



BOOKS

Culture history v. cultural process: a debate in American archaeology

by Kent V. Flannery

AN INTRODUCTION TO AMERICAN ARCHAEOLOGY, VOLUME I: NORTH AND MIDDLE AMERICA, by Gordon R. Willey. Prentice-Hall, Inc. (\$16.95).

A dominant characteristic of American archaeology has been its long history of reaction to American ethnology. When ethnology was little more than the collecting of spears, baskets and headdresses from the Indians, archaeology was little more than recovery of artifacts. When ethnology increased its attention to community structure, archaeology responded with studies of settlement pattern—an approach in which Gordon Willey was an innovator. Publication of works by Julian H. Steward and others on “cultural ecology” was answered by great archaeological emphasis on “the ecological approach.” When the concept of cultural evolution emerged triumphant after years of suppression, archaeology showed great interest in evolutionary sequences and in the classification of “stages” in the human career. The interaction of these two disciplines has been increased by the fact that in the U.S. both are housed in departments of anthropology; as Willey remarked some 10 years ago, “American archaeology is anthropology or it is nothing.”

And now, in 1967, Willey—Bowditch Professor of Mexican and Central American Archaeology and Ethnology at Harvard University—has written a monumental synthesis of New World prehistory. There is nothing like it. Recently we have had several edited volumes on the New World with contributions by regional specialists, but this book is written cover to cover by one man. Thus the inevitable lack of firsthand familiarity with certain areas is partially offset by the advantage of having one consistent approach and writing style throughout. Although aimed at the student, the book's costly format almost prices it out of the student range. It is a centerpiece for the

coffee table of the archaeological fraternity, at least until an inexpensive paperback edition can be produced.

Willey's archaeological career is reflected in monographs and articles on every major land mass of the New World, from the region of the Woodland culture in the U.S. Northeast to the Maya area, the shell mounds of Panama and the coastal border of the Andean civilization. He is a perennial favorite who for a variety of reasons has never come under attack. One reason is his avoidance of any one polarized theoretical position; the other is his adaptability in the face of continual change. While other members of the establishment have clenched their fists and gritted their teeth when their formerly useful theories dropped from favor, Willey has shown no such hostility; younger archaeologists sense he would rather join them than lick them. And he is always free to join them as long as he maintains no vested interest in any comprehensive theory that needs defending.

This book, well organized from the primary literature and from constant conversations with Willey's colleagues, is no exception. It is unlikely to stir up controversy except where Willey commits himself to one of a series of possible theories proposed by others—for example, siding with Emil W. Haury rather than Charles C. Di Peso on the interpretation of the U.S. Southwest, or with Henry B. Collins rather than Richard S. MacNeish on the American Arctic. It is not Willey's aim to intrude his own theories into the synthesis. Indeed, he tells us that he is “not demonstrating or championing any one process, theory or kind of explanation as a key to a comprehensive understanding of what went on in prehistoric America.” Clearly Willey feels that it would be misleading to do more than present the student with the facts as most of his colleagues agree on them in 1967. Hence “the intent of this book is history—an introductory culture history of pre-Columbian America.”

This statement by Willey makes it appropriate to consider one of the current theoretical debates in American archae-

ology: the question of whether archaeology should be the study of culture history or the study of cultural process. In view of this debate it is interesting to note that in practically the same paragraph Willey can brand his book “culture history” and yet argue that he is “not championing any one point of view.”

Perhaps 60 percent of all currently ambulatory American archaeologists are concerned primarily with culture history; this includes most of the establishment and not a few of the younger generation. Another 10 percent, both young and old, belong to what might be called the “process school.” Between these two extremes lies a substantial group of archaeologists who aim their fire freely at both history and process. And although Willey himself belongs to this group, his *Introduction to American Archaeology* also constitutes a massive restatement of the accomplishments of the culture-history school.

Most culture historians use a theoretical framework that has been described as “normative” (the term was coined by an ethnologist and recently restressed by an archaeologist). That is, they treat culture as a body of shared ideas, values and beliefs—the “norms” of a human group. Members of a given culture are committed to these norms in different degrees—the norm is really at the middle of a bell-shaped curve of opinions on how to behave. Prehistoric artifacts are viewed as products of these shared ideas, and they too have a “range of variation” that takes the form of a bell-shaped curve.

