

BOOK REVIEWS

Land, Food and Work in East Java. By Wade C. Edmundson. New England Monographs in Geography, No. 4. Department of Geography, University of New England, Armidale, N.S.W., Australia, 1976, vi + 115 pp., photographs, tables, appendices, bibliography, A\$6.00 (paper).

It is quite unlikely that many have heard of the New England Monographs in Geography, at least outside of Australia. The present contribution is only the fourth, and the paper-bound, mimeograph format indicates a low-budget limited circulation enterprise. Yet, if *Land, Food and Work in East Java* is any indication of what has gone before and what is to come, subscriptions should be on the increase in the near future.

Edmundson's basic concern is with documenting the relationships among wealth, nutrition, and work in order to test the widely held belief in development circles "that low levels of caloric intake result in low levels of human performance" (p. 1). This is a quite straightforward problem as stated, but it brought the author face to face with two of the most difficult areas of human behavior to measure: diet and activity patterns. Both have escaped accurate and easy quantification, and there are no widely agreed upon field methods. Given this situation, Edmundson decided to concentrate on a relatively small sample, 18 adult males from each of three villages, and to obtain as much data from their food intake and activities as possible. Diet was determined by weighing home consumed meals for six consecutive days at 2 month intervals during the year and estimating the weights of foods eaten away from home during these same periods. Fortunately, help was available from Javanese health officials, who provided teams of two researchers each, one for weighing each meal's ingredients and the other for observing the subject outside the home. Activities were measured by portable respirometer to determine oxygen calorimetry. Standardized values were obtained for lying, squatting, sitting, standing, walking, carrying, hoeing, and a variety of other activities categorized as light, medium, or heavy work. Each subject was then followed during his waking hours in order to classify his daily rounds. All the various procedures and problems of these recording methods are clearly discussed in Chapter 2, which also includes a

valuable discussion of how to draw a sample population when textbook circumstances do not prevail.

Chapter 3 follows with an overview of the three study villages. One is located in an alluvial valley with wet rice the subsistence base, the second is supported primarily by maize cultivation on upland volcanic soils, and the third is situated on a limestone plateau and relies on a combination of maize and cassava. Edmundson mixes a general narrative designed to give a "feel" for each place with specific facts and figures on environment, economy, and society with effective results. Missing, however, are maps that would have helped in establishing even more effective images. I suspect that costs were a primary factor in their omission.

The fourth and final chapter deals with the data. Using Pearsonian Coefficients of Correlation, Edmundson finds size of land holdings a good index of wealth, but he then observes no correlation of this with caloric intake. In other words, wealth seems to be unrelated to diet. This is an unexpected finding, and Edmundson sets forth six possible relationships to explain it. First, cultural preferences outweigh economic status when it comes to food because rich and poor have similar tastes. Second, there are enough calories available to meet basic needs, at least for the adult males who are given special preference. Third, the poorer work harder and therefore consume more food than the wealthier members of the community. Fourth, the wealthier families tend to have more dependents to feed. Fifth, a variety of alternative activities are available for those who are land-poor so that they can earn enough for minimal subsistence. And sixth, social obligation and reciprocity act to redistribute food throughout the village.

Edmundson continues to explode the conventional wisdom by finding no relationship between caloric intake and work output. All 54 subjects expended about 2400 calories per day, but varying consumption led to a range in balances from a deficit of 918 calories to a surplus of 1552 calories. The former person should have wasted away over the year and the latter become obese, but no significant weight changes were observed. The only explanation offered is the likelihood of individual differences in energy efficiency, with some needing more and others less to support the same level of activity.

Several other worthwhile findings emerged in addition to the two primary ones. One is the seemingly adequate nutritional status of the villagers. Edmundson found few signs of malnutrition, the main exception being among the elderly. He notes that past age 45 both males and females are no longer fit for heavy field work, and they age rapidly. The reasons for this deterioration are not discussed, but two possible explanations come to mind. It could be that the nutritional state of adults is not as adequate as it seems and, when the stresses of old age begin, the body is less able to cope with them. More likely is that there is a cultural adjustment to a limited food supply. Edmundson does point

out that individuals reaching their mid-40s begin eating less and less, thus possibly helping to ensure an adequate food supply for those in their most productive years.

Edmundson also notes the considerable work activity of the adult males, belying the stereotype of the "lazy" Javanese peasant. About 26% of the time involved productive work, amounting to over 40 hours per week. People are not underemployed, rather "they are simply unproductive in the sense of having too much inefficient work done by too many people" (p. 82). Surprisingly, the sample laborers from the wet rice village worked a little less than those from the other two.

In his discussion of energy expenditure, Edmundson lapses into a rare moment of carelessness. He claims that Javanese peasants are likely to be more fit than Westerners, or at least energetically more efficient. But his comparison group was 10 college students from the United States and Australia who happened to visit the area, hardly an appropriate one against which to measure physically active peasant males.

All in all, this is a valuable and rewarding book. The problem is an important one from the standpoint of nutrition policy and development, and Edmundson does his best to bring hard data to its resolution. Also, as in most good studies, more questions are raised than answered.

James L. Newman
Department of Geography
Syracuse University
Syracuse, New York

Human Ecology in the Tropics (2nd edition). Edited by J. P. Garlick and R. W. J. Keay. Symposia of the Society for the Study of Human Biology, Volume 16. Halsted Press, New York, 1977, vii + 200 pp., figures, chapter references, indexes, \$12.00 (cloth).

