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Archeological Research Strategies: Past and Present

Current goals, methods, and structure of thought approach more closely those of the natural sciences.

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Archeology, in the minds of most laymen, probably has two aspects. One involves explorations in exotic lands—sun and sand, the menace of wild tribesmen, the lure of treasures from the East. The other involves the painstaking excavation, restoration, and display of individual antiquities which are thought to be the primary focus of study—a kind of philately of art styles or material objects abstracted from their cultural surroundings and handsomely illustrated on quarto volume plates or placed in museum cases.

At first glance, neither of these images of archeology has much to do with prevailing concepts of scientific research as they are usually formulated with especial reference to the natural sciences. Instead, both seem to stress a subjective quality of scholarship—beginning with the inexplicable act of discovery beneath the sand and then proceeding almost mystically from physical description to intuitive reconstruction of forgotten historical events. Any attempt to wrest meaning from broken, unrecognizable artifacts seems, after all, to partake more of imagination than of plausibility. In fact, this is almost the direct antithesis of laboratory-generated procedures involving the inductive fashioning of explanatory hypotheses and then rigorous empirical testing.

Now that science is in the ascendancy, and the expansion of research frontiers into interstellar space and subatomic structure are proclaimed virtually as national goals, archeology nonetheless thrives as never before. Paperback books, popular articles, and news media communicate even quite specialized or rather doubtful findings of archeological research to an ever larger public; the number of recognized practitioners of the subject climbs at every professional meeting; and the scale and diversity of archeological undertakings mounts steadily. In a recent article the director of the University Museum in Philadelphia (one of the outstanding institutions in the field) speaks of "the archeology explosion" (*1*). Why is this? Is the growth of the field a mere epiphenomenon of the powerful forward surge of research in the natural sciences? Is it a consequence of international attempts to solve balance of payments problems by attracting growing numbers of tourists to newly opened archeological monuments and museums? Worse still, is it possible that man's increasing interest in the unearthing of his past is an outgrowth of increasing uncertainties over his future—that archeology has prospered because it panders to a prevailing mood of escapism?

To some degree, all of these possibilities may be true. But the deeper truth is that the scientific dimensions of archeology have grown most rapidly. Although the process is disorderly and controversial at many points, profound changes can be discerned not only in immediate research strategies but also in underlying structures of thought. This is less an explosion than a revolution in the sense that it is a broad shift from one paradigm to another not unlike the shifts which Thomas S. Kuhn (*2*) has metaphorically outlined for the history of physics. In any case, the major current changes offer parallels with many disciplines in the natural sciences. In choosing to discuss them, I am perhaps throwing another slender bridge across the void between the hard and soft sciences, or even between the sciences and the humanities. We all deplore this void; I attempt to span it because frequently we may overestimate its breadth and importance.

A brief description of the earlier stages in the maturation of archeology as a discipline may help to clarify the issues in dispute at the present turning point. As the field emerged, one concern was the confirmation of its basic stratigraphic assumptions. Exploration in its own right was equally important, at least until the maps of empty continents began to fill with archeological discoveries. Some geographical voids still remain, but the once commanding appeal of exploration has cumulatively been reduced to a secondary level. The last lost civilization was brought to light in the Indus Valley during the 1920's. It can reasonably be said that no unsuspected discovery of like magnitude awaits the spade of any future digger.

In addition to purely exploratory and methodological concerns, several other themes dominated early work. Viewed retrospectively, the most repugnant was an often highly competitive interest on the part of the major muse-

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ums in excavating or otherwise acquiring beautiful objects with a minimum of supervision or record keeping. Such activities are now prevented by law in most countries—the United States is not one of them—but the two generations or so that have elapsed since the end of that era of pillaging have not erased its memory in much of the underdeveloped world.

Another major concern was the verification and amplification of written records that play a central role in our cultural heritage. Schliemann's work at Troy falls into this category; so also does the heavy concentration on biblical sites in Palestine. This objective persists, although tempered by increasing cautiousness in interpretation and much reduced in relative importance. So also, more regrettably, does a preoccupation with seeking origins. The quest for origins has contributed very little of scholarly importance, but continues to be encouraged by the distorted values that the news media attach to what is first.