In the normative framework cultures change as the shared ideas, values and beliefs change. Change may be temporal (as the ideas alter with time) or geographic (as one moves away from the center of a particular culture area, commitment to certain norms lessens and commitment to others increases). Hence culture historians have always been concerned with constructing “time-space grids”—great charts whose columns show variation through the centuries. Some have focused an incredible amount of

attention on refining and detailing these grids; others have been concerned with discovering "the Indian behind the artifact"—reconstructing the "shared idea" or "mental template" that served as a model for the maker of the tool.

While recognizing the usefulness of this framework for classification, the process school argues that it is unsuitable for explaining culture-change situations. Members of the process school view human behavior as a point of overlap (or "articulation") between a vast number of *systems*, each of which encompasses both cultural and noncultural phenomena—often much more of the latter. An Indian group, for example, may participate in a system in which maize is grown on a river floodplain that is slowly being eroded, causing the zone of the best farmland to move upstream. Simultaneously it may participate in a system involving a wild rabbit population whose density fluctuates in a 10-year cycle because of predators or disease. It may also participate in a system of exchange with an Indian group occupying a different kind of area, from which it receives subsistence products at certain predetermined times of the year; and so on. All these systems compete for the time and energy of the individual Indian; the maintenance of his way of life depends on an equilibrium among systems. Culture change comes about through minor variations in one or more systems, which grow, displace or reinforce others and reach equilibrium on a different plane.

The strategy of the process school is therefore to isolate each system and study it as a separate variable. The ultimate goal, of course, is reconstruction of the entire pattern of articulation, along with all related systems, but such complex analysis has so far proved beyond the powers of the process theorists. Thus far their efforts have not produced grand syntheses such as Willey's but only small-scale descriptions of the detailed workings of a single system. By these methods, however, they hope to explain, rather than merely describe, variations in prehistoric human behavior.

So far the most influential (and controversial) member of the process school has been Lewis R. Binford of the University of California at Los Angeles, and it is interesting to note that Binford's name is confined to a single footnote on the last page of Willey's text. It is Binford's contention that culture historians are at times stopped short of "an explanatory level of analysis" by the normative framework in which they construct their classifications. Efforts to reconstruct the "shared ideas" behind arti-

fact populations cannot go beyond what Binford calls "paleopsychology"—they cannot cope with systemic change. And where Willey says that "archaeology frequently treats more effectively of man in his relationships to his natural environment than of other aspects of culture," Binford would protest that most culture historians have dealt poorly with these very relationships; their model of "norms," which are "inside" culture, and environment, which is "outside," makes it impossible to deal with the countless systems in which man participates, none of which actually reflect a dichotomy between culture and nature. The concept of culture as a "superorganic" phenomenon, helpful for some analytical purposes, is of little utility to the process school.

As a convenient example of the difference in the two approaches, let us examine three different ways in which American archaeologists have treated what they call "diffusion"—the geographic spread of cultural elements. It was once common to interpret the spread of such elements by actual migrations of prehistoric peoples (a view, still common in Near Eastern archaeology, that might be called the "Old Testament effect"). The culture historians attacked this position with arguments that it was not necessary for actual people to travel—just "ideas." In other words, the norms of one culture might be transmitted to another culture over long distances, causing a change in artifact styles, house types and so on. A whole terminology was worked out for this situation by the culture historians: they described cultural "traits" that had a "center of origin" from which they spread outward along "diffusion routes." Along the way they passed through "cultural filters" that screened out certain traits and let others pass through; the mechanics of this process were seen as the "acceptance" or "rejection" of new traits on the part of the group through whose filter they were diffusing. At great distances from the center of origin the traits were present only in attenuated form, having been squeezed through so many filters that they were almost limp.

Since process theorists do not treat a given tool (or "trait") as the end product of a given group's "ideas" about what a tool should look like but rather as one component of a system that also includes many noncultural components, they treat diffusion in different ways. The process theorist is not ultimately concerned with "the Indian behind the artifact" but rather with the system behind both the Indian and the artifact: what other com-

ponents does the system have, what energy source keeps it going, what mechanisms regulate it and so on? Often the first step is an attempt to discover the role of the trait or implement by determining what it is functionally associated with; some process theorists have run extensive linear-regression analyses or multivariate factor analyses in order to pick up clusters of elements that vary with each other in "nonrandom" ways. When such clusterings occur, the analyst postulates a system—tools X, Y and Z are variables dependent on one another, constituting a functional tool kit that varies nonrandomly with some aspect of the environment, such as fish, wild cereal grains, white-tailed deer and so on. By definition change in one part of a system produces change in other parts; hence the process theorists cannot view artifacts X, Y and Z as products of cultural norms, to be accepted or rejected freely at way stations along diffusion routes. When such elements spread, it is because the systems of which they are a part have spread—often at the expense of other systems.

