Peruvian pre-history and archaeology since my last visit to this country. An active group of Peruvian archaeologists--Drs. Valcárcel, Tello, and their assistants; Sr. Larco Hoyle operating as a free-lance; among clerics, Villar Córdoba and Bernedo Málaga; and too many others to name individually--these have assembled masses of new data on the antiquities of all parts of the country. Entire new civilizations of the past have been discovered and their monuments gathered and described--cultures not only pre-conquest in period, but pre-Incaic in almost every instance. The recently determined cultures of Pucará, Casma, Nepeña, Cupisnique, Cajamarca, the Marañon have been added to the record of those which were novel only fifteen or twenty years ago, such as Chavín and Paracas. All this addition to our knowledge of the intricate ancient past of the native race has been achieved by Peruvian scholars. There truly is an infinity for the visitor to learn.

One consequence has been the attraction not only of tourists, but of students and investigators from abroad. It may be illuminating to compare the conditions of today with those which existed when I first arrived in Peru in 1925. At that time, seventeen years ago, there was not one North American or European archaeologist on Peruvian soil. Max Uhle, the true founder of scientific archaeology in the Andean region, had left Lima for Chile and Ecuador; it is a pleasure to recall that he is again here, enjoying a ripe old age as an honored guest. Even before him, Adolf Bandelier, the Swiss, had returned to the United States. Some years later, my fellow countryman, Philip Ainsworth Means, had spent some time in Peru; but he too had returned. In that period, we archaeologists arrived here from abroad intermittently--one or two in a decade. By contrast, this one last year has brought five or six of my compatriots to your hospitable shores, to participate in the current investigations and excavations. Were it not for the World War, this number would, no doubt, have been increased by the presence of Europeans as well.

It may be of some slight interest to mention how I came to be enrolled in this growing little army of Peruvianists. Born in New York and trained there as an anthropologist, it was almost inevitable that my first interests should concern themselves with the native race and culture of North America. My apprenticeship was devoted to the Eskimo; my first investigation in the field, to the Indians of the Mississippi Valley. Fate then carried me permanently to California--full of remnants of primitive tribes of the most diverse speech, and interesting perhaps chiefly for the very backwardness of their customs. They had been neglected by students of anthropology; so for years it became my first duty to preserve for posterity all possible ethnographic information which it was still possible to collect from these Indians. Gradually, however, it became increasingly clear that these tribes of California and the United States formed only a chapter in a book, as it were, a fragment of the story of the development of the race aboriginal in the Americas. Mexico and Peru were the regions where this race, in the many centuries of the pre-Colombian isolation, had unfolded

its attainments of civilization. The north and the south of the double continent were only peripheries, to which broken influences from the achievements of Mexico and Peru had penetrated in dilute form. The fountain head of indigenous American history lay in these higher centers. It was only through inclusion of these fountain heads in the field of active investigation, that interpretations could become fully significant and integral. I turned to the collections which Max Uhle had formed in Peru for the University of California, and which happened to be in my charge; and I analyzed them as intensively as possible. Thereupon the wish was inevitable to see the country as well as samples of its remains, and to participate actively in the prosecution of its archaeology. Thus I first came to Peru; and the thrill of contact experienced then is the greater now when there is so much more to be learned.

Basically, it is clear that the indigenous culture of Peru, and of the adjacent Cordilleran regions both to the south and north, is one: it is a single larger development, perhaps wholly autochthonous, certainly largely so. Also it is clear that, through common origins or through inter-influencings whose exact course cannot yet be traced, this Andean culture and that of Guatemala and Southern Mexico, possess more distant relationships. The agriculture, metallurgy, pottery and other industrial arts, the architecture, and the religious ideas and cults are at least similar. As examples, it may suffice to cite the same maize as fundamental to subsistence in Mexico and Peru; the same casting of gold and silver, the same pyramidal structures, the same human sacrifice. One might add also the same strange absences: iron, the plow, the wheel in every form, all musical instruments with strings, were alike unknown in ancient Mexico and Peru, as well as everywhere in the pre-Colombian Americas.