This heterogeneous assortment of initial objectives reflects the diversity in modes of thought among those who, in the latter part of the 19th century, called themselves archeologists. Recent work in the history of ideas is making us increasingly aware of the degree to which the intellectual paternity we proclaim for ourselves today often has more in common with the origin myths of primitive peoples recorded by anthropologists than with the actual roots which nourished our contemporary academic disciplines. Nevertheless, two partially distinct paths of development may be traced well into the past. The first to take shape involved the rediscovery of the classical world as a complex, viable, and aesthetically pleasing civilization that yet was culturally distinct, pagan, and temporally remote. This brought together almost from the beginning the private collector, the historian of art, and the philologist. The acquisition of monuments through excavations stimulated the formation of an objective, external view of entities like style and culture.

The other intellectual taproot of archeology led largely through the terrain of the natural sciences. It probably began in the 18th century, with the uniformitarian assertions that the earth's surface should be regarded as a system of matter in motion over immensely long time periods. Later this was linked with speculations on natural selection and evolution in the biological world, and ultimately with Darwin's classic synthesis. Of course, the separation we draw retrospectively between these two sources of ideas may be somewhat artificial. Only as scholarship became a specialized, university-based activity, in the later 19th century, did the outward form of archeological reports in both categories cease to be that of a description of travels that could appeal to a wide audience.

With the shift in focus of activity from private travelers and collectors, to public museums, and finally to university departments, began the bifurcation of the field that continues today. At least in the United States, all of prehistory and all of the aboriginal New World fell within the province of anthropology, a discipline in the generalizing, comparative tradition of the social sciences. Each of the literate civilizations of the Old World, in distinction, became the province of a more specialized, humanistic tradition of study in which archeological interests generally have been secondary to those of documentary historians and philologists. Even though a few individuals manage to keep an uneasy foot in each camp, the division is nonetheless deep and genuine. This makes the task of speaking of the field as a whole considerably more difficult. Since most of the visible ferment has at least originated in the domain of the anthropologists, one is justified in placing major emphasis there.

The next broad stage in the growth of archeology involved a virtual preoccupation with systematics. The guiding assumption was that styles uniformly followed a curve of normal distribution, changing gradually in both time and space, so that a plotting of the distribution of formal similarities in time and space would provide a reliable index of cultural and historical relationships. Consciously conducted, empirical tests of this assumption have been conspicuous by their absence; it supplied the seemingly self-evident paradigm of normal science by which alone a pattern of order and significance could be imposed on increasing masses of descriptive data. The main objectives of new research became the extension and progressive refinement of chronological charts showing the succession and distribution of clusters of formal similarities in artifacts that were called "cultures." Except in the hands of a few vigorous theorists of the time like V. Gordon Childe, interpretation was generally confined to descriptions of changing architectural and artifact inventories at individual sites, and to assessments of trade, migration, and culture contact that could be deduced from formal similarities linking different sites.

With due allowance for obvious regional variability, this was roughly the state of affairs at the time of the long hiatus that the second world war induced in fieldwork. In spite of the small number of institutions actively involved, there had been some impressive accomplishments. Leaving aside substantive discoveries, a number of classic monographs had been published by meticulous excavators, or soon would be finished on the basis of materials already in hand. Imposing standards of excavation technique and descriptive analysis had been erected, in other words, against which archeological undertakings everywhere were increasingly subject to critical comparison. A systematic framework of temporal and spatial distributions had been at least roughed out for most areas and in some had been highly refined. Without such a framework, the more analytic, causally oriented approaches of more recent decades never could have been initiated. Nevertheless, we can best describe and evaluate present trends in the field not as they have built continuously upon this underlying body of methods and assumptions but as they have radically enlarged it and even departed from it.