Thus the archaeologist James Deetz recently presented evidence that the spread of a series of pottery designs on the Great Plains reflected not the "acceptance" of new designs by neighboring groups but a breakdown of the matrilineal residence pattern of a society where the women were potters. Designs subconsciously selected by the women (and passed on to their daughters) ceased to be restricted to a given village when the matrilineal pattern collapsed and married daughters were no longer bound to reside in their mothers' villages. In this case, although each potter obviously did have a "mental template" in her mind when she made the pot, this did not "explain" the change. That spread of design could only be understood in terms of a system in which designs, containers and certain female descent groups were nonrandomly related components. The members of the process school maintain that this is a more useful explanatory framework, but even they realize that it is only a temporary approach. They are becoming increasingly aware that today's human geographers have ways of studying diffusion that are far more sophisticated and quantitative than anything used by contemporary archaeologists.

One other example of the difference in approach between the culture historian and the process theorist is the way each treats the use of "ethnographic analogy" in archaeological interpretation. The culture historian proposes to analyze and describe a prehistoric be-



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havior pattern, then search the ethnographic literature for what seems to be analogous behavior in a known ethnic group. If the analogy seems close enough, he may propose that the prehistoric behavior served the same purpose as its analogue and then use ethnographic data to "put flesh on the archaeological skeleton."

The process theorist proposes a different procedure. Using the analogous ethnic group, he constructs a behavioral model to "predict" the pattern of archaeological debris left by such a group. This model is then tested against the actual archaeological traces of the prehistoric culture, with the result that a third body of data emerges, namely the differences between the *observed* and the *expected* archaeological pattern. These differences are in some ways analogous to the "residuals" left when the principal factors in a factor analysis have been run, and they may constitute unexpectedly critical data. When the archaeologist sets himself the task of *explaining* the differences between the observed archaeological pattern and the pattern predicted by the ethnographic model, he may come up with process data not obtained through the use of analogy alone.

Wiley is certainly alert to the current debate, and although he summarizes the New World in a predominantly culture-history framework, he concludes Volume I with a discussion of the hopes and promises of the process school. These he leaves for the future: "I shall be less concerned with process or a search for cultural 'laws,'" he says, "than with at times attempting to explain why certain cultural traditions developed, or failed to develop." Certainly the process school would argue that he cannot explain, within a culture-history framework, why such traditions developed or failed to develop; yet, as he explicitly states, explanation is not the purpose of this volume but rather history.

Let us hope, as Wiley seems to, that there is a place in American archaeology for both approaches. Certainly we can use both the historical synthesis and the detailed analysis of single processes. By no stretch of the imagination do all process theorists propose to reject history, because it is only in the unfolding of long sequences that some processes become visible.

In fact, what does the difference between the two schools really amount to? In terms of the philosophy of science, I believe the process approach results in moving "decisions" about cultural behavior even farther away from the individual. It is part of a trend toward de-

terminism that the culture historians began.

It was once common to hear human history explained in terms of "turning points," of crucial decisions made by "great men." This view proved unacceptable to the culture historians, with their normative framework of shared ideas, values and beliefs. They argued convincingly that this body of shared norms determined the course of history—not the individual, who was simply a product of his culture. Possibly the most devastating critique of the individual as decision-maker was due to Leslie A. White, who in one brilliant polemic concluded that the course of Egyptian history and monotheism would have been the same "even had Ikhnaton been a bag of sand."

Now the process school would like to move crucial decisions still 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 process-school archaeologists—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.

Obviously individuals *do* make decisions, but evidence of these individual decisions cannot be recovered by archaeologists. Accordingly it is more useful for the archaeologist 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.

But then if both historical and processual approaches are useful, why should there be a debate at all? I believe the debate exists because of two basically different attitudes toward science.

The previous generation of archaeologists, who did mostly culture history but also laid the foundations for the process school, were often deathly afraid of being wrong. Many of them felt (and many still feel) that if we will only wait until all the facts are in they will speak for

themselves. They spoke in awe of the incompleteness of the archaeological record and of the irresponsibility of speculating on scanty data. Somehow they seemed to feel that if they could get together a few more potsherds, a few more projectile points or a few more architectural details, their conclusions would be unshakable. There has not been, however, any convincing correlation between the quantities of data they amassed and the accuracy of their conclusions.