However, this great integral civilization of Peru, with its outliers in Chile, Argentina, Bolivia, Ecuador, and perhaps Colombia, early developed many provincial phases. With the passing of the centuries, these sometimes amalgamated, sometimes diversified still farther, until the total picture of events, of the cultural unfolding, became a very complicated one. This intricate composition of Andean history for perhaps 2,000 years back, is the task of archaeology to unfold, first by analysis, then by resynthesis. The analysis must be exact and evidential; the synthesis authentic and evidential. The analytic method uses the mental microscope, so to speak; the synthesis, the mental telescope. But both must be exact: the analysis in its observations and discriminations, the synthesis in the judgments and appraisals. Also, both must content themselves with being progressive, and, therefore, partial in their results. However much we understand today, the next generation, that of our disciples, will know more and, therefore, understand it better. The wisest answer we can give to most of our problems are tentative conclusions, not final ones.

In this task of unraveling the past into a comprehensible story, archaeology and history, of course, go hand in hand. Their purpose is identical: the understanding of the major currents of human development as they have actually occurred. The difference is only in material and, therefore, in the techniques employed. History outlines primarily words formerly written down in documents; archaeology the tangible objects physically surviving from the past. The help which each discipline can give the other is so

evident as to need no explanation. The limits of this mutual aid are two-fold. On the one hand, archaeology is most successful in ascertaining general conditions within a period and an area, and therefore only imperfectly satisfies the desire of the historian for knowledge of crucial, particular events. On the other hand, in civilizations like the Andean one, which developed without writing, the oral record of memory is too impermanent to allow the historian to penetrate as far back into the past as the archaeologist wishes to penetrate; so that for the earliest periods, he must of necessity pursue his investigations alone; just as for the later periods of written documentation, the historian scarcely needs the help of the archaeologist.

In ancient Egypt, whose culture was a literate one, the excavating of inscriptions by archaeologists has provided the historians with a history, in the full sense of the word, stretching back 5,000 years. We know the names of the kings, their dates and years of rule; their capitals, provinces, victories, and reforms. Ancient Peru, by contrast, being a culture ignorant of writing, though in many other respects not less high than that of Egypt, was able to provide the first Spanish chroniclers only with orally transmitted memories, often conflicting and confused, rarely wholly concordant, and in no case, probably, possessing genuine authenticity beyond 500 years before Pizarro, if that much. For the Inca period, the historical and archaeological data supplement each other beautifully. Garcilaso de la Vega and Machu Picchu are documents of equal value. But farther back in time--what have the chronicles to offer us? Some mentions of Tiahuanaco, vague, and unplaced in time; and references to Gran Chimu, in which the earlier Mochica and later Chimu are blended into one undistinguishable assimilation, although the periods were perhaps as distinct culturally as were Greece and Rome. Of the often superb manifestations of the cultures of Chavin, Cupisnique, Nazca there is no longer a trace of mention in the legends or traditions available to the Spanish and Inca chroniclers. Even the less great but nevertheless distinctive local cultures of Chancay and of Ica-Chincha, which flourished into Inca times, and were actually seen by the followers of Pizarro, find, so far as I can recall, no mention whatever in the chronicles.

It is clear, accordingly, that beyond say about 1300 A.D. the history and the archaeology of Peru are like ships sailing in the same direction but so far apart that they can communicate only imperfectly; and beyond 1000 A.D., the ship of archaeology has become isolated and must pursue its course alone, as best it may.

The problem of Peruvian archaeology accordingly is to trace as far back as possible the historical development of man and his cultural manifestation in the Andean region. The method is that of archaeology everywhere, with such minor adaptations as may be imposed by the data characteristic of the area. Fundamentally, there seem to be two requirements in all archaeological method, and these two are related. The first requirement is to determine which of the phenomena occur associated, or not associated, and in what degree. The second requirement is to translate the space relations of the data into time relations, so that a descriptive picture may be converted into a sequential story.

The matter of associations is not only fundamental in archaeological method, but so simple as sometimes to be taken implicitly, or even overlooked.

It means that objects or qualities which occur together in the ground, must have coexisted, not only in space, but in time. Here then we have an irrefutable, objective, positive datum of science. Contrariwise, if two classes of objects, or features of style, or other phenomena of the past, both occur repeatedly, but never in association, their very dissociation is also an objective, scientific fact, although a negative one. At times the situation is less regular, in that phenomena A and B may occur now separately, now in association; or A may associate with C, and B with C, but never A directly with B alone. In such a case, we are manifestly confronted with a partial correlation. A and B are manifestations mainly distinct in their geography or history, but also contiguous or overlapping; or, they both overlap with C.

