

## BOOK REVIEWS

**The Survivor: An Anatomy of Life in the Death Camps.** By Terrence Des Pres. Oxford University Press, New York, 1976, viii + 218 pp., bibliography, \$10.00 (cloth).

Recently several authors have attempted to tell us about human behavior in extremity and, by extension, about human nature. The Ik, in Turnbull's *Mountain People*, serve as a mirror in which we may see our future selves; writers, while telling us about human nature, warn us of its darker side. The abhorrent aspects of our bestial natures will be manifest in extremity, the argument runs, and we will soon face extreme conditions. Sending children to nursery schools in our own society is an incipient parallel, says Turnbull, to the exclusion of Ik children from their natal rights; the extreme selfishness of the Ik foreshadows the behavior of nations faced with worldwide famine.

Because they are buttressed by observations of people in extremity, it is difficult to counter these arguments. Yet there are counters, produced in "civilized" conditions by "civilized" human beings. One such is the Holocaust, the slaughter of millions of Jews; another is the Soviet labor camps.

Terrence Des Pres recounts and recasts the behavior of the survivors of these events; in the recounting, there are new insights and, in the recasting, there are familiar hypotheses about human behavior. In his recounting, Des Pres draws from the descriptions given by the survivors and develops a number of themes: his major thesis is that human beings survive as human beings, not as beasts, not as subhumans, not as infantile ids. The process of survival strips people of many of their attributes, to be sure, and such luxuries as compassion and hope may be lost. But the loss is temporary; in place of superfluous sentiments comes a new realism. The theme of the book is human survival under inhuman conditions: "My subject is survival, the capacity of men and women to live beneath the pressure of protracted crisis, to sustain terrible damage in mind and body and yet to be there, sane, alive, still human" (p.v).

Des Pres's theme can be dealt with on several levels. First, as a critique of theorists who see the victims' behavior as "infantile regression." Des Pres disagrees with analyses that explain concentration camp posture as one of total submission. He not only disagrees with the theories; he documents his disagree-

ment with accounts of the experiences of those who were helped to survive, of those who lived for years next to the gas chambers, of those who devised and enforced such things as the "Bread Law." From Auschwitz, after a bread thief is caught:

"So what happened? Did the others beat him up?"

"They killed him, of course. What's the use of beating up a bastard like that,"

That was the law in Block 18. If a man stole your food, you killed him. If you were not strong enough to carry out the sentence yourself, there were other executioners; it was rough justice, but it was fair because to deprive a man of food was to murder. (Vrba, 115, quoted on p. 141)

Justice, albeit rough—but not mayhem, or chaos, or killing for its own sake. The Bread Law, according to Des Pres, "was the foundation and focal point of moral order in the concentration camps" (p. 140).

But the order that emerged is contrary to that posited by the psychoanalysts; it is not an order imposed from without in imitation of the captors—it is an internal arrangement in defiance of the guards. Initially, fear and chaos prevailed. Then, slowly, "in sorrow and a realism never before faced up to" (p. 146), order emerged, gifts were given, help was offered.

On another level, the book is an answer to the question puzzled over by the psychohistorians and the psychoanalysts: why did the prisoners not revolt? Des Pres's answer is that they did revolt and they did resist, in all the ways possible for them, once the preliminary shock of being treated like animals had worn off:

To pass from civilization to extremity means to be shorn of the elaborate system of relationships—to job, class, tradition and family, to groups and institutions of every kind—which for us provides ninety percent of what we think we are. (p. 182)

It is easy to theorize about infantile regression and reversions to fascination with excrement—but harder to acknowledge or to imagine an excrement-filled environment. Yet this is what the survivors coped with—starvation, excrement, degradation—and it is from this coping that we learn about adaptation to unimaginable circumstances. One of the most important points the book makes is that some of the victims adapted to these conditions by creating a new social organization, appropriate to an excrement-filled environment, appropriate to starvation. The problem with the easy theories is that they do not reflect the realities of either social organization or environment.