The process theorists assume that "truth" is just the best current hypothesis, and that *whatever* they believe now will ultimately be proved wrong, either within their lifetime or afterward. Their "theories" are not like children to them, and they suffer less trauma when the theories prove "wrong." Their concern is with presenting developmental models to be tested in the field, and they have noted no consistent relationship between the usefulness of a given model and the absolute quantity of data on which it is based. To be useful a model need only organize a body of disorganized data in such a way that hypotheses can conveniently be tested, accepted, modified or rejected. Thus the process school will continue to present model after model on the basis of returns from the first few precincts, and at least some of the culture historians will continue to accuse them of being "hasty," "premature" and "irresponsible." And the issue will be settled years from now by another generation that will probably not belong to either school.

Wiley's synthesis sums up nearly 100 years of American archaeology, and it comes at the start of one of the most exciting archaeological eras yet begun. My prediction for the next decade is that we shall see general systems theory, game theory and locational analysis all applied successfully to American archaeology in spite of the loudest mutterings of the establishment. I also predict that, in spite of his decision to concentrate his own efforts on producing reliable culture history, we shall hear all these subversive approaches applauded by Gordon Wiley.

Short Reviews

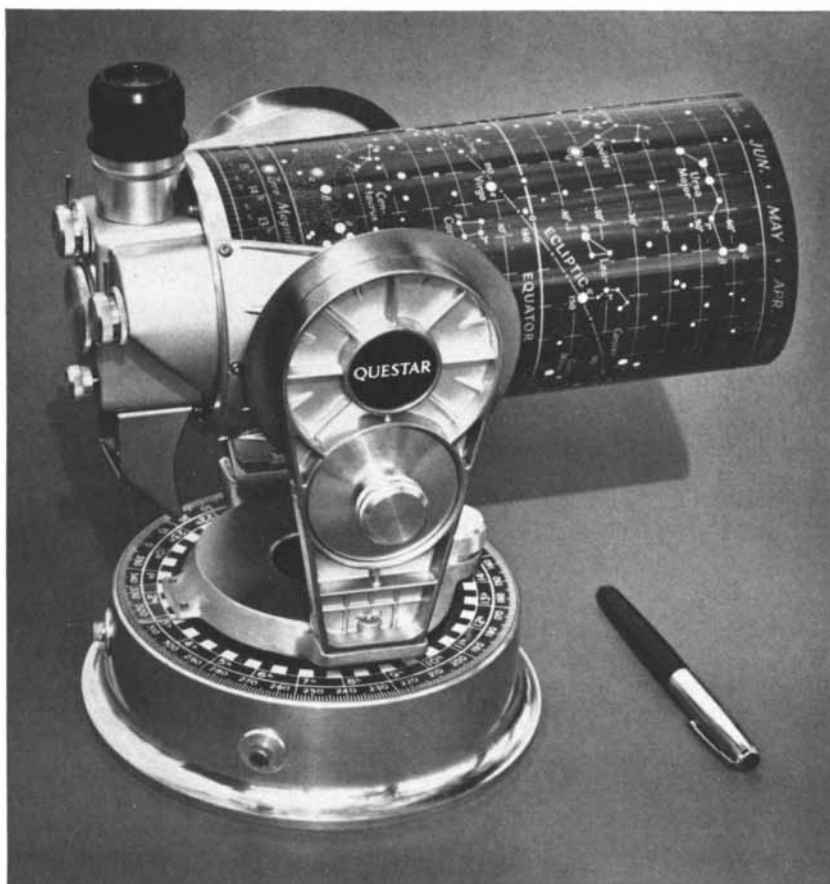
THE ENCYCLOPEDIA OF PHILOSOPHY. Paul Edwards, editor in chief. The Macmillan Company and The Free Press (\$219.50). When the *philosophes* of France assembled their great *Encyclopédie*, it undertook to encompass at least the beginnings of all knowledge. Accordingly its marvelous prints display all the engines and artifacts of the age.

The Encyclopedia of Philosophy is the dwindled and specialized work of our modern philosophers, some 500 of them, mainly American, with a company of British allies and a sprinkling from the corners of the earth. Most of the authors are professors of philosophy by vocation, but a few are distinguished amateurs such as Arthur Koestler and Martin Gardner. All the great universities house contributors, IBM claims one on the payroll, but what the expert on Arabic Aristotelianism does to earn his pay from the Pentagon is not made clear.