In recent years it appears to have become obligatory to publish the proceedings of symposia. The present volume goes one step further; it presents a second edition of proceedings originally published in 1969. The first edition arose from a 1966 symposium on human ecology in the tropics organized jointly by the British Ecological Society and the Society for the Study of Human Biology. Changes in the second edition comprise the addition of three new papers to the eight originally published. Unfortunately there is no evidence in these additions of the results of research in population biology and plant/animal interactions that has so transformed tropical ecology in the last decade.

The subject matter treated in the papers is a mixed bag. Two may be called anthropological, four treat agricultural and forestry topics, and the remainder are medical. Throughout, the emphasis is heavily upon the African tropics. The formats of the papers also vary. Some are case studies, others are review papers, and a few are no more than brief comments. Matters are worsened by the lack of adherence of the topics of most papers to the symposium's title. (Wright's paper on the ecology of African schistosomiasis is a notable exception.) While such a varied mix can be stimulating in the proceedings of a large symposium, in such a slim volume as this one the treatment provided can only be described as uneven. I would have hoped for greater care in the selection of a balanced blend of topics in order to bridge a gap that seems to exist in writings on tropical human ecology. Most of this literature seems to fit naturally into one of two classes. The first are those generalizations about the tropics as a habitat for man that are so often treated as to be clichés. The second are detached case studies. What is so often missing, and what I had hoped to find here, are papers spanning the gap by evaluating old generalizations in the light of recent case studies. Unfortunately, the dichotomy has been preserved.

The lack of firm organization is reflected in other ways. Editorial control appears to have been nonexistent. Some papers are grossly padded: that by Moss and Morgan on the savanna/forest transition in Nigeria contains no less than seven pages of plant species lists that will be virtually unintelligible to anyone save a tropical taxonomist. Other papers are repetitive and lack clarity: I found the one by Turton on the response to drought by the Mursi of Ethiopia especially tedious. Not even the bibliographies are in a standard format. I find such lax editing to be annoying in first editions and inexcusable in second editions.

Despite these criticisms, the volume does contain some good papers, mostly on medical topics. Ingram provides a useful summary of the physiological reactions to heat in man, a topic that he has treated in great depth in a recent volume published jointly with Mount. Thompson provides a summary of findings by a medical/sociological team on morbidity and mortality among children in a Gambian village. This represents an excellent piece of inductive tropical field work. Wright's paper on schistosomiasis neatly spans the medical and sociological dimensions of the problem. Ford's paper on the epidemiology of trypanosomiasis provides no clear conclusions but illustrates the complexities that so often bedevil tropical research.

Despite these highlights, I remain unconvinced that proceedings of this symposium justified publication, let alone a second edition. While the few good papers represent sound scholarship, the majority of papers would not, in my opinion, have passed a critical journal reviewer. Synthetic treatments of the ecology of man in the tropics are certainly needed, but, with the few exceptions cited, these have not been provided in this volume. This is not to say that the contributions may not have been of value within the context of the original

symposium. However, not all symposia produce publishable material, and I wish that more editors would bear this in mind.

Martin Kellman
Department of Geography
York University
Downsview, Ontario
Canada

Where Hunters Gathered: a Study of Holocene Stone Age People in the Eastern

Cape. By H. J. Deacon. South African Archaeological Society, Cape Town, Monograph Series No. 1, 1976, xiii + 232 pp., illustrations, bibliography, appended tables, R7.50 (paper).

The prehistory of southern Africa was long viewed as an uneventful chapter in the saga of a turbulent continent, where most biological evolution and cultural innovation were first centered in the Rift and then in the Nile Valley. In part, this impression was due to the seemingly homogeneous post-Pleistocene lithic industries, and the quaintly conservative culture of the Khoisan speakers reported by travelers of the 17th to 19th centuries. In part, too, it was a reflection of ignorance and imperfect understanding.

Where Hunters Gathered brings together much of H. J. Deacon's research on the Later Stone Age (LSA) in a sector of the eastern Cape Province. Although organized as a report on two sites, Melkhoutboom and Highlands, the volume strives to generate models of prehistoric adaptations during the Pleistocene-Holocene transition ca. 14,500-7500 BP as well as the environmentally less eventful millenia that followed. Deacon's data base is provided by meticulous excavations and detailed study and evaluation of the rich paleobotanical record at his sites, complemented by Klein's (1977) essential study of an unprecedented number of faunal assemblages from almost every prehistoric sequence in the Cape Province.

Melkhoutboom is an open quartzite cavern in the irregular terrain of the Cape Folded Ranges, 650 m above sea level and 60 km from the Indian Ocean. The setting is a vegetation mosaic, with evergreen forest in the adjacent ravine, bush savanna downstream, sclerophyllous vegetation on the higher mountain slopes, and grass savanna on the upland ridges. Some 14 m² of the potential cave living area of 150 m² were excavated to a maximum depth of 1.95 m (bed-rock). Occupation began ca. 15,500 BP and may have terminated as late as 250 BP, based on eight Carbon 14 dates. Except for a 30 cm layer of crude rock rubble, probably dating 14,500-10,500 BP, the strata are preeminently cultural

in origin, with little or no mineral accumulation apparent at times of cave abandonment.