In part, the theories reflect our fantasies, and it appears that we of the civilized world like to imagine that Hitler's Germany was a monolithic, perfectly coordinated war machine. The survivors, too shrewd and too poor to maintain such illusions, located the weak spots and set to work creating a viable social

structure in the chinks. They hid prisoners in the typhoid wards of hospitals; they smuggled bits of pitiful treasure to celebrate birthdays; they preserved in themselves their humanity, reordering it to fit the circumstances.

Des Pres answers another question that plagues theorists: why degrade people who were already doomed? The answer: "To condition those who actually had to carry out the policies" (p. 61). The victims and their captors were human, and cultural conditioning was taking place on both sides. The captives had to be taught to die; the captors had to be taught to kill.

In the recounting, Des Pres tells of a struggle for survival, of revolt and of reordering that most of us have not heard. In the recasting, he makes some more familiar interpretive points. He believes that human beings possess "a set of activities evolved through time in successful response to crisis . . ." (p. 192). These activities are innate, genetically transmitted patterns that serve the "general cause of survival" (p. 193). But which aspects of the survivors' experiences are to be regarded the product of genetic transmission? Des Pres does not make this clear. "Bread Laws" are surely not inherent in the human species, nor in any other; the capacity for making rules may be. "Survival behavior," says Des Pres, "reveals a fixed system of activity, biological in origin, which is specific to humanness as such" (p. 194). No matter how impressive the story of survival the author tells, it is difficult to follow him into this trendy and largely unsubstantiated area of speculation. If survival were a fixed system of activity, genotypically established for all time, why did 6 million people allow themselves to be annihilated? Were they examples of faulty genotypes?

To say that living beings struggle to maintain life is one thing; to say that they struggle because they are genetically programmed to do so is either meaningless or frivolous. Meaningless because so many died; frivolous because biology is the study of life and assumes that living organisms will live. If biologists studied death instead, they would assume that organisms tend to die. In fact, organisms live and die, and many aspects of both processes are doubtless genetically controlled. Beyond that, however, it is difficult to generalize, and the location of the death genes is as difficult to establish as the location of the survival genes.

Thus the most telling point made by Des Pres is muted: people in concentration camps—some people, not all—were able to relearn, to reorganize their behavior, and to survive. Social organization is a precondition for human life, but it is not clear that some instinct for social life was operative in the camps: these were not human beings stripped to the level of raw "instinct"; they were fully socialized human beings who, as a condition of survival, replaced one social organization with a more viable one. Those who abandoned hope were killed by their captors; those who abandoned morality were killed by their comrades. These who survived were those who were able to adapt, not those programmed

for survival in some mysterious way. The capacity to adapt, that most marvelous of human abilities, deserves more credit than it is given.

One Soviet inmate, quoted on page 94, put it best:

Human beings are like weeds. They take some killing. Now if you treated horses like this they'd be dead in a couple of days.

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**Biogenetic Structuralism.** By Charles D. Laughlin, Jr., and Eugene G. d'Aquili. Columbia University Press, New York and London, 1974, 211 pp., illustrations, notes, references, index, \$10.00 (cloth).

Because the brain is the place where human biology and culture interact, the structures and functions of this organ are of major importance to the study of human ecology and social behavior. I had great hopes that *Biogenetic Structuralism* would show how this interaction takes place in those structures and functions. I was disappointed to find an obsessive preoccupation with only a small part of the story: genetically determined cerebral subsystems.

The major thesis of *Biogenetic Structuralism* is that perhaps "most social (and linguistic) universals are manifestations of genetically determined neurognostic models, or sets of such models, in the deep memory structure of the human brain" (p. 155). These neurognostic models are envisioned to be spatiotemporal patterns of organization in the neuronal activity of the brain's association areas. They are thought to store representations of reality in something like holographic interference patterns of postsynaptic potentials, and these models are believed to govern the behavior of human beings. The authors contend that "The most fundamental models . . . are structured genetically and thus provide a biogenetic base for the organization of sensory input" (p. 82). These genetic structures are held to be similar in all people, and this, we are told, may explain "the deep structural similarities in various human societies noted by Levi-Strauss, Piaget, Chomsky, Slobin, and others, as well as for the universal similarities in unconscious production so well documented by Jung" (p. 103).