Of the greatest importance has been the elaboration of an emphasis on an ecological approach. Descriptive statements on climate and environment already were included in some traditional site reports, but now the questions asked and the methods followed bear little resemblance to this prototype. What is seen as crucially important for study is no longer some uniform, predetermined set of obvious environmental features but the points of articulation between the subsistence activities of a particular human group and the wider natural and social setting within which it operated. The focus of concern, then, is the shifting, complex set of adaptive responses which must characterize any community, ancient or modern, and which in turn can help to explain the changes it undergoes through time. Average annual rainfall, for example, is an obvious and once frequently cited statistic which now is seldom regarded as important. What counts more is the reliability of

its onset and periodicity during the growing season, the frequency with which it fails to meet the minimum needs of cultivation, or the destructive intensity of the storms in which it falls. Factors like these are critical in explaining changing subsistence productivities and hence also many related sociocultural changing features including patterns of settlement. As often as not, a perceptive assessment of the setting of an archeological site along such lines goes far beyond even the best contemporary data gathered for other purposes, and requires the gathering of additional data as a part of the archeological project itself.

Rainfall is only one among a very large number of features which might be used as an example. The trend has been toward reliance on greater and greater numbers of converging lines of evidence for both ancient and modern environments—soils, bones, pollen, geomorphology—in order to discover unsuspected cultural variables, to reduce ambiguities in interpretation, and to deal with the interlocking effects of the widest possible range of adaptive relationships. This in turn requires other reorientations, principally in the training of students and in the increased funding of projects. In some of the most important and productive undertakings of recent years, such as those concerned with the locally differentiated processes by which plants and animals were independently domesticated in the Old and New worlds soon after the end of the Pleistocene, the greater part of the effort and expense has been directed at the analysis of ecological variables rather than at all of the traditional classes of archeological findings taken together.

The immense broadening of effort that an ecological approach requires has altered the social milieu of the research itself. The organizational model increasingly being followed is not dissimilar in some ways to that of the physical sciences. Groups of collaborators form, whether on one faculty or several, and institutionalize their relations in a variety of ways. Unlike the very large research groups currently active in physics, however, such groups in archeology still remain characteristically fluid in internal structure and egalitarian in outlook; a serious deficiency is that these groups do not have long-term support from technicians. The groups are not tied to particular laboratories or to expensive equipment like particle accelerators, but they are constrained by equally effective geographical limits. Long experience in an area is almost a necessity in fashioning a valid ecological approach, and in the case of work overseas one must add to this the need for linguistic competence and for a wide knowledge of administrative procedures and political realities within a host country. Moreover, these groups also generally tend to work within the bounds of a particular focal problem, probably in large part because of close operating relationships with natural scientists whose contributions are most appropriate for those problems. Natural scientists, in fact, form a major component of such groups. However, since the core problems remain those of understanding changes in human culture, the responsibilities for direction, coordination, synthesis, and fund raising generally remain with the archeologist.

The increasing emphasis on an ecological approach also is related to a growing dissatisfaction with narration or description for its own sake. This has brought in its wake a correspondingly heightened interest in seeking causal explanations of a deterministic character, and such explanations usually have stressed ecological factors. At the extreme, it has been suggested that environmental differences are virtually the only explanation of cultural differences, aside from periodic quantum jumps in the availability of nonhuman energy resulting from the growth of technology. More persuasive, although less sweeping, is the position taken on this question by a coherent, highly innovative group of "new" or "process oriented" archeologists, one of whose number recently formulated it as follows (*3*).

> . . . [T]he process school would like to move crucial decisions . . . farther from the individual by arguing that systems, once set in motion, are self-regulating to the point where they do not even necessarily allow rejection or acceptance of new traits by a culture. Once a system has moved in a certain direction, it automatically sets up the limited range of possible moves it can make at the next critical turning point. This view is not original with the process-school archeologists—it is borrowed from Ludwig von Bertalanffy's framework for the developing embryo, where systems trigger behavior at critical junctures and, once they have done so, cannot return to their original pattern. The process school argues that there are systems so basic in nature that they can be seen operating in virtually every field —prehistory not excepted. Culture is about as powerless to divert these systems as the individual is to change his culture.