The associations, and dissociations, attain their full reliability only when they are determined with sufficient fineness. A given site may have been inhabited continually through two or more periods, say the last pre-Inca epoch and the Inca. In that case, pre-Inca objects may seem to come associated with Inca ones, as long as the site is treated as an indivisible unit. But as soon as discrimination is made between portions of the ruin, and almost infallibly as soon as discrimination is made between its separate graves, the associations ought to eventuate as authentic, certain of the tombs proving to be pure Inca in their contents, others pure pre-Inca. All this is obvious enough, and any competent excavator would observe the distinction. Yet it is also important that he not only observe the distinctive associations, but that he record and publish them, else the rest of the world is necessarily left in doubt whether the asserted distinction is merely subjective or is verifiable from accessible information on the evidence. If a foreigner may be permitted to express himself frankly, the one criticism of the procedure of Peruvian archaeologists which is occasionally to be heard in Europe and the United States, is not in regard to their conclusions, but that the basic, descriptive information, detailed site by site and tomb by tomb, on which these conclusions rest, is too often not made accessible to the world of science at large by publication. It is true that a catalogue or inventory of mere facts never makes interesting reading; but such an itemized invoice is as necessary for other scientists to form their own independent judgment, as the account books of a business are necessary for both auditor and owners.

It is, of course, also possible to err on the opposite side, by presenting a mere factual record without interpretations; or to make discriminations unnecessarily fine. I plead guilty to the latter error in some of my earlier descriptive writings on Peruvian archaeology, such as those on the Uhle collections from Chíncha and Ica. Dr. Strong and I, for instance, at first recognized five periods: Middle Ica I, Middle Ica II, Late Ica I, Late Ica II, Inca. I still believe that these five aspects or associations represent actual distinctions; but I am ready to admit that the distinctions refer only to quite transient phases or minor differentiations. From any broader, comparative point of view, the material in question is probably comprised in two significant periods only: first, what Dr. Strong and I called or miscalled Middle Ica, which is wholly free of Cuzco-Inca associations and, therefore, pre-Incaic; and second, Late Ica, which has Cuzco-Inca associations in variable degree, and is, therefore, Incaic in general time.

However, I do not believe that our hyper-discrimination has had any seriously unfortunate influence on the progress of Peruvian archaeology, because it is very easy to consolidate the five phases into the actual two periods; whereas on the contrary, it is always nearly impossible subsequently to segregate out data which have been presented mingled or confounded. For instance, all of the collections of Uhle dealt with by us in this case are relatively late, in the sense that they are undoubtedly post-Tiahuanaco. Suppose that Dr. Strong and I had accordingly thrown all these tombs together into one generalization which we called merely "Late." In that event, the genuinely valid distinction, though perhaps not of supreme importance, between the pre-Incaic and Incaic sub-periods within the general Late era, would have been lost. I hold it to be the archaeologist's duty to his profession to present his discoveries, with their detailed associations, so fully to his fellow archaeologists, that these can form their own interpretations, or reinterpretations, if they wish. With the full facts of association on public record, there will gradually come about an approximation to consensus of interpretation. Without the full record, conclusions are likely to remain mere opinions, as numerous as there are archaeologists, and none really substantiable.

So much for associations. We consider next the conversion of space into time.

The task of translating the relations in space of the discoveries made, into relations of time--of construing distributions into historical sequences--is most difficult when the distributions are horizontal, most sure when they are vertical. A vertical distribution has come to be known as a stratification, by the borrowing of a geological concept and term. In both geology and archaeology, stratifications have almost final value. They have at any rate the greatest possible value as determinants of actual sequences, as compared with hypothetical or speculative sequence. This recognition of the evidential value of stratification, however, leads to a danger of abuse of the method. This danger consists in the premature or deceptive recognition of stratifications which do not actually exist; or in their recognition as simple when actually the depositions in the ground may be much more intricate. In short, stratifications are so obviously desirable to encounter, that eagerness to find them may lead to their being reported without sufficient basis of fact. We may speak in such cases of mental stratifications as compared to actual ones in the ground. Or, we may word the distinction as being between conceptual stratifications, which are unproven possibilities, and evidential stratifications, which constitute proof. In the United States, we have come to recognize that the majority of stratifications reported by amateurs are of this hasty type of wish-fulfillments; and that they are always in need of verification by the spade and by critical observation of trained archaeologists. The scientist may have formed the working hypothesis that the time sequence of three types was L, M, N, and will, therefore, also be gratified if the superimposition in the ground shows the same order. But, having been taught to observe critically, he will, if his observations demand it, withdraw his working hypothesis in favor of another order, such as M, N, L; or, as most often happens, he will decide that the observed facts, taken in their totality, are insufficient, or too contradictory, to permit the sure establishment of any sequence; at least not at the particular site being investigated.