The first millenium of habitation at Melkhoutboom is represented by the Robberg industry, made primarily on quartzite (44%) and silcrete (17%), including a strong micro-bladelet component, produced from high-backed cores and presumably intended for hafting of stone bits into composite tools. Medium to large grazers, namely red hartbeest and a zebine (quagga?), are the key faunal elements, while marine shell is absent. Plant preservation here and in the overlying cultural level is limited to carbonized seeds that were not studied systematically, but that are consistent with those of the Holocene levels, with the possibly significant exception that seeds of *Podocarpus*, a modern forest dominant, are absent. Occupation 10,500-7500 BP pertains to the Albany industry, which favored quartzite (70%, with less than 5% silcrete and chalcedony) and here consists chiefly of flakes. Medium-sized alcelaphine antelopes and quagga were the chief game, and marine shell is present throughout.

From 7500 BP to the 18th century the cave occupants produced varieties of the Wilton industry, characterized by microlithic convex scrapers and segments (crescents), backed bladelets, adzes and borers, some with examples of attached mastic, and all attributed to composite tools. Raw materials initially included 20-40% chalcedony and 9% silcrete but, following an occupation break ca. 5500-3000 BP, quartzite frequencies increased abruptly at the same time that marine shell decreased, freshwater mussel increased, and storage pits became a feature in the cave. The Wilton levels not only include rare manos, abundant wooden tools, bone implements, and ostrich eggshell ornaments, but also fiber cordage, netting, and matting, as well as leather fragments. The plant residues show that the rootstocks, corms, and bulbs of monocot geophytes (growing in the sclerophyllous and grassy areas above the cave) were staple foods; these would have been most visible or palatable in spring and early summer. Although the popular *Watsonia* corms are low in protein, seeds are present and there was subsidiary hunting of nocturnal, solitary, small browsing antelopes (by snares?) as well as collecting of tortoise and freshwater crab. Deacon further postulates a seasonal pattern of "transhumance," with winter coastal settlements focused on fish and mollusks.

The rich Wilton record suggests stable technological and subsistence sub-systems over 7 millenia, with changes in size and form of specific tool classes interpreted in stylistic rather than functional terms. Deacon prefers a homeostatic model for the unspecialized Wilton adaptation: Shifting modes in scraper production are considered as minor adjustments that maintain the system in relative constancy. The earlier Robberg and Albany adaptations are also considered stable, but they were of a different kind: These late Pleistocene to early Holocene populations were more mobile, more dispersed, and less "territorial," whereas the Wilton people knew the precise location of a broad selection of resources in a mosaic environment, controlled a successful exploitation strategy, and were

thus permitted a greater population density, a closer nesting of population cells, a more sedentary existence, and more fixed territories.

The excavation at Highlands complements the spatial perspective. This is an 18 m² rock overhang and ledge on the side of a residual hill ca. 1300 m elevation, at the periphery of the semiarid South African interior and 210 km from the coast. A disconformity of at least 25 millenia separates weathered, older strata (with unreported Middle Stone Age) from a LSA deposit recording most of the last 4500 years. An area of 6.7 m² was excavated to a depth of 90 cm, of which up to 65 cm pertain to the LSA ("Smithfield"). The artifacts, generally made on metamorphosed shale, are dominated by unmodified flakes, with formal, shaped tools well represented by hafted tool bits, scrapers, adzes, backed pieces, and borers; bone and wood points, shell ornaments and rare manos are also present, together with a single piece of leather and some trade beads in the topmost level. Ostrich eggshell is at least 10 times as frequent, relative to unmodified flakes, as at Melkhoutboom, suggesting an important food resource. The most common faunal remains include two small antelopes – the springbok, a plains grazer, and the steenbok, a browser – as well as rock hyrax, hares, and tortoise. Plant remains include a rootstock and, above all, bulbs of *Cyperus usitatus* that imply summer occupation. Larger game and freshwater mussels are rare, but may have been exploited in lowland microhabitats. Highlands is attributed to short-term summer occupation by small groups representing a single socioeconomic system through time. Although the lithics suggest derivation from a different cultural tradition than Melkhoutboom, and even though different species were utilized (or the same species utilized in different frequencies), the basic subsistence pattern appears to have been much the same at both sites.

Deacon views his Highlands excavation as a pilot that helps define technological and subsistence contrasts and similarities between the Wilton, of the mesic coastal and montane environments, and the "Smithfield," of a marginal interior setting that was largely unpopulated for several millenia prior to 4500 BP. The Wilton crescents and the "Smithfield" backed points and segmented back bladelets represent a significant differences in formal, hafted tool design and preferences that Deacon considers to be stylistic. The Wilton and "Smithfield" corresponded to major ecological divisions and may reflect different linguistic groupings. In concluding, Deacon presents and discusses a model for relating the hierarchical levels of archeological data (industry, phase, occurrence) to social units (linguistic, dialectic, residential, subsistence) within the geographical context of the Eastern Cape.