There are some 1,500 articles in the 4,000 large pages of these eight volumes. About 900 of the articles are listed by the names of individual thinkers from the history of philosophy up to the present day. Indeed, several of the contributors are the subjects of articles. The affinities of the contemporary profession are clear from the apportionment of space. Rousseau and Socrates get about five pages each, Whitehead 10, Hobbes and Hume 15 each. To dispose of the protean thought of Lord Russell takes 20 pages and three authors, who divide their task by topics.

The central nonbiographical burden of the work is the analysis of language. Witty and acute, if somehow picayune, article after article discusses just what one might mean under headings such as "Any and All," "If" and "Why." There are many excellent pieces on the foundations of mathematics and of logic, with a simple and clearheaded article on infinity and a deep one on Zeno and his paradoxes particularly catching the eye. More technical matters are also handled carefully, such as Craig's theorem on the completeness of axioms. Another large theme is theology, treated with a catholic interest in all eras and climes. There is, for example, an entry "Popular Arguments for the Existence of God," and a quite earnest piece on reincarnation.

The word is the beginning and the end of this work; there are amazingly few pictures, maps, diagrams or even formulas. Two articles on logic display a major fraction of the diagrams in the volumes. All this verbal argument lends a curiously vicarious quality to these books. The article on aesthetics is 50 pages mostly about visual impressions of beauty and has zero illustrations. There is evidence that this second-handedness leads to credulousness; extrasensory perception is regarded as a fact as plain as evolution, and even the ruined mind of Wilhelm Reich, the promoter of orgone, is not entirely dismissed. There are good pieces on scientific issues such as action at a distance, although they are only be-



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ginnings, and an outstanding one on quantum mechanics. A monument to its author, the late Norwood Russell Hanson, it brings to the reader something of the power that physicists know resides in that subtle and surprising theory.

Students of philosophy, in both the broad and the narrow sense, will find the book valuable, although perhaps not for a long time to come. It will nonetheless remain an honest and able report of a time of fashionable modesty. And it is impossible not to admire one volume whose spine displays these alphabetized contents: *Cabala to Entropy*.

FOUR-LEGGED AUSTRALIANS, by Bernhard Grzimek. Hill and Wang (\$7.95). This great conservationist and zooman, defender of Africa's Serengeti Plains, travels to Australia to report on what the two-legged inhabitants have preserved of their unique fauna. Market-hunting for kangaroo meat is a busy trade, but it does not in fact appear to threaten any species of kangaroo with the final solution; rather it is the loss of specialized habitat to road, ranch and suburb that brings extinction to many species. There are three genera of marsupials that have learned to behave like "animated kites": the pygmy glider, the sugar glider and the greater glider. They represent separate inventions, that is, each resembles the others less than it does some nonflying marsupial. The photographs of them shown here are endearing. There is also a shot—perhaps unique—of a wild dingo running free. His marsupial analogue, the Tasmanian wolf, "has reached the point of no return and the best of intentions will not save it now." Here Grzimek becomes angry. If this largest carnivorous marsupial dwelt somewhere nearer the centers of culture, he feels, "there would be a tremendous to-do"; in Tasmania it was attacked by trap and gun as a sheep-killer. Roads and people spoiled its natural cover, just as they did the cover of the wolf in England and Scotland. No Tasmanian wolf has been captured since 1933, although as recently as 1961 a couple of fishermen seem to have encountered one. There are probably a few on the wild western shore of the island, but they are doomed unless a range is set aside for them and stocked with game.

The case of the rabbit (and the less familiar and less dramatic case of the camel) present man-caused increases in the Australian fauna. There is a fine chapter on the rabbit, complete with the story of the artificial rabbit plague and the rabbit flea. It is a striking fact that the rabbit was a native of Spain and Mo-

rocco; everywhere else in the Old World he has been introduced. Rabbits came to England in the 12th century. There they paradoxically became a thoroughgoing pest once they were subjected to the steel trap; the trap all too frequently caught their predators, and the fast-breeding rabbits gained thereby. The control of rabbits by myxomatosis was a special case; the Australian rabbits are gaining an immunity even to the new strains of the pathogen. Airplane-sown poison bait and good fencing has controlled Tasmanian rabbits even though they have never been attacked with myxomatosis. Forty million wild rabbits are killed for skin and meat in Australia each year; there are plenty more on the range.