The most certain instances of valid stratifications are recognized to be those which result from depositions which we may call natural or accidental. That is, they are unintentional. Most frequently, such stratifications are the product of the throwing away of waste, of *basura*, by a settled population, gradually accumulating generation after generation. As the culture changed, the layers of accumulation changed. Such deposits of *basura* ordinarily yield many broken pieces and fragments that had become useless. The complete and beautiful specimens to be encountered in graves or intentional deposits can ordinarily not be found in stratified *basurales*. The excavation of such sites, therefore, requires a certain renunciation. It will be the work of the scientist as compared with the *aficionado*. The reward of the abnegation, however, is that the scientist may succeed in proving the actual sequence of the types of which notable examples have been assembled by the collector or *aficionado*.

Away from deposits of *basura*, reliable stratifications are much more difficult to establish. A later grave may have been sunk into the ground next to an older one, but to a greater depth, or an old tomb may have been re-used at a later time. A late wall may have had its foundation carried deeper than an adjacent ancient one, or may have re-utilized parts of the ancient material. If *basura* deposits cannot be found, the archaeologist may have to have recourse to stratifications of interments and structures; but unless the evidence of these is uniform and overwhelming, it is best regarded as merely provisional.

In a country like Peru, there is the added difficulty that the pre-Colombian natives were addicted to the habit of rearing massive and voluminous structures, sometimes wholly of adobes or worked stone, but at other times consisting of walls of adobe or stone containing fill of earth. This filling of earth, in turn, may have occasionally been taken from more ancient *basurales* that happened to be conveniently near the subsequent constructions; thus causing an apparently contradictory collocation.

A parallel example may be cited from the archaeology of the United States. This incident occurred after the succession of cultural periods of the prehistoric Pueblo Indians of Arizona and New Mexico had been pretty accurately established by the cooperative labors of a whole series of archaeologists, and was being confirmed by actual dates (*fechas*) secured from examination of tree rings. A large rubbish heap in the northwestern part of New Mexico was being carefully trenched, and removed in layers (*capas*); when it became apparent that at this site the objects of the latest type or period III were at the bottom, those of type II next, and those of the early type I at the top of the mound of *basura*!

In the middle of the mound of *basura*, however, was a depression; and this depression finally provided the key to the apparently insoluble contradiction of the site to all others. The population of the ruin was a rather large one, and had continued to inhabit the pueblo for several centuries. Most of the rubbish was deposited in one spot just outside the pueblo walls; and it was deposited first in period I, then II, then III, which lay on top. Toward the end of period III, however, it was decided to construct a new and large kiva, as the underground temples or places of

worship of this culture are called. As the site of this new kiva, the basural was chosen. In the excavation for this subterranean structure, the constructors of late period III of course first removed the top soil, which had accumulated during period III, and threw it outside. Digging deeper, they encountered the basura of period II; and throwing this too outside their structure, it fell on top of the later soil of III which they had already thrown out. Finally, they came to the layer of the earliest period I, disposed of it in the same way, and so it came to lie on the surface! When still later the excavated kiva was abandoned, and crumbled with the centuries, the basural as a whole had taken on the appearance of a reverse stratification, which required most minute examination to explain.

In a country like Peru, where the ancients had almost a passion for building, rebuilding, and moving large masses of material, possibilities like this incident must, of course, be guarded against with special care. Also, to be reliable, a stratificatory exploration should remove a fairly considerable volume of soil, which requires patience, time, labor, and funds; and all this without prospect of reward of attractive or beautiful objects. These circumstances explain why, as I pointed out fifteen years ago, discoveries of legitimate stratifications of consequence have been few in Peru. However, they do remain the final evidence for the unraveling of prehistoric sequences; and in future, more and more excavations specifically directed at stratifications will undoubtedly be undertaken, and will prove as successful and significant as in other parts of the world.

However, the archaeologist cannot suspend all operations until such costly stratification investigations have been undertaken. He has at his control a vast mass of discovered material and information on the prehistory of Peru, which it is his desire, we might say his duty, to explain as best he may--provisionally in terms of probability if not of demonstrated certainty. How shall he proceed?