The book does have several features to recommend it. First and most striking, it is *interdisciplinary*. The authors (whose backgrounds include anthropology, linguistics, neurological science, and psychiatry) discuss an impressive diversity of topics ranging from Plato to Pavlov and from "culturology" to

the Krebs cycle. Second, it does try to bring an *evolutionary perspective* to bear on our understanding of the human brain. As the authors put it, the book attempts “to found [orthodox] structuralism on a meaningful physical base and to answer questions concerning the evolution of cognitive structures through the course of hominid development” (p. 14). And, third, it is *daring*. Laughlin and d’Aquila propose volatile new explanations for, among others, (1) the size of the human brain (an “allometric by-product” of selection for increased body size), (2) the evolution of tool use and language (effects, not causes, of the increased organizational complexity of the brain), (3) science and empiricism (“the underlying mode of human inquiry is structured biogenetically” p. 143), and (4) sleep (a period of release from sensory input to allow for modification of dendritic-synaptic structures).

However commendable, these assets still do not outweigh a number of serious, general flaws in *Biogenetic Structuralism* which threaten to distort our understanding of biology, culture, and human behavior. First, Laughlin and d’Aquila misrepresent the evolutionary process that is central to their “evolutionary neurostructural approach.” Uncritically and without evidence, they assume that natural selection in primates operates exclusively at the level of social groups and demes. Human traits are thus examined for their evolutionary significance “for the species” or “reproductive population” as a whole. Apparently unaware of the arguments and evidence that interindividual selection is usually more important in evolution, they then propose some improbable group selection pathways for the evolution of their cerebral structures. Ironically, these arguments suggest just how unlikely their postulated structures may be.

To give one example we are told that a neurognostic structure for clinical depression would have evolved as “a built-in mechanism for dominant males to move down the group hierarchy once they become too old to be effective.” When a few failure experiences cause this structure to be activated, the alpha male

may experience helplessness or “depression” resulting in withdrawal and his relatively peaceful replacement by other males in the dominant position. . . . Activation of this neurognostic structure may provide the smooth transition the group needs to survive. (pp. 121-122)

At times, an activated “depression structure” would sacrifice the reproductive interests of a dominant male for the good of some or all of his associates. This presents a problem. The organic evolution of such a structure would require a sustained amount of group selection sufficient to override opposing individual selection. Until we have convincing documentation for such an occurrence in primate evolution, theory alone would sooner suggest that such a structure did *not* evolve. No wonder these authors remain unable to specify a definite neural structure behind this behavior. Chances are good that there isn’t one.

A similar misuse of evolutionary theory arises in their discussion of a polygenic model for schizophrenia. The polygenic model is attractive to biogenetic structuralism because of “the important theoretical advantage of emphasizing the interrelationship of multiple cerebral structures and of indicating

the redundancy of the mechanisms underlying higher brain function” (p. 177). Such an arrangement could well have evolved for its adaptive benefits, but how then does one explain the recurrence of its maladaptive form? Easy, say Laughlin and d’Aquila: “In terms of polygenic theory, schizophrenia could be thought of as simply part of the genetic load, the *price paid for conserving genetic diversity*” (p. 176, their italics).

These and other blunders in representing the evolutionary process are still not as serious as the obsessive overemphasis on genes that permeates the text. The authors rationalize their “heavy emphasis on the inherited quality of neurognostic structures . . . because this concept has great explanatory power in analyzing cultural universals” (p. 124). In this, Laughlin and d’Aquila join sociobiologists in forgetting that widespread human practices may represent parallel *cultural* solutions to a few basic problems that confront human beings everywhere.

In general, their “evidence” for genetic bases is just not convincing: “The universal and adaptive quality of the neurognostic structures described in Chapter 5 (self-other dichotomy, causal thoughts, binary opposition, etc.) seem to mitigate against an experiential explanation for the formation of these neurognostic structures” (p. 182). This argument, used in some form for nearly every structure in the book, must be criticized for two reasons. First, there is absolutely no reason to believe that the universality of a trait proves its genetic origin. There may always be a cultural explanation. Consider their example of “binary opposition,” “the primitive, but universal, tendency to order reality into pairs that are usually subjectively experienced as opposites” (p. 115). The observation that people almost everywhere think in terms of light and dark, over and under, up and down, etc., in no way proves that this is some wired-in feature of our human hardware. For all we know, it may simply reflect the advantage of learning to think in these terms in all habitats which have day and night and three-dimensional space.