Grzimek comes away enthralled, saddened and hopeful: there are two-legged Australians who begin to care and scientists who are finding out how to make their concern count. If they, and not the heedless developers, succeed, the year 2000 will find Australian animals still visible outside of the postage stamps where today the Tasmanian wolf is memorialized. One hopes Grzimek is right; his book will help.

THE ORIGIN OF GENETICS: A MENDEL SOURCE BOOK, edited by Curt Stern and Eva R. Sherwood. W. H. Freeman and Company (\$2.25). **HERITAGE FROM MENDEL**, edited by R. Alexander Brink with the assistance of E. Derek Styles. The University of Wisconsin Press (\$2.95). **G. MENDEL MEMORIAL SYMPOSIUM 1865-1965**, edited by Milan Sosna and B. Sekla. Verlag Werner Flach (31 West German marks). Gregor Mendel planted peas in the experimental garden of the Augustinian monastery at Brno in eight cycles; his biggest plot was about seven meters by 35. He chose peas because he was studying traits of hybrids under conditions that allowed artificial crossing. He taught experimental physics and natural history at the Brno secondary school, for which he had a good university training in mathematics and in physical experiment. He published his work—for which he had found a remarkable explanation, particulate heredity and chance segregation—in a single paper in the journal of the Brünn Research Society. He published a second paper four years later, the year after he had been elected abbot. Then the choice of materials was not so happy; the flower he used is often parthenogenetic, and shows no response to an apparent cross. His work was repeated unknowingly by two other experimenters in the years before 1900, and in that year Carl Correns

of Germany and Hugo de Vries of the Netherlands independently found his results and his paper, and modern genetics began its triumphs. (The paper was by no means unknown in its time but was simply misunderstood and neglected by those who were aware of it.)

The first of these books is a reprint volume, bringing together the papers of Mendel, his letters to a contemporary scientist, the excerpt from the contemporary treatise that misunderstood his work, the papers of the rediscoverers and a wonderful controversy. The argument was raised by the statistician R. A. Fisher, who demonstrated that Mendel's data showed improbably small fluctuations and strongly suggested that he altered them to make his logical point. It would be a great joke on the world if that were true; however, Sewall Wright makes it pretty clear that what happened, and still happens in most genetic papers, was a slight unconscious selection in the course of the many subtle decisions one must make, in classifying and counting living materials, that tends to eliminate extreme cases.

The second work is a first-rate symposium celebrating genetics. The main sections, each including papers at a general level by four or five leading geneticists, center on the gene concept, the molecular basis of heredity, the action of the gene in the cell, the genetics of populations; finally, there is a fascinating set of papers on applied genetics and its history, plus papers by the always original Tracy M. Sonneborn and H. J. Muller. Indeed, the last word is had by the irrepressible and even eccentric Muller, who seeks to define life itself as a material with the potential for evolution by inherited variation and natural selection. This is perhaps Muller's last piece of writing (he died earlier this year), and it is well worth reading.

The third book contains the proceedings, both historical and biological, of a fine symposium held at Brno itself on the Mendel centennial. It is particularly valuable as a record of the present-day Russian estimate of Mendel and his work—an estimate much like the non-Russian one.

RADIO ASTRONOMY, by John D. Kraus, with a chapter by Martti E. Tiuri. McGraw-Hill Book Company (\$13.75). Thirty years ago the author, then a young radio engineer working with astronomers at the University of Michigan, tried without success to detect radiation from the sun at a wavelength of one centimeter. For 20 years he worked in the fundamentals of radio, becoming an