Whatever its shortcomings, *Where Hunters Gathered* is an invaluable study. Some less than exemplary aspects must, however, be mentioned. For example, in evaluating the faunal evidence Deacon ignores Klein's sound case for several megafaunal extinctions (*Equus capensis*, a large zebra; *Pelorovis*, a giant buffalo; *Megalotragus*, a giant alcelaphine) in the Cape Province during the Pleistocene-

Holocene transition. Although these animals are not recorded at Melkhoutboom, Klein has made a reasonable case that selective hunting pressures within changing habitat mosaics may have had significant impact on the demography of several key game species. Furthermore, the botanical analyses lack information on possible pollen and opal phytoliths while systematic flotation studies were not undertaken. This may conceivably result in a one-sided assessment of the relative composition of gathered plant resources. The other botanical data, unparalleled in archeological excavations almost anywhere, nevertheless do not allow estimates of the ratio of vegetable to animal food components. Altogether this implies that Deacon's subsistence conclusions are, statistically viewed, qualitative, or, at best, semiquantitative.

No sedimentological study accompanied Deacon's excavations, and even the basic geological descriptions are inadequate and inconsistent (including a basal, roof spall horizon in Melkhoutboom, Fig. 9, not mentioned in the text but negated by the description on p. 24). The fact that deposits are mainly of cultural origin has not precluded significant lithostratigraphic deductions in numerous other cave sequences, including South African sites. Deacon attempts to consider the role of environmental change, but he does so reluctantly, admitting a change from primarily open to more forested environments, accompanied by marine transgression across the coastal shelf, during the Pleistocene-Holocene transition. He assumes that mosaic environments exploited by unspecialized, broad-spectrum subsistence techniques are unsusceptible to minor ecological shifts. This is fallacious, since geophyte productivity is partly related to climate, and animal communities are influenced by the expansion or contraction of bush in mosaic settings. Such short- or long-term changes should eventually affect the ratio of site ("local community") size to effective catchment, with demographic or territorial implications.

The Carbon 14 framework at Melkhoutboom suggests rapid sedimentation rates of 30-60 cm/1000 yr during intervals of repeated, periodic occupation, and a few more Carbon 14 dates would certainly serve to demonstrate long periods of cave abandonment (as much as 500 years prior to 6000 BP and 1500 years prior to 3000 BP) – a matter never considered by Deacon. It may well be relevant that a paleosol and a period of temporary forest advance in the southeastern Cape have dates of 7030 and 6870 BP, subsequent to which coastal vegetation was more open and slopes were unstable; truly mesic conditions can be dated ca. 4200-1000 BP, with recurrent geomorphic instability thereafter (Butzer and Helgren, 1972). The "minor adjustments" between "Formative," "Developed," and "Post-Climax" Wilton coincide with the major sedimentary breaks at Melkhoutboom; they also happen to match the landscape shifts after 6870 and before 4200 BP, while slope disequilibrium since 1000 BP coincides with the appearance of herding and pottery in the area. One may therefore wonder whether Deacon has not overemphasized stability and downplayed the possibility of ecological readjustments during the Holocene.

These reservations do not deprecate the quality of a unique study, since it is in the nature of archeological research that optimal results are never quite achieved. The very fact that Deacon can arrive at so many well argued conclusions demonstrates the quality and detail of his data. *Where Hunters Gathered* is the most thoughtful and thought-provoking analysis of its kind to deal with sub-Saharan Africa in almost a decade.

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Karl W. Butzer
*Departments of Anthropology
 and Geography
 The University of Chicago
 Chicago, Illinois*

The Legacy of Malthus: The Social Costs of the New Scientific Racism. By Allan Chase. Alfred A. Knopf, New York, 1977, xxvii + 686 pp., figures, chapter notes, glossary, references, index, \$17.95 (cloth).

It is always pleasant to review the misconceptions of the past from the standpoint of our own "modern and enlightened" age; it is less pleasant to discover that the misconceptions persist in virulent form. It's rather like casually reading about plague in the Middle Ages, then discovering that plague is on the increase in one's home town. Comfortable illusions of superiority are shattered, and this is Allan Chase's intention in tracing the genealogies of the founding fathers of eugenics — Malthus and Galton — through to their present-day adherents, such as Jensen and Eysenck.

In *The Legacy of Malthus* Chase discusses the illusions that Americans maintain about improvements in human well-being over the last two centuries. Medical advances have benefited the middle and upper classes, but these advances have not always been available to the poor. This lack, the author says, is largely due to the contrivances of those who embrace Malthusian doctrines about poverty. Chase begins with the notions propounded by Malthus — that population increases must inevitably overtake food production, that the poor must remain poor to serve as a stimulus to production — and continues to the eugenic laws propounded by Galton — that the poor should be prevented from breeding if the species is not to be degraded. Chase then brings his analysis of this curious

dogma down to current legislation and court decisions dealing with forced sterilization, IQ test scores, and EEG readings.

Blaming the poor for poverty is not a new pastime, nor is the logic that underlies the pseudoscientific claims in any way abstruse. Indeed, the simplicity of the argument must be part of its long-standing appeal. If A equals B and B equals C, then A must equal C, or, in 19th century terms, if the Irish are poor and poverty is correlated with "inherited diseases," then being Irish must mean having inferior genes. In 20th century America, if low IQ scores are correlated with poverty and poverty is correlated with skin color, then low IQ scores must be correlated with skin color. Or, if pellagra is associated with poverty, and poverty is associated with a particular ethnic group, then pellagra and ethnic groups must be associated in some fixed way, not subject to the efforts of true science or to the betterment of the diet or living conditions.