There are several points worthy of notice here. For one, this is by no means to be equated with the naive assertions of geographical determinism that were in wide circulation a generation or two ago. Cultural and environmental features are seen as closely interacting, rather than the former being either the helpless pawn of the latter or else free to improvise within well-defined limits imposed by the environment. At the same time, the stimulus and model are quite explicitly of biological rather than historical origin. It is fair to say that all schools of historical thought today, including the Marxists, have moved well away from rigorously deterministic modes of interpretation which tend to limit and distort all but the grossest, most self-evident kinds of social and cultural change. By taking its lead from ontogenetic analogies rather than from the broad trend of historical studies of human groups, this school of archeologists lays itself open to the serious charge of reductionism.

That the dominant regularities of cultural behavior should be imposed by sustaining or unstabilizing ecological interactions is surely only a hypothesis. The extent of validity of that hypothesis is a matter for empirical determination rather than prejudgment, and hardly can be determined convincingly so long as attention is confined to systems that fit biological patterns alone. There is, to be sure, a heuristic defense for pursuing this strategy, at least where documentary evidence is not available (*3*).

Obviously, individuals *do* make decisions but evidence of these individual decisions cannot be recovered by archeologists. Accordingly it is more useful for the archeologist to study and understand the system, whose behavior is detectable over and over again. Obviously, this approach is too deterministic for some purposes, but for others it is of great theoretical value.

Included in the program of this small but growing group of process-oriented archeologists are a number of other features which serve to differentiate their approach from the traditional one. To begin with, they place heavy emphasis on formal procedures for critically testing deductively drawn hypothesis against independent sets of data. Hence very little tolerance is shown for intuitive interpretations or analogies, on the grounds that judgments about the correctness of the latter must depend to a large extent on a subjective sense of internal consistency and fit that cannot be replicated.

In addition, their attitude toward

the limits of interpretation attainable from archeological findings is characteristically an expansive, optimistic one. The traditional starting point for archeology has been that differing degrees of preservation and later disturbance usually limit the evidence directly recoverable from the ground to only a fragment of that laid down originally. Moreover, even before they were diminished by the effects of time, archeological data were only the material vestiges of much more complex behavioral patterns of which no direct trace survives. But rather than limiting their concerns to questions of subsistence, technology, and economy that can be most directly and unambiguously answered from the archeological record, the process-oriented archeologists observe that social organization and even ideology must have influenced and been influenced by these other realms of organized behavior at innumerable points. On this basis, a leading spokesman (*4*) states flatly that "data relevant to most, if not all, the components of past socio-cultural systems *are* preserved in the archeological record. Our task, then, is to devise means for extracting this information from our data." Clearly, this group has moved away from sifting and synthesizing what is known of an extinct way of life as a whole through its archeological vestiges, and has centered its interests instead on the formulation of more sharply focused, but also more adequately testable, hypotheses.

I do not mean to imply that process-oriented archeology will or should replace the traditional reliance on induction. Its protagonists have called attention to defects in prevailing strategies of study, and have fashioned an approach within which ecology becomes not merely a fashionable slogan but an organizing concept. Nevertheless, relatively few propositions have been advanced and fully documented by members of the process school as to systemic interrelationships involving material, ecological, and sociological components. The bane of subjective interpretations and categories still affects much of the primary data on which they depend, albeit somewhat disguised by increasingly sophisticated quantitative manipulations. Although of great methodological interest, most such propositions to date remain merely plausible; validation, or even the evaluation of probability, has proved again to be a difficult goal to pursue through the complexities that surround man and his works.

Moreover, it seems hard to deny that the central creative activity in archeology, like in all scholarship, lies in induction, in outstripping the narrow base of available facts to suggest new and essentially speculative unities. Formal analytical procedures are surely a useful adjunct to qualities of reflective judgment in assessing the consistency, utility, and fit to these hypothetical unities or explanations, but they are hardly likely to become a full-scale substitute. Both will surely remain, their complementarity enhancing archeology's claims to work within the framework of a genuinely scientific methodology when the current agitation subsides. What the ferment does indicate, however, is that issues at the core of any philosophy of science now have become critical for a discipline many would unhesitatingly assign to the humanities.