Second, Laughlin and d’Aquila seem to believe that an adaptive function also implies that a trait is genetic. Earlier in the book, they conveniently set this up by claiming that cultural behaviors are generally “adaptively superfluous”:

*Much that we view as social behavior, institutions, cultural values, etc., among contemporary human societies stem from models having for the most part no adaptive importance to the species or individual reproductive population. p. 97, their italics)*

While this assertion nicely supports other claims of *Biogenetic Structuralism*, it strikes me as a bit hasty. The authors want us to believe *both* that properties of the human brain evolved all along for their adaptive value *and* that the major accomplishment of that brain (culture) simultaneously evolved to be adaptively superfluous. This is the kind of sloppy thinking that underlies most of the genetic theories for human social behavior. In this case, though, the authors’ inability to see the adaptive value of cultural behaviors may also be a product of their looking at the wrong level.

In short, the "evidence" presented by Laughlin and d'Aquili to argue that cerebral models of reality are inherited in no case refutes the implicit null hypothesis that they are not. If these authors want biogenetic structuralism to guide the social sciences toward "becoming normal sciences with a highly coherent body of nomothetic and predictive theory" (p. 204), they will have to do better than this.

Laughlin and d'Aquili might have laid the groundwork for a really significant breakthrough in our understanding of human behavior had they focused more on the interaction of biology and culture and less on postulated imperatives of biology. To me it is significant that their "perhaps most compelling body of evidence" comes from research on *learning* (p. 107). There, "What has emerged is the recognition that certain behaviors are very easily learned by some species, often even after only one trial, while they require the usual or 'normal' number of trials to be learned by other species" (p. 110). Such evidence leads to the conclusion that these organisms may have built-in mechanisms that facilitate learning certain behaviors, particularly those of direct adaptive significance to the organism. Laughlin and d'Aquili actually review the evidence that human phobias represent such "prepared learning," but they fail to read the important message of this example. To me, it is significant that their best evidence involves the interaction of inheritance and learning *and* leads to behavior of adaptive value to individual human beings.

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**Chemicals in Food and Environment.** M. Webb, Scientific Editor. *British Medical Bulletin*, Volume 31, Number 3. Medical Department, The British Council, London, 1975, 87 pp., \$9.00 (paper).

The *British Medical Bulletin* serves an important function in medical literature. It has a long tradition of publishing up-to-date collections of related papers on a wide variety of relatively specific topics, which are of great concern to clinicians and researchers. The issue under review continues this tradition. There is no question that chemicals in food and environment is a timely topic and one that is of interest to a diverse group of scientists and laymen. It is certainly of major importance to many human ecologists.

In the brief space of 87 pages, 15 papers are presented. All are summary reviews of the state of the art. A minimum of new data is provided, but the references include a large proportion of recently published material. The authors

are British, and the majority of the data presented are based on studies carried out in the United Kingdom. This may reduce the collection's appeal to an American audience.

Although they usually focus on specific topics, it is possible to divide these studies into three major groups: general themes, papers dealing with chemicals naturally occurring in foods, and those concerned with chemicals introduced into foods and the environment through human activities. The authors assume primarily either an epidemiological or toxicological perspective.

Three papers fall into the "general group." Neuberger's "Introduction" places the subject in a historical framework and examines the role of the scientist in pollution studies. He notes: "The function of the scientist is to provide the facts, and to remain as objective as he possibly can be in presenting his data and in interpreting them" (p. 183). This caveat is often more easily suggested than followed. Pochin's paper on "The Acceptance of Risk" examines mortality rates and other data on occupational and nonoccupational risks. The author raises a number of important points regarding the evaluation and acceptance of risk and suggests the need for providing populations with quantitative estimates of risk levels associated with toxic chemicals. The final paper in this group, "Importance of Epidemiological Studies Relating to Hazards of Food and Environment," by Higgins, clearly examines the role of epidemiological studies in the evaluation of chemical hazards. A number of examples are considered and the importance of monitoring and surveillance is stressed. This paper is among the most significant for human ecologists.