A fine example is given in Chase's ninth chapter, in which he documents the discovery of the cause of pellagra. Pellagra was long held to be an hereditary affliction, until its cause was discovered in 1914, when the disease was produced by feeding prisoners a high carbohydrate diet with no protein or green vegetables. Despite this and other proofs, pellagra continued to be thought of as hereditary (mainly occurring among poor White Southerners) and was not eradicated until federal work and food relief programs were introduced in 1933.

If the logic of the eugenicists' argument is poor, its premises are worse. A major premise, one that recurs in article titles, is that the dreaded effects of democracy must be mitigated, that the "dark-skinned untrammelled copulators" (p. 514) must be stopped before the "good genes" of the rich are overwhelmed. Minor premises are that genes are unit characters (one gene = one trait), and that some genes preclude certain kinds of development. Armed with such hazy dogma and a determination to demonstrate the superiority of the "Nordics," the eugenicists could deal with discoveries that seemingly refuted their ideas. When it was found that Blacks had faster reflexes than Whites, the genes for reflexes were said to prevent normal brain development (p. 230), and, under the same premises, Jews were considered to be too "intelligent" to find places in American society (p. 277). Genes do not work this way, and it is unfortunate for the poor that legislators can be misled into believing that they do, with such results as the U.S. Immigration Act of 1924 (p. 289).

This kind of misguided attempt to show the "genetic" basis of poverty is the true legacy of Malthus. Galton and Malthus were not racists, except in the broadest sense of the term. They believed that there were two kinds of people in the world, those whom Spencer called the "deserving rich and the undeserving poor," and that the rich were of superior genetic make-up. The poor inherited all sorts of afflictions, just as the rich inherited careers in banking. The addition of skin color to these premises came later, but even while most of the data used as proof were discredited (head form by Boas, p. 181; IQ scores by Binet and Simon, p. 36), the philosophy has shown a remarkable staying

power. Science may proceed slowly, but pseudoscientific findings, unhampered by facts, can proceed apace, arriving at 20th-century conclusions that the 18th century would have applauded. Malthus's conclusions about the futility of the poor laws are rewritten in modern terms that deal with the "ineducability" of the poor (p. 273).

Chase refers to the dogma of scientific racism as the "great counter-humanist tradition," and the phrase is well taken. What is most frightening about the scientific racists is their conviction that equality of opportunity must be legislated against: "the error of human equality" is a recurring phrase (p. 179). The success of the scientific racists in undermining legislative attempts to help the poor or the unfortunate has included immigration restrictions and cutbacks of food supplement programs. The efforts of the scientific racists may be clumsy or ludicrous — as when they attempt to prove that various sages must have been White men, that Jesus Christ was not a Jew (p. 172), that Confucius was not Chinese (p. 176) — but their intentions are not funny, and neither are the effects of their beliefs.

There were things about this book I did not like. It is too long, it is occasionally repetitious, and there are moments when refutation gives way to polemics. But these are unimportant quibbles. Chase, a professional writer, has done the scholarly community a good turn. The book will serve as a fine reference text for courses on demography, ecology, growth and development studies, and many others. I would like to see its message and (only some of) its data reproduced in a series of magazine articles more accessible to the general public, lest the message — as happened with the finding that Black school children in an affluent Los Angeles neighborhood scored higher on IQ tests than any other group in the public school system — be lost.

Eugenia Shanklin
Department of Sociology-Anthropology
Trenton State College
Trenton, New Jersey

Culture and Practical Reason. By Marshall Sahlins. The University of Chicago Press, Chicago, 1976, xi + 252 pp., illustrations, bibliography, \$17.50 (cloth).

Sahlins' aim here is to "liberate anthropology from the prison house of naturalism" (p. 102). His plan is to criticize the various theories that portray the forms of human culture as the outcomes of utility-optimizing interactions between people and the objective noncultural world. After criticizing these theories of practical reason, Sahlins argues that symbolic or meaningful reason structures

our behavior. He asserts that the former theories ultimately reduce human affairs to biological survival, whereas the latter portrays us as an organism uniquely endowed with the capacity to impose meaningful schemes upon natural instruments. Thus the issue that Sahlins raises is potentially crucial for human ecology. Can it develop by the analytical, reductionist methods of the natural sciences and by grounding the social sciences in biology and physics? Or will understanding ultimately come from relegating the natural sciences to a subordinate role while attending to the distinctive qualities of minds creating symbolic order?

The first three chapters of the book examine the work of a series of theorists in light of the distinction between practical and meaningful reason. Chapter 1 treats British structuralism, French structuralism, and the Marxist reaction to both. The unsatisfactory provisional conclusion drawn by Sahlins from this chapter is that two theories are apparently required: (1) French structuralism for tribal cultures, and (2) praxis theory (to some extent Marxism and British structuralism are both praxis theories) for modern bourgeois ones.

The second chapter deals with the historical development of theory in the social sciences by examining the work of influential anthropologists including Morgan, Boas, Malinowski, Murdock, Steward, and Leslie White. In assessing the debates among these theorists, Sahlins sees a "repetitive and cyclical opposition" between practical and symbolic reasoning, between those who would keep culture and mind and those who cast them out in favor of ecological adaptation. White is the exception in whose work the two paradigms coexist in unresolved opposition.