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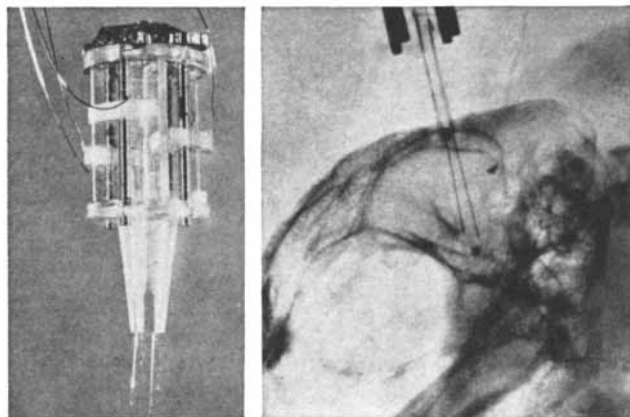
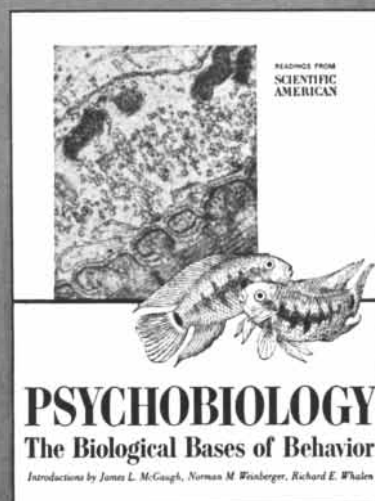
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outstanding designer of antennas and inventor of the helical microwave antenna. For a dozen years now he has been caught by radio astronomy; he is the designer of the Ohio State University radio telescope, a structure with a fixed vertical parabola looking at a tilting plane mirror across three acres of a ground plane defined by wires, and he directs its work. This is his text, aimed at seniors or early graduate students, and it is patently a useful, up-to-date, clear and pedagogically often brilliant introduction to the subject. It is plainly the book of an understanding engineer; about two-thirds of the text is devoted to fundamentals such as the elements of radiative transfer, polarization by Stokes parameters and matrixes, the theory of propagation in an ionized medium, the response of antennas and the many ingenious designs for squeezing signal and resolution out of noise and small scale. A chapter on receivers is done by a guest expert (Tiuri) at the level of block diagrams and noise-figure analysis. A good general chapter on coordinates and the usages of astronomy begins the work, and a lively overview of the appearance, nature and mechanisms of radio sources from the sun to quasars ends it—almost. The fact that it is a working text is made visible by the appendixes: the famous 3C list of radio sources is here, with three or four other useful tabulations; there is a graph for easy conversion between galactic and equatorial coordinates, and another one for correcting catalogued positions for the precession of the equinoxes. This is a book that will sit on the desk of just about every American student of radio astronomy, and the pirating of that splendid page which presents the outlines of 10 extragalactic radio sources on a common linear scale will be epidemic among popular and technical lecturers alike. Only for the theory of the sources and for a deeper astronomy is the text insufficient; there we still have nothing better than the Russian astronomer I. S. Shklovski's now somewhat dated book and the journals.

THE FOUR-COLOR PROBLEM, by Oystein Ore. Academic Press, Inc. (\$12). Francis Guthrie was a mathematics student of Augustus de Morgan at University College London in 1852; he dropped out, but first he showed his brother Frederick that "the greatest necessary number of colors to be used in coloring a map so as to avoid identity of color in lineally contiguous districts is four." Frederick Guthrie asked de Morgan to explain the result, and de Morgan wrote William Rowan Hamilton. Thus did this

famous problem enter the ken of proof; there seems to be no sure evidence concerning any earlier practical understanding, or concerning Leonhard Euler's reputed prescience in the matter. This monograph is a full and modern review, with all the methods, in their interrelation, that a century of effort has built for the solution of the still-refractory problem. The problem is part of the theory of plane graphs: figures consisting of a set of points in a plane and reasonable curves joining them. The earliest methods of this theory do go back to Euler; his famous formula using the number of faces, edges, vertexes and so on is a pillar of the work. Duality plays a role, as between cube graph and octahedron, and most of the work revolves around ingenious means of relating graphs one to the other, by various operations such as contracting and decomposing. The results are tantalizing. It is known that five colors will suffice for any graph; there is a certain conjecture which in a particular case would imply the truth of the four-color theorem that has been proved for a wide variety of cases but not for the correct one; finally, the best result to date, due to C. E. Winn 30 years ago, demonstrates that any graph that *cannot* be managed with only four colors must have at least 36 vertexes. The proof appears here, and it requires a rather tedious and inelegant enumeration of cases. Professor Ore clearly hopes his review will set a "younger generation" to work. The problem does not seem to be accessible to amateurs; this text is, however, much more self-contained than most modern monographs of mathematics.