Three papers deal with naturally occurring food toxins. I found the first two papers in the group particularly informative because they integrate toxicological and epidemiological data. Crampton and Charlesworth's paper, "Occurrence of Natural Toxins in Food," considers data which show that toxic chemicals may be normal ingredients of foods. The focus is primarily on oxalate toxicity and on substances shown to be carcinogenic in animals, but the authors also include a useful table of other food toxins known to affect man. In "Mycotoxins," Austwick presents a comprehensive review of toxins produced by fungi and some mycotoxicoses of humans and animals. Spicer's very brief article on "Toxicological Assessment of New Foods" discusses the appraisal of diet safety and concentrates on possible toxicity associated with two "new" foods, soya beans and microbial protein.

The remaining nine papers consider various aspects of the introduction of chemicals into foods and the environment through human activities. Three of these deal specifically with foods. In "Analytical Surveys of Food," Egan and Hubbard discuss specific examples of contaminants and illustrate the needs for, and strategies behind, surveys which identify traces of toxic substances. The problem of repeated ingestion of small amounts of toxic substances is addressed by Barnes in "Assessing Hazards from Prolonged and Repeated Exposure to Low Doses of Toxic Substances." He stresses our limited knowledge of important variables such as tissue concentration and speed of reaction, the nature of the



toxic injury, and sensitization. This is certainly an area that demands further investigation. Lloyd and Drake, in "Problems Posed by Essential Food Preservatives," examine chemicals used to extend the usable period of foods. They suggest that "In the final analysis it will be necessary to measure the benefit that accrues from the availability of preservatives and compare it with the scale of 'hazards' resulting from dietary sources as a whole, and not least, the natural constituents of food" (p. 218). Many of the ideas considered in this section relate to points made in Pochin's paper on the acceptance of risk. What is, and is not, acceptable?

Goulding considers "Chemical Hazards in the Home." He reviews statistical data from England and Wales on common household poisons and poisoning, and concludes that the danger of such chemicals has probably been overstressed.

Three papers deal with heavy metals in the environment. In "Lead: The Relation of Environment and Experimental Work," Clayton examines the occurrence of lead in soil, water, and air, and the toxicological effects of mild exposure to lead on enzymes, mental development, and the fetus. Although the effects of large doses of lead on the human organism are well known, the long-term effects of mild continuous exposure remain to be elucidated. Chemical and toxicological properties of "Mercury and Mercurials" are addressed by Magos. Even though a causal relationship between methylmercury poisoning and Minamata disease has been clearly established, Magos suggests that the dangers of mercurial contamination of the environment have been exaggerated. The final paper on metals, "Cadmium," by Webb, reviews the biochemical and toxicological aspects of environmental cadmium. Unknown hazards of prolonged low-level exposure in humans are emphasized.

Problems of "Water Supplies of the Future and the Recycling of Drinking-Water" are examined by Martin. Maintenance of adequate potable supplies demands that attention be directed to factors such as concentration of mineral salts, pollution by organic substances, and microbiological hazards. The air we breathe is no less important than the water we drink. In "Carbon Monoxide," Lawther reviews the sources, distribution, absorption, and toxic effects of carbon monoxide, and considers air quality criteria. The volume concludes with book reviews of several related publications.

These papers provide informative summaries of varied topics related to chemicals in food and the environment. The long-term effect of small doses of toxic substances is a recurrent major theme. I recommend this collection to those who are concerned with these aspects of human ecology.

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**Fields of the Tzotzil: The Ecological Bases of Tradition in Highland Chiapas.** By George A. Collier. University of Texas Press, Austin, 1975, xv + 255 pp., figures, photographs, graphs, maps, bibliography, index, \$9.75 (cloth).

The Indians of the Central Highlands of Chiapas in Mexico have been studied extensively. During the 1960s, hundreds of research reports were produced by small armies of investigators from Harvard, Stanford, Chicago, and elsewhere. This book is one of the most conceptually ambitious and complex products. Collier is a Stanford social anthropologist who draws both on his Harvard fieldwork and on other research "to characterize the causes of traditional [Indian] behavior and the reasons for its persistence" (p. 3).