Another major trend of thought in archeology involves the increasing tendency to study cultural change as evolutionary change, recognizing the unique properties that culture imparts to the human record but nonetheless employing biological models for certain of its most significant or widely recurrent features. Most emphatically, this does not imply a return to crude popular notions of cultural Darwinism, such as the direct competition of inherently unequal and antagonistic groups for survival. I refer instead to some of the underlying conceptions of contemporary evolutionary biology: variable populations, rather than individuals or types, as the units upon which alone the action of selective pressures can be understood; the delicate, many-vectored mechanisms of the process of natural selection itself; adaptive radiations, through which populations rapidly evolve to fill new ecological niches; and, finally, the conceptualization of the results of evolution as a progression of irregular but irreversible transformations.

How are these conceptions reflected in current archeological research? Partly in the increasing attention being given to ecology that has already been mentioned. But equally important, greater emphasis is being given to the critical processes of transformation that have led from one general level of organizational complexity in human society to another. Unlike the earlier use of stages as little more than typological constructs, the problem of the basic structural features of these successive quantum levels, and of the detailed sequence of steps by which they emerged in parallel instances, now are becoming uppermost. Transitional processes rather than static conditions are the focus of an unprecedented, if still loosely coordinated, attack by a considerable number of individuals at different institutions here and abroad, providing clusterings of greatly heightened activity within what previously had been a diffuse scattering of projects and problems. Among such developmental processes are, for example, the independent origins of agriculture, urban centers, and primary technological complexes like metallurgy or, more generally, pyrotechnics. And in the same way that key processes of change have been identified, key regions of change are receiving correspondingly increased attention. Central Mexico apparently was such a region within the much broader area where aboriginal civilization once flourished in Middle America, and the intensified surveys and excavations there in recent years are a good illustration of the point.

Having alluded to variable populations as the crucial unit of evolutionary analysis, I must return to this theme to describe an important additional trend in thought. Variability is always present in the inventory of artifacts that the archeologist recovers, but from former acceptance of curves of normal distribution in space and time as a priori assumptions, archeologists have moved to empirical studies of geographic variability and tempos of change. Processes of innovation, stylistic drift, and diffusion all are brought under scrutiny wherever circumstances permit adequate control over differences in time. Similarly, patterns of spatial variation that do not approximate the normal ones now are being regarded as significant clues to the kinship and other social groups of which the makers of the artifacts were members. A variety of new models and methods are required in the search for correspondences of this type, most of them originally developed by locational geographers; among them are linear regression and multivariant-factor analyses in order to detect nonrandom clusterings of variables dependent upon one another. From a holistic conception of extinct cultures as bodies of shared norms, changing only in response to the slow movement of stylistic variables

except where subjected involuntarily to external influences, we have come to expect sharp accelerations and retardations of change and wide differences in the range of variability. By relating such differences to their cultural and natural setting, a new and powerful tool has been fashioned not merely for documenting the fact of change more accurately and interestingly than heretofore, but also for supplying convincing explanations of it.

And what of advances stemming from the physical sciences? It may have struck some as odd that I have yet to mention the dating and detection devices whose impact on archeology is perhaps the most widely publicized aspect of my subject. The delay to the end is deliberate, for while their contribution is certainly very great it does not alter the whole structure of thought to the same degree as the other new features with which I have dealt.

Radiocarbon dating is the best and most important example. In one sense, as a recent overview states, it has "revolutionized archeological ideas concerning the chronology of human events during the last 40,000 years (*5*). The worldwide synchronism of late glacial and postglacial climatic phases, the timing of agricultural origins and dispersals, the succession of cultural periods in many areas where long stratigraphic sequences do not exist to provide them securely, and the correlation of the Maya and Christian calendars are among the many important issues to which radiocarbon determinations have contributed decisively. One can argue also that the use of radiocarbon led indirectly to a considerable improvement in prevailing standards of fieldwork, requiring greater sophistication in sampling, in the detection of disturbances and contaminants, and in the evaluation of context if the resultant dates were to withstand critical comparison with others. Moreover, the slowly advancing precision of the system, both with regard to individual determinations and more especially to the cumulative series of them, has opened up the prospect of more carefully controlled studies of change in the future than any heretofore possible.