PRINCIPLES OF STATISTICS, by M. G. Bulmer. The M.I.T. Press (\$7.50). A good college library is likely to give over 10 or 15 feet of shelving to elementary texts on statistics and applied probability. Here is a fresh text that stands out from that host. It achieves its graceful eminence by the breadth and interest of its prose and by the balance of its intellectual design. It never offends by the cookbook presentation of tables, of results and of complicated procedures that the reader is supposed to learn to use without much understanding, nor does it inhibit by the detailed and rigorous pursuit of mathematical results into depths where a user will rarely expect to dive. For instance, it derives the central-limit theorem via moment-generating functions with a few clear pages of text on the implications of the result. The troubles with those distributions whose moments diverge are mentioned, and the

more difficult task of using characteristic functions for the most general result is simply alluded to. This expresses very well the interests of most readers of such a text, who are sure to prize meaning above detailed technique, even above the maximizing of generality. Distributions that are not normal come in for genuine attention (although not enough time is spent on the lognormal case), and tests of significance and regression methods are given brief but quite usable explanations. One helpful chapter, informal and critical, is spent on inference, comparing the classical use of confidence intervals with more dubious methods and the modern decision methods out of games theory. This chapter is a convincing and commonsense account, kept at a surprisingly simple mathematical level (and rather skeptical in its conclusions). Some calculus alone will carry any thoughtful reader through this small, clear book, leaving him with both understanding and skill.

ON A PIECE OF CHALK, by Thomas Henry Huxley. Edited and with an introduction and notes by Loren Eiseley. Drawings by Rudolf Freund. Charles Scribner's Sons (\$4.95). Next year will mark the centennial of this luminous lecture delivered to the workmen of Norwich during a meeting of the British Association for the Advancement of Science. Good-humored and yet grave, deep and yet simple, this noble account of the span of geologic time and of the flux of life remains as eloquent as when it was new. In these pages one sees the Foraminifera of the chalk, flint tools from the boulder clay, the *Great Eastern*, and the sea bottom from Ireland to Newfoundland (not, alas, as level as the "prodigious plain" evoked by Huxley but broken by the massif of the Mid-Atlantic Ridge). These are among the dozens of subjects drawn with devotion to the truth and a quiet beauty by Rudolf Freund, in a way that would have pleased Huxley. Professor Eiseley writes a sensitive and elegiac appraisal of the man that would not have pleased him at all, shadowed as it is with pessimism and underlain by the very hunger Huxley thought he had helped men to assuage. A handsome reissue of a piece of prose that has fully earned the epithet "classic."

PRINCIPALS AND PRACTICES OF HEAVY CONSTRUCTION, by Ronald C. Smith. Prentice-Hall, Inc. (\$16). There are many city folk who find themselves watching the performance of builders with the attentiveness and pleasure of the concert-

goer. This is a work for them. It will serve equally well the many specialists, in blue collar and white, who play some role in the construction of large buildings. It is a sequential and detailed account of what all the people are doing, and what tools of the trade are used, as a great structure grows. The text is most specific in visible and easily grasped techniques; it contains no hint of the sophisticated means of computing stresses or testing subsoil. It tells about setting the batter boards for running the surveyor's wire lines, about blasting caps, about piling of every kind, about staying off high steel in the rain, about checking concrete, about calking aluminum sash. It has photographs of concrete vibrators, roof framing, metal fire doors, terrazzo machines and bulldozers. Practical and matter-of-fact, it is a once-over-closely story of how men build big. It is based on recent Canadian practice, and the final chapter, illustrated with photographs of coke-burning "salamanders," tells how and why to work through a bitterly cold winter. There is no account of the great whirligig cranes for putting up steel structures; perhaps they do not use them in Canada, as they did not in the U.S. until the past decade. This is an agreeable book; its varied lore, detailed in part although always rather superficial, will not suffice to design and build a building, but it is a genre all its own.

THE LANGUAGE OF NATURE: AN ESSAY IN THE PHILOSOPHY OF SCIENCE, by David Hawkins. Doubleday & Company, Inc. (\$1.95). A paperback reprint of a handsome book that thoughtfully seeks an ambitious unity of viewpoint (mainly on the basis of information theory) toward number and motion, chance and order, acting and knowing—all the way from the axioms of Giuseppe Peano to the theory of personal and social moral choice.

CAPITAL: A CRITIQUE OF POLITICAL ECONOMY, by Karl Marx. Translated from the third German edition by Samuel Moore and Edward Aveling. Edited by Frederick Engels. International Publishers Co., Inc. (\$9.95). A three-volume paperback facsimile of an 1887 translation, with the corrections made by a Moscow publisher in 1965.

ON THE ORIGIN OF SPECIES, by Charles Darwin. A facsimile of the first edition, with an introduction by Ernst Mayr. Atheneum Publishers (\$3.45). A paperback reprint of the 1964 hardback. Admirable.

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