The third chapter explores the interplay of culture and practical reason in Marxist thought. According to Sahlins, Marx neither fell prey to the error of vulgar naturalism nor developed a fully symbolic paradigm. But while seeking an explanation for the ultimate source of value, Marx was compelled to naturalize the cultural determination of production in order to have a material prime cause of historical processes.

The fourth chapter argues that practical reason is a special bourgeois myth developed by and appropriate for our peculiar kind of economizing society. Western food and dress customs are cited as examples of the way in which symbolic reason in fact preempts practical economizing, even in America; for Sahlins, eating beef but not horsemeat and dressing up for visits to the city are evidence of a cultural system independent of practical reason. Ultimately, he believes that we must turn to cultural reason to understand the generation of the values that become objectified in bourgeois culture as exchange value.

Sahlins presents his theory of culture in the concluding chapter: The symbolic cultural order is primary, no specific cultural form can be derived from material circumstances, and cultures differ by the institutions which figure most prominently in their systems of cultural categories. Tribal societies have kinship as the primary locus of symbolic differentiation, whereas in "hot" modern societies the locus is the relationship of individuals to production. Practical reason,

or economizing in the face of nature, is for Sahlins a symbolic cultural logic; we see the world as a locus for optimizing individual advantage only because we constructed such a world in the first place. For us, as well as for other societies, the free play of cultural reason invents the ultimate values for which people organize their use of natural instruments, subject only to the minimum ecological constraint of physical persistence.

As an abstract exercise in formulating a plausible link between French structuralism and Marxism, *Culture and Practical Reason* is a clever piece of argument. But does it effectively exclude the possibility that individual or collective economizing behavior is an important determinant of cultural form? Absolutely not, and the difficulties with Sahlins' position are manifold. Most important is that Sahlins is very vague about what he means by cultural reason, a serious shortcoming given the uncompromising position he takes with regard to the importance of ecological adaptation. He develops no model of the structure-generating process, and the essential categories of "mind" and "meaning," fraught as they are with unresolved philosophical debates, are virtually undefined and unexamined here. Sahlins likewise uses "culture" without a hint of the conceptual complexities surrounding this term. Accordingly, we cannot devise tests to determine whether or not Sahlins' cultural logic is an adequate scientific hypothesis because the idea, as it stands, is not sufficiently rich in propositions with empirical referents to be useful. Not really knowing what cultural logic is, we cannot imagine how it might have arisen from mere animal existence, or just how it interacts with the material world.

This objection is not to say that a genuine hypothesis cannot be developed from Sahlins' rudimentary concepts, but only that he fails to do so. Indeed, a very simple one can be constructed. The key to cultural reason is the relative arbitrariness of symbolic forms. This feature is common to all code systems, from DNA to human languages. Since referents to material reality in such systems are conventional, and since the grammars of many complex codes permit a very large number of messages, the code can evolve arbitrarily so long as conventional meanings are maintained. Synchronically, the same pattern holds. An industrial system of production can apparently be equally well organized using a wide variety of languages as the media of expression. Indeed, where secret or semi-secret messages, such as the etiquette signals that signify membership in a privileged elite, are to be communicated, the message must evolve faster than outsiders can learn it. This kind of hypothesis is admirably formalized by Abner Cohen (1974) to apply to just those sorts of nonlinguistic symbolic phenomena of complex societies that Sahlins views as requiring cultural logic for explanation. Cohen's theory involves an intimate dialectic between the symbolic and practical orders. From this perspective, Sahlins' cultural determinism is just one extreme along a continuum of potentially logically consistent hypotheses which range to the opposite extreme of complete ecological determinism. Cultural and adaptational causality may interact in complex ways. Further, insofar as the

same meaning can be communicated by an arbitrarily large number of symbolic systems, the rigid opposition between practical and cultural logic disappears and the two kinds of phenomena become weakly interacting, and therefore partly independent, intellectual problems.

It is clear that Sahlins' empirical examples of Eastern Fijian dualism (in Chapter 1) and American food and dress customs will fit the weak-interaction hypothesis. Structural metaphors, like language, have arbitrary conventional meanings. In changed circumstances, a group of people can change the referent of a symbol, while keeping the system of symbols intact, or invent a minor addition to cover the new case. To show that cultural reason controls behavior with respect to material reality, Sahlins would have to show how structural logic strongly constrains such behavior, which he does not do. Further, in order to make his theory in the least plausible, Sahlins must postulate an extreme ecological possibilism (p. 209) in order to give cultural logic force over practical exigencies. His idea that adaptations are "minimum positive functioning," a wholly erroneous interpretation of the underlying Darwinian theory, is required to sustain this view.

The rhetorical force in Sahlins' argument stems from two sources. The first is his separation of cultural and practical reason into categorical alternatives. This treatment is almost certain to erect paradoxical polarities which seem insoluble. This approach seems counterproductive, as the success of modern mathematics and natural science stems from their reformulation of philosophical paradoxes into linkage and continuum (Rapoport, 1967). The second is the historical approach to his critique. This permits Sahlins to let Marx, Malinowski, and Steward serve as foils for his argument, rather than his more sophisticated contemporaries like Cohen. In any case, scientific explanations are always more or less incomplete, nowhere more so than in the social sciences. Human ecologists have every reason to take seriously Sahlins' critique of naive adaptation theories, but no reason to accept his alternative explanation.