One method is that of stylistic relations. This method, by itself, can not attain to absolute proof, because style inevitably contains an aesthetic and therefore subjective factor; but it can hope to attain to reasonable probability. I should like to cite one or two minute but concrete examples.

In the paintings on the pottery of Nazca, stripes or rays are often shown proceeding from a face or figure. These rays take two forms: either simple, or with the end knobbed or rounded, by having one of the border lines of the stripe folded over on the other. The two forms of ray do not occur together on the same vessel; and their associations are different. On vessels with two spouts (picos), the simple rays are painted on heart-shaped jars, the folded rays on cylindrical or flattened jars. However, the simple rays proceed from animals or monsters with a single face, the folded rays often from beings with the face repeated two or three times. Each association of details is consistent; they do not mix, or ordinarily occur on the same pot. We must, therefore, conclude that they are expressions of two substyles within the general Nazca style of pottery. Presumably, therefore, they differ also in time within the general period of the Nazca culture. Which of the two treatments or manners is the earlier? I



explored for three months in Nazca, in 1926, part of the time in collaboration with Dr. Tello, and searched for stratifications, but without succeeding in finding a case of the superimposition of one of these Nazca substyles on the other. It is therefore necessary to fall back on the indirect evidence of stylistic qualities.

Now the folded-over ray is by no means a complex figure of design, but it is a little more complex than the simple ray. One can understand it as a modification or slight elaboration of this; but the folding-over at the end of a projecting ray is difficult to conceive as an original form. Similarly, a human or animal body with a series of two or three faces is hardly a natural original inspiration. It suggests a repetitive, decorative expansion of design, derived from a single-faced body. Similarly again, the variably flattened or cylindrical jars on which the folded rays are painted, indicate experimental seeking after new forms developing out of a more original standard heart-shaped form. We may conclude accordingly, on grounds of the logic of normal stylistic development, that within the period of the Nazca culture there were two phases, the earlier characterized by simple rays and a set of associated features, the other by folded rays and another series of associated stylistic features. In order to avoid unnecessary or distracting implications, I designate these two sub-periods as Nazca A and Nazca B. I also admit that full proof is lacking for the temporal priority of A over B; there exists only a reasonable probability of the priority. If an actual contrary stratification were discovered, or sufficient contrary evidence of stylistic associations were assembled, I should, in intellectual integrity, have to abandon the working hypothesis that A was earlier than B.

Similar reasoning can be applied to another element of design: the signo escalonado, so widely spread in Peru, both in its simple form and in combination with the fret (greca). In the Nazca ceramics, this signo escalonado invariably has its normal form on vessels painted with the simple ray or other elements of the substyle A. If, however, the other characteristics of a vessel point to substyle B, the signo escalonado is sometimes varied by having the lines in the front of each step project beyond the level of the step. Now no one first representing the symbol of a terrace or stairway, would presumably think of thus carrying the vertical lines into the interior of the figure, where they are meaningless. The extension is evidently the result of a stylistic impulse toward novelty, variation; or perhaps hasty execution, which could scarcely arise until the regular escalonado was well-established as a standard design. Again therefore, the logic of style indicates one form as earlier, the other as later, in all probability. When in addition, the regular escalonado is the only one found in association with the simple ray, but the extended-line escalonado, whenever it occurs, is associated on the same vessels with one or more form-elements of the folded-ray complex, the two stylistic inductions obviously reinforce each other.

Sometimes such inferences even carry across cultures, and between them. It has long been noted that the famous relief of the Raimondi stone shows not only multiple faces but the folded-over rays of Nazca B ceramic designs. This is remarkable in view of the distance separating Chavin of the northern interior from Nazca of the southern coast. It would be too much to insist on exact contemporaneity, but there was without doubt an

interinfluence, which in turn presupposes a closeness in time. Whether the Raimondi stone art of Chavin has influenced Nazca B ceramics, or whether conversely Nazca has influenced Chavin, I would not be prepared to say, because such evidence can often be read or construed two ways, until it finally becomes cumulatively one-sided. However, the resemblance and connection would mean that Nazca A was presumably earlier than the Raimondi stone art, and at any rate independent of it, if our previous reasoning is sound that Nazca A antedates B on stylistic grounds.