He focuses on "the bases of Tzotzil tradition through an ecological analysis of the factors bearing on Tzotzil land use" (p. xiv). These include kinship and land tenure, land abuse, land-use patterns, input-output analysis of farming practices, social/ethnic status and its historical development, mobility, demography, occupational specialty, the region's "refuge" status in the nation, nativistic movements, and Indian nationalism. This expansive and integrated treatment of land use should please interdisciplinarians. Collier's extensive command of these empirical matters reflects apparently profuse and careful fieldwork. While deserving of attention, some of his theoretical and methodological views are less convincing.

The main thesis is that tradition is a "dynamic, adaptive response" of Tzotzil Indians to "their special placement in a larger, encompassing system" (p. xiv). Such a system is characterized as "the very evident hierarchy of social relations leading upward from family to hamlet, township, intertribal, and interethnic relations embedded in a context of state and national processes" (p. xiv). Collier chooses not only to look at Indians in their relatively isolated home territories but also to see them as parts within larger wholes. Thus the Indians' "environment" is conceived to include both the local physiographic features "usually thought of" and also "less tangible," although "equally important," impacts of, for example, "distant markets or national political ideology" (p. 3). This attention to a spatial hierarchy of "environmental" data is one part of Collier's "ecology." The other derives from "semiautonomous" traditional Indian subsystems of kinship, social organization, and ethnic-group relations, which have "regular, recurrent, or relative stable features of internal organization" (p. 3). His "ecology," then, views these subsystem features as being "conditioned by" a large set of environmental factors. Unfortunately, Collier does not clarify whether "tradition" itself also becomes a "conditioning factor" helping to modify man-land relationships within the larger system.

Four situations evince environmental factors that "condition" Tzotzil livelihood: (1) their increasingly heavy use of rented lowland farms to supplement traditional highland subsistence agriculture, (2) dependence on distant wage-labor markets by Indians whose highland territory is destructively over-

populated, (3) ethnic discrimination against Indians derived from their subordinate social position within an already geographically and economically marginal "refuge region," and (4) a "nativistic colonization effort that is spurred by nationalist agrarian ideology" (p. 5).

The book's most serious stylistic flaw is the recourse to elusive straw men for support of the author's methodological and theoretical pleadings. For example, about 80% of one township's income derives from farming rented lowlands. This in turn sustains less economically important farming on traditional highland plots on which native kinship-property relationships depend. This lends to the local (highland) man-land system "an appearance of autonomy actually dependent on an external influence" (p. 6). The straw man is the unidentified person to whom this system gives an appearance of autonomy. Is it perhaps a misguided fellow anthropologist? Elsewhere the straw men are "outsiders [who] see pottery production as . . . [a] principal activity, despite the fact that pot-making is a full-time occupation only for poorer women" (p. 173). Perhaps the straw men here are mere tourists? Sometimes they carry a more specific label, such as the "environmentalist" or "social economist" who is apt to focus too sharply on the separation of the highland Mayas from the rest of Mexico; he might "analyze the adaptations that these groups make to their locality rather than those they make to their environment" (p. 4).

This flaw is compounded by a paucity of references to pertinent and often contrasting literature. "Citations in the Bibliography refer to many sources not cited directly in the argument of the book. These citations have been classified into categories related to the unfolding of the argument, which can be considered a guide to the more detailed background of the study" (p. 225). It is a bad scholarly practice, especially in a book that purports to challenge earlier methods and interpretations, not to tell reader precisely *whose* methods and interpretations are being challenged. There is veiled criticism of earlier "ethnographers" who used the township as the "primary unit of study" (p. 7). Collier gratuitously tells us that selection of the "right" unit depends on the problem studied. For example, "all Indians have a similar subordinate relation to Ladinos [the culturally non-Indian population]. This relation is region wide and is best studied in relation to the region as a whole . . ." (p. 8). But one might ask a different question: are Indians in all townships equally subordinate to Ladinos? While the general subordinate relation is explained, equally interesting variations are neglected. To substantiate the implicit criticism of earlier township studies, Collier would need to show that this unit was inappropriate for problems under investigation.