Part of the problem is epistemological. Sahlins (1972:xiii; 1977) is skeptical of conventional scientific modes of explanation and believes that scientific theory can be substantially (irredeemably?) contaminated by the imperatives of a given cultural order. But if this opinion is correct, how can any attempt to understand the human condition be more than merely a relative account? If we have a choice between modes of understanding, how do we choose? Although these problems are raised explicitly against theories of practical reason by suggesting that these are bourgeois myths, Sahlins abandons any close adherence to the methods of positivistic science without sketching an alternative. Positivistic science may eventually be proven a particular Western cultural logic, but it has succeeded dramatically in providing internally consistent explanations of previously mysterious natural phenomena. Given the existing state of human enlightenment, we cannot be sure that scientific explanations of cultural pheno-

mena will be as satisfactory, but I certainly believe the attempt is worthwhile pending a much more convincing demonstration of an alternative mode of understanding.

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Peter J. Richerson
Division of Environmental Studies
University of California
Davis, California

The Use and Abuse of Biology: An Anthropological Critique of Sociobiology.
 By Marshall Sahlins. The University of Michigan Press, Ann Arbor, 1976, xv+120 pp., bibliography, \$8.00 (cloth), \$3.95 (paper).

The split between the "two cultures" did not begin, as C. P. Snow would have us believe (1963), in the world of his own youth at the beginning of the present century but in the opposition posed by the Romantic movement to the Enlightenment over a hundred years earlier. However, as Lord Snow records, the two cultures were still speaking to each other at the time of World War I. Now, it would seem, we have arrived at the point where, if they converse at all, they are talking *at* rather than *with* each other, and genuine communication is severely reduced at best.

The different reactions to a single word, "sociobiology," trenchantly illustrate the state of the current impasse. This is interpreted to mean everything from a manifestation of the second coming to the devil incarnate. In a very real sense, this is a continuation of the nature/nurture arguments of nearly a century ago, and these themselves were just slightly rephrased versions of the free-will vs. predestination conflicts of a still earlier time.

The current renewal of this old controversy was sparked by the publication of E. O. Wilson's book, *Sociobiology: The New Synthesis*, in 1975. Many have viewed this as a call to arms and have rushed to ally themselves pro or con. The

result has been a sharp polarization among the more vocal discussants. These have tended to resort to "which-side-are-you-on" rhetoric with an emotional stridency more characteristic of religious or political dispute than of dispassionate science. Indeed, an underlying political motivation has been suggested as the reason for adherence to its tenets by both supporters and detractors.

Among the foremost of the latter must be ranked Marshall Sahlins, whose little book, *The Use and Abuse of Biology*, is really an extended review of Wilson's tome. It takes just a couple of Wilson's unguarded statements as the *raison d'être* of what basically winds up being a *cri du coeur*. The sprinkling of French references in Sahlins' text leads one to suspect that at least some of the outrage expressed is the result of his debt to the milieu of French intellectualism. At bottom, what the book represents is a continuation of the 19th-century French objection to the bases of Darwinian evolution, and Sahlins carries it off with a panache that is in the best Gallic tradition. The verbal style is skillful and engaging, the display of multifaceted erudition is impressive if occasionally superficial, and eloquent logic is used in the characteristically French manner in which the object is to obfuscate rather than to edify.

Wilson's *Sociobiology* is a ponderous volume, and few who are not somehow directly involved are likely to read it from cover to cover, although it is in fact quite readable throughout. Many, however, will choose a shorter route such as Sahlins' work. Unfortunately they will be in the position of the three blind men who tried to describe an elephant, noting variously that it resembled a rope, a tree, and a snake. For all the brilliant vignettes and the occasional depiction of what are indeed serious flaws in Wilson's "sociobiology," Sahlins never once tells the reader what *Wilson* thinks it is. Since the vast bulk of Wilson's book is based upon just that, I think it is worth quoting his definition here: "Sociobiology is defined as the systematic study of the biological basis of all social behavior" (Wilson 1975:4).

Note carefully that Wilson did *not* say that all social behavior can be understood by the systematic study of its biological basis, although it is true that he comes close to suggesting just that in some of his unguarded moments. These moments appear at the beginning and at the end of his volume, and clearly represent an expression of exuberance at having accomplished such a *tour de force*. Surely Wilson has earned the right to crow a bit, and even if it does lead him to occasional uncautious overstatement, this can hardly detract from an accomplishment of such magnitude. If this were the total of Wilson's sins, then Sahlins' insistence upon the "integrity of culture as a thing-in-itself" for which biology is "absolutely necessary" but an "absolutely insufficient" condition (pp. x-xi) should have been all the caution necessary. As Sahlins later put it, "I am making no more claim for culture relative to biology than biology would assert relative to physics and chemistry" (p. 63).

This sober curb on Wilson's excessive claims is just what we should expect from a professional anthropologist, and if that is all there were to it, the flap

over sociobiology would never have reached the pages of *Human Ecology*, let alone the other avenues of exposure that qualify it as a "media event." Obviously there is more to it, and the more comes from the attempts of Sahlins and others to politicize the pursuit of science. For example, "What is inscribed in the theory of sociobiology is the entrenched ideology of Western society" (p. 101); which is really "genetic capitalism," (p. 72); and which assures the "naturalness" and "inevitability" of "the exploitation of others" (p. 77).