Does this mean that Nazca in general is earlier than Chavin? By no means. The Chavin sculptural art in general is executed in a different manner from the Raimondi stela. It is monumental, it is massive, it deals with other themes, it lacks multiplied heads, it lacks folded-over rays. In short, the Raimondi stone, though found at Chavin, is unique; it does not really belong to the proper style of Chavin, which I long ago designated arbitrarily as Chavin M, but the Raimondi stone as Chavin N, the letter N designating its Nazca resemblances. As between the Chavin culture as a whole and the Nazca culture as a whole, the question of priority in time, therefore, is still open and unsolved. At any rate, it can be answered only on the basis of other evidence, and much fuller evidence than folded rays or repeated faces. All we can affirm is that some Nazca culture of the form A, seems to be anterior to some Chavin culture of the exceptional Raimondi type.

I hope that this excursion into minutiae will be forgiven. I have cited the details because they illustrate the principle that sometimes, by narrowing the focus of attention to small elements which in themselves are trivial, we can attain to a sharpness of comparison which frees interpretation from ambiguity and serious doubt. Such interpretations, however, limited and special, may then serve as clues which can lead to wider interpretations and generalities. In science, no piece of evidence is too minute to be brushed aside, provided it is relevant and its authenticity is confirmable.

In the same spirit, I should like to enter a plea for the frequently significant value of what may be called the small site of pure style; namely the ruins or rubbish or cemetery left by a small population occupying a given site for a relatively short period only. Such remains are likely to be stylistically pure. The material obtained from them can therefore be used as a touchstone to segregate out the phases occurring within the material obtained from larger sites, whose population may have been ethnically mixed, or may have had wide relations of commerce, or may have persisted through several stages of changing culture. It is the large site--like Pachacamac--which has generally left the monumental ruins, and rich cemeteries, the splendid collections of objects. But its history is too complicated for easy or sure understanding. The large site calls for explanation; the small, pure site may help to give the explanation. Perhaps you will pardon one more example.

Forty years ago, Max Uhle defined, from his excavations at Chincha and Ica, two closely related cultures, or two local variants of one culture, which we may call the Chincha-Ica civilization. Going further downstream along the Rio Ica to the oasis of Ocucaje, he found there again remains of the Chincha-Ica type; but alongside them, in a separate small cemetery, a different type, which we now call the culture of Nazca. It was the first

time that an archaeologist had discovered this Nazca culture in situ; previously it had been known only to a few huaqueros. Uhle excavated at Ocucaje only some sixty or eighty pieces of Nazca ceramics. When subsequently he pushed still farther south and entered the valley of Nazca he encountered, like every one since, a much greater abundance of material of the Nazca culture. But this material was more diverse; and its segregation into its cultural constituents might have been difficult except for the little Ocucaje find. This, being stylistically pure on account of its very smallness, happened all to be what I call Nazca A. It was, therefore, a simple matter to subtract from the larger and more mixed collections from the valley of Nazca, everything which equalled the Nazca from Ocucaje, and the residue, except for some transitional forms, was the related variant to which I give the name B. In short, the material from Nazca that first came into our museums was a mechanical mixture of A and B, as almost always happens when the provenience is from huaqueros; but Uhle's fortunate discovery of pure A at his small site in Ocucaje, allowed the isolation of B. We might almost express the process of determination mathematically:  $(A + B) = A + B$ .

Finally, I should like to make the plea that no quarrels over nomenclature be allowed to distract the progress of archaeological determination. Differences of fact must be recognized, and differences of interpretation are legitimate. But difference of name should be mutually respected. What I have called Nazca B, Dr. Tello now calls Chanca, no doubt for good and sufficient reasons. I shall probably continue to call the culture Nazca B and accord to him the full right to call it Chanca, and reciprocally; the important thing is that we know we mean the same thing. Future generations of archaeologists will decide which of the two appellatives they prefer to use--or whether it will be a third one. Similarly with what Sr. Larco calls Cupisnique and Dr. Tello Chavin, which are certainly mainly the same thing; or the most recent finds at Ocucaje, which, whether made into a cultura Ocucaje or denominated as cultura Paracas-Cavernas, still remain strikingly alike.

The preference for one nomenclature over another, so far as it is not wholly personalized, is probably due to implications of interpretation, of which the appellations chosen are suggestive. It is well to remember, however, that in all science theoretical generalizations are transient. They die, in time, or are modified beyond recognition, or perhaps persist but only with new weighing and altered significance. The one thing that is permanent in archaeology, as in the other sciences, is the gradually accumulating body of organized and relational facts, which is the product of no one worker, but of an indefinite number of collaborators striving toward the same ends.