Collier's hypothesis is that Tzotzil tradition is not simply a syncretized, distinctive indigenous and Spanish cultural complex that has survived. Rather, it is a dynamic, active response that Indians make to their marginal niche in a larger, changing system. Apparently, he would have us reject the alleged anthropological conventional wisdom that native lifeways persist because they

have been isolated from those of the modern world. He shows that (1) substantial information and forces of change do reach the Tzotzil from afar, (2) migration intermixes the Tzotzil peoples of different traditions, and (3) local economic adaptations shift, thus suggesting they lack the bases of tradition. The book's contribution rests on calling into question isolation as the single explanation for the persistence of tradition, and for offering an interesting alternative ("ecological") interpretation. However, the importance of communication or isolation for particular change or stability is an empirical question. Even though there has been some communication from afar and some migration, intermixing does not obviate the fact that the Chiapas Highlands have been and still are relatively isolated from the mainstream of modern Mexico. One can accept the author's focused interest on persistence of tradition, and one can appreciate his carefully marshalled evidence to support his view. One need not, however, accept the implication that relative isolation does not enter into the explanation.

If I interpret him correctly, it is Collier's position that tradition is preserved if ethnicity (distinctiveness) persists. This is unassailable if one accepts social distinctiveness as one's definition of tradition. He also asserts that while distinctiveness may persist, it is constituted differently over time. Indian lifeways *are* changing. More orthodox students of culture have been willing to interpret such changes in land-use practices, economic roles, and spatial mobility as evidences of loss of tradition. If, as Collier argues, tradition is distinctiveness (ethnicity), then tradition persists; conversely, if tradition is the actual traits representing the way of life of the people, then tradition is slowly being lost. Collier chooses to view tradition as an ethnic phenomenon that is fundamentally social, rather than cultural, and that has an "ecological" basis. In this sense, the Tzotzil are like ethnic groups everywhere; their ethnicity is an active, adaptive response to placement in a system. One hopes that Collier will soon explain how *loss* of ethnicity occurs.

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**Ecology and Change: Rural Modernization in an African Community.** By C. Gregory Knight. Academic Press, New York, 1974, xx + 300 pp., maps, photographs, figures, appendixes, bibliography, index, \$16.50 (cloth).

Scholars who work within a particular research paradigm are rarely objective about the merits of alternative modes of interpretation. To invite a self-described analytical economic anthropologist to review a book by a self-admitted ecological geographer seems to invite controversy. While I have misgivings about

Knight's point of view, I do not intend to question the book's disciplinary value. Its treatment of the production system of the Nyiha of Southwest Tanzania may, or may not, meet standards of his discipline. I know that it raises problems for me.

Knight's approach is essentially descriptive and inductive. In his own words, he treats environment as an absolute and studies how the process of "modernization" has occurred within this confine. Population pressure is seen as forcing swidden agriculturalists to apply more intensive cultivation methods and to develop an expanding cash coffee crop. This process is analyzed in the context of agricultural change models developed by Boserup, Allen, Ojo, and others. Generalizations about rural transformation are related basically to the development of coffee cultivation. If a main conclusion exists, it is that the Nyiha have reacted to opportunities for change as rational men who desire to minimize uncertainty as they attempt to innovate.

My estimate is that Knight has no real sensitivity to economic analytical methods even though he invokes economics and economists in several instances. He treats Boserup's model of variable labor costs as if it were a historical model of the evolution of farming systems, and he does not allow for the possibility of an indigenous exchange economy. Thus he ignores the growing literature on the economic role of cattle in East African indigenous societies and never asks how investment in cattle might affect indigenous agricultural inputs. There is a general pattern in East Africa of rising opportunity costs associated with agriculture in relation to increased cattle wealth. Therefore, some allowance must be made for the possibility that wealthy cattle owners may move back to agriculture when opportunity costs drop as labor becomes cheap relative to land (Boserup's model). The implications of this for internal exchanges and production are problematic.

His treatment of the habitat as "absolute" creates another problem. This in effect focuses attention only on the *cost* of utilizing a certain habitat. Yet the negative features of the habitat encourage cattle raising, which is the chief form of indigenous wealth. To think of people as being determined by their habitat is analytically questionable.