You well may wonder how one gets from studying the biological basis of social behavior to the declaration that such a procedure promotes the "exploitation of others" without some pretty tortuous intermediary steps. These there are, but even after explanation, some are neither clear nor defensible. While disclaiming "the slightest suggestion of *ad hominem* criticism," (p. xiii), the genesis of sociobiological evil is depicted thus: "Adam Smith produces a social version of Thomas Hobbes, Charles Darwin a naturalized version of Adam Smith; William Graham Sumner thereupon reinvents Darwin as society, and Edward O. Wilson reinvents Sumner as nature" (p. 93). Maybe that is not *ad hominem*, but the last of it surely would qualify as a *saltus ad absurdum*, or should that be vice-versa?

If Sahlins' complaint, like that of Alexander Portnoy, appears to be ethnic, on the other hand it certainly is no joke, and for several reasons it would be wrong to call it the "French disease." At bottom, however, it would appear to be based on the same unwillingness to accept Darwinian evolution that has characterized French academia from 1859 right up to the present. This in turn is rooted in the Romantic tradition, in which it is unacceptable to think of life as being subject to the mechanics of the inorganic, and where human will is clearly superior to mere life. And if this is not the ethnocentrism against which anthropologists so frequently contend, it surely is a form of anthropocentrism about which they ought to be particularly careful.

These are the general themes of the book, specified and implicit, but in the course of it Sahlins unwisely chooses to rest his case against his own version (perhaps I should say "perversion") of sociobiology on the development of a single anthropological case, "something like Durkheim's one well-chosen experiment that can prove (or disprove) a scientific law" (p. 41). Since he believes that the applicability of "scientific sociobiology" to "the human sciences depends largely on the fate of its theory of kin selection" (p. 17), he feels that "the issue between sociobiology and social anthropology is decisively joined on the field of kinship." He notes that the driving force of Darwinian evolution is natural selection, which he correctly defines as the "differential reproduction among members of a species or population." But since he clearly feels that such a model for evolution is to be deplored, he adds that, "An effective anthropological criticism of kin selection, therefore, would do great damage to the thesis and interdisciplinary objectives of sociobiology" (p. 18). He then attempts to furnish an appropriate example.

What he does not consider are the consequences that would follow if his one well-chosen example were to prove the exact opposite of his expectation. As it happens, this indeed appears to be the case, although, at least in this book, he has yet to realize it.

The case on which he chooses to take his stand is Polynesia, which he suggests is "privileged" since kinship is bilaterally reckoned ("cognatic") and so should provide conditions that are structurally favorable for the operation of "kin selection." Furthermore, Polynesia is where Sahlins has done his own field-work, and his expertise is well known. He discusses some examples from here and elsewhere and concludes that "each kinship order has . . . its own theory of heredity or shared substance, which is never the genetic theory of modern biology" (p. 57).

This is perfectly true as stated, but he then presents a quantity of anecdotal material which demonstrates how far real behavior deviates from that actually prescribed for given kinship orders. Given the degree of paternal uncertainty that follows from Polynesia's famous sexual freedom, it is evident that a rigid adherence to the stated rules of formal kinship practice would result in a failure to maximize investment in the promotion of a person's genetic material. But, as his informal data show, the widespread existence of fostering and adoption effectively balances the phenomenon of uncertain paternity. As I said, Sahlins' use of exemplary material is selected and anecdotal, but it raises the intriguing possibility that the theory of kin selection may just provide a better prediction for what people actually do under given circumstances than does a study of the kinship rules which they profess to follow.

To a degree, then, it would appear that Sahlins has marshaled the evidence for his own defeat. In part this can be traced to the anthropocentrism of the traditions of anti-Darwinism. The remainder is due to his chosen style of presentation which is structured along the lines of continental debate rather than of investigative science. As a previous review of his book has noted, he has cast his discussions in an "either-or" mold, a form of the adversary approach that is inappropriate (Simpson 1977:773). The inappropriateness of the adversary method, although noted in relation to another matter, has been well stated by Hammond and Adelman (1976:391), who write, "The focus on persons and their methods has led not only to the filing of bias statements but to the advocacy of the adversary method for the settlement of disputes about truth — a method which is ascientific not only in its procedure, but in its greater commitment to victory rather than to truth." *The Use and Abuse of Biology*, as one might expect, is more committed to winning an argument than to scientific clarification.

But I do not want to close on such a one-sidedly negative note. If I have not shared Sahlins' hostility to the "scientific sociobiology" of Wilson and his followers, I fully support his rejection of the "vulgar sociobiology" of Ardrey, Lorenz, and their ilk. Sahlins also points out some genuine weak spots in the

scientific sociobiology which bothers many observers – for instance, the mysticism that surrounds the process by which animals can identify the degree of their genetic representation in their relatives and accordingly govern their behavioral investment. But when some, in the name of sociobiology, claim that human social systems are genetically predetermined, and others shout back that this is simply the projection of a reactionary political ideology, my feelings, like those of Simpson, are best expressed in the words of the Bard: “A plague o’ both your houses.”

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C. Loring Brace
Museum of Anthropology
University of Michigan
Ann Arbor, Michigan