Errors and misinterpretations on the part of archeologists have not been uncommon, but the principal deterrents to the realization of this potential are inherent in the radiocarbon process itself. The expression of standard deviations from the mean determination at times has been taken to imply absolute limits within which the age of the specimen must fall, and at other times has been ignored altogether by archeologists. But even in the absence of these two linked forms of confusion the existence of a still fairly large plus or minus range for each dated specimen limits any fine-grained analysis of change. There has been considerable progress in recognizing sources of contamination, but this also implies that the reliability of determinations is to some degree dependent on when, how, and by whom they were made. Most important, the recognition of a number of factors which have exercised an irregularly distorting influence on the production of the carbon-14 isotope in the upper atmosphere for a time threatened to increase our interpretive uncertainties almost beyond tolerable limits. Recent empirical determinations based on bristlecone pine tree-ring sequences of known age are helping to correct these very considerable sources of error from the late sixth millennium B.C. onward (*6*), but determinations of greater age still remain disturbingly uncertain. None of this is intended to minimize the major contribution that radiocarbon dating has made. In the aggregate, it has supplied a system of absolute chronology that was essentially lacking previously. But numerous examples could be cited indicating that it remains unwise to rely very heavily on individual dates or even groups of dates. And unless ways can be found to obtain a further increase in optimum accuracy by an order of magnitude, studies of the dynamics of change based on the archeological record will continue to be noteworthy more for their promise than for their performance.

In some ways, the availability of a whole series of supplements and alternatives to radiocarbon provides the greatest hopes for archeology. Even within the age range for which radiocarbon is now the preeminent method, determinations based on the thermoluminescence of pottery offer certain potential advantages. Principal among these is that pottery is itself a human artifact, while occasionally there has been a considerable interval between the lifespan of some organic material dated by radiocarbon and its employment as an artifact. Thus far, however, the margin of error in the thermoluminescent method is still much larger than with the radiocarbon method (*7*). Of greater current importance are measurements of the thickness of a hydration layer that forms continuously on chipped artifacts of obsidian. Since the rate of hydration varies with temperature, the method at first sight is not very promising for absolute chronology. However, it can very sharply distinguish components of different relative age within a particular site or area where the temperature is essentially a constant, and its low cost permits multiple determinations (*8*). Finally, for sites too old to be subjected to radiocarbon analysis, thorium-uranium, protactinium-uranium, and potassium-argon dating also are in use. The remote geochronological horizons to which they are applicable make them of particular importance for advances in the understanding of the biological evolution of the human species.

Space permits only the briefest mention of the promising beginnings made with a variety of detecting and locating systems. Magnetometers of rapidly increasing sensitivity have been shown to be effective in mapping ruined settlements beneath as much as 5 meters of overburden. Grids of soil resistivity measurements have been employed for the same purpose at shallower depths. Aerial photography is becoming an increasingly commonplace adjunct of both surveys and excavations, and there is considerable experimentation with the use of high resolution, multi-band and photogrammetric techniques. Underwater archeology, only the romantic province of the untrained skin diver a fews years ago, is now an elaborately equipped, highly specialized field of its own. Without any doubt at all, the use of these and similar approaches will become a regular, even dominant, feature of archeology in the decades immediately ahead. But again, their present importance is more in the realm of promise than of published performance. And the changes in the basic tenets of archeological thought that I have emphasized are largely anterior to and independent of them.

Some of my colleagues will object that the emphasis I have given to these new trends of thought also is more of a hope or a promise than a balanced estimate of accomplishments to date. Probably they are at least partly correct. My personal bias has always been to look less at what the world is—or was at a given time—than at what

it is—or was then in the process of—becoming. I would insist that the new paradigm I have tried to describe is taking hold, particularly among the younger members of the profession. On balance, however, there is little doubt that most of the work in the field is still descriptively, rather than causally, oriented.

In relatively few studies is the central problem of explaining change directly considered in terms of either the relevant data sought or the conceptual apparatus used. In spite of the widespread acceptance in theory of regional-ecological models that should depend on rigorous statistical sampling, the predominant focus of research for most investigators remains the arbitrarily chosen slice of a particular ancient site that is excavated. Specialists in the natural environment are still too often employed as technicians "expected to provide ready answers to poorly formulated questions" (*9*), rather than engaged in a genuinely collaborative study. Problems and objectives too often are formulated only as armchair generalizations, rather than as sharply focused hypotheses to which definitive answers must be sought in regional or local sequences. Meanwhile, separated by a wide gulf from the former, the conduct of much research at the local level remains practically innocent of relevance to any theoretical problem whatever.

Holistic presuppositions about societies in general also have not been sufficiently clarified and tested. As a result, there may well be an excessive emphasis in archeological interpretation on stability rather than instability as the salient human condition. To phrase this differently, an oddly antiquated, almost Victorian emphasis continues on institutions and behavior that performed integrative functions—art styles, rituals, elites—at the expense of conflict, marginality and dissonance as sources of creativity and change. Possibly for the same reason, most reconstructions of archeological sequences still consist of a succession of qualitatively distinct, smoothly functioning phases or stages rather than of the continuous interplay of forces marked at intervals by new transformational forms. Finally, most of us remain excessively timid, reluctant to tackle the grand problems of comparison, generalization, and synthesis, even though the certainty of being found in frequent error if we did so ought to be heavily outweighed by the opportunity to deepen, sharpen, and ultimately justify our inquiries. But, before I allow these criticisms of the present state of archeology to seem overwhelming, perhaps I should ask whether at least some of them do not have analogs in the natural sciences as well.

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The Development of Retinal Neurophysiology

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For some 20 years my main experimentation has not been within the field of retinal electrophysiology; my interests have shifted to problems of motor control—chiefly muscular sense organs and quantitative properties of firing motoneurons. For this reason my lecture will be as retrospective as the Award and deal with the rebirth of retinal studies that was inspired by the increasing accessibility of electronic equipment from the mid-1920's onward. Before this era, vision was chiefly studied by psychophysical methods; these had led to well-established correlations between perceptions of such things as color and luminosity and physically defined entities within the centimeter-gram-second system of units. But in 1926 Adrian and Zotterman (*1*), using the afferent nerve of a stretch receptor in a frog muscle, had shown that it was possible by amplification to record the impulses in single nerve fibers, and in 1927 and 1928 Adrian and Rachel Matthews (*2*, *3*) published their important experiments with the mass discharge in the long optic nerve of the *Conger* eel. Evidently the electrophysiological approach was the one now destined to pave the way for deeper understanding of how this, our noblest sense organ, had organized its interpretation of the world of light, form, and color.

Frithiof Holmgren (1831–97) (*4*) at Uppsala, who in 1865 discovered the electrical response of the retina to light, the electroretinogram of today, had realized that many problems of vision could be analyzed by what he called his "method of objectivating the impressions on the retina," but amplification with the aid of the vacuum tube gave this general idea quite a different dimension; it was—Lord Adrian said—as if we had got a new, very powerful microscope to work with.

The basic idea of my own approach was formed during my study of Ramón y Cajal's (*5*) classical description of the retina as a "true nervous centre," as clarified by his silver-stain preparations and likewise suggested by its embryological development from the brain. It seemed to me likely that psychophysical data might, with some profit to the field, be translatable into neurological equiva-

Professor Granit won the 1967 Nobel Prize for medicine or physiology, along with H. K. Hartline and George Wald. This is the text of his lecture before the Royal Caroline Institute on 12 December 1967. He is professor emeritus in the neurophysiological department of the Medical Nobel Institute, the Royal Caroline Institute, Stockholm 60, Sweden. The article is published here with the permission of the Nobel Foundation and will also be included in the complete volume of *Les Prix Nobel en 1967*, as well as in the series *Nobel Lectures* (in English) published by the Elsevier Publishing Company, Amsterdam and New York.