Finally, Knight indicates in his last chapter that he thinks a reasonable guideline to ecologically viable change is one that avoids long-term threat to the health of the environment. This is, he says, the ecological perspective. Georgescu-Roegen is responsible for having underscored that fact that development is an entropic process. If we are to deny other people the possibility of destroying part of the environment in their quest for affluence, we deny development for them.

Probably the biggest value of the book is its complex description of the Nyiha production system. Such descriptions are rare. This aspect of culture has been largely ignored by agricultural economists and poorly illuminated by an-

thropologists. On balance, then, the book comes across as good description and uninteresting analysis.

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**Baegu: Social and Ecological Organization in Malaita, Solomon Islands.** By Harold M. Ross. University of Illinois Press, Urbana, 1973, 334 pp., photographs, tables, diagrams, appendixes, bibliography, index, \$8.95 (paper).

Ethnography has been defined as the description of one culture in the terms of another. In the same way, cultural ecology can be seen as the study of how one culture's cognized model of its environment relates to the operational model of that environment which exists "in reality," and which exists also (one hopes) in the mind of the ecologically trained ethnographer. Harold Ross, however, in his book *Baegu* attempts more than a description of the cultural ecology of a forest-dwelling people in the interior mountains of North Malaita, Solomon Islands. By organizing his research around the twin themes of social organization and ecological adaptation, and by recognizing that residential behavior and land tenure are common factors in both, Ross provides himself with both a fieldwork focus and a conceptual theme. The result greatly transcends traditional ethnography. The book's value as a description of a pagan culture and subsistence way of life that has disappeared, unrecorded, from nine-tenths of island Melanesia is itself noteworthy, and it was a value that Ross's informants themselves fully appreciated. What *Baegu* also does is to ask and to provide tentative answers to wider questions of residential behavior in agrarian societies. In doing so, it also raises some general problems of carrying out fieldwork in human ecology.

It is understandable that as an anthropologist Ross was interested mainly in the cognized model of the Baegu environment and in Baegu perceptions of residential behavior and kinship relations. In convincing detail, he describes the people's world view at microscale (layout of houses and hamlets), mesoscale (neighborhoods and districts, and their resources and important features such as sacred groves of primary rainforest), and macroscale (Malaita Island, and the heavens, hades, and netherworld that surround it). But an understanding of this cognized world, as opposed to a mere description of it, requires other sorts of data:

What really existed were people, houses, and garden plots; but the patterns I found resulted from human decisions made according to certain criteria. Therefore I have tried to develop descriptions of aspects of Baegu behavior, based on the way people make decisions. (p. 243)

In decisions concerning the *siting* of a settlement, factors such as water supply and access to gardens are paramount. On the other hand, decisions about settlement *size*, which depend on how many households have joined or left a hamlet, and consequently decisions about the overall *spacing* of the population relative to its resources, are influenced mainly by social and supernatural factors. For example, prolonged residence in one place involves a cumulative increase in taboo violations, so that supernatural punishment becomes more and more probable. Bad luck is manifested in events like the death of children and crop failure. Of course, the disease hazard and the likelihood of soil exhaustion are themselves related (in the "operational" view) to the size and persistence of settlements and to the frequency of use of garden land, respectively. The strength of Ross's approach is in showing how decisions about where to live are processes acting through cultural media, being perceived and explained according to beliefs that are far removed from the normal subject matter of human ecology. At the same time, Baegu residential behavior does function as a human spacing mechanism providing economic flexibility and permitting a satisfactory development of behavioral, social, and cultural patterns.

The practical problem of Ross's approach to Baegu residential behavior is also a general problem of procedure for researchers in human ecology. How can one investigator manage to cover both the cognized and the operational spheres of ecological processes so that his generalizations will satisfy specialists in both areas? According to Ross, "in a very real sense the ancestor spirits and their living descendants are united in a partnership to maintain the fertility of the land" (p. 230). In other words, a settlement pattern of small scattered hamlets provides the means both to pacify the spirits of the ancestors and, it is presumed, to ensure a short period of use of each plot, thus maintaining soil fertility and facilitating the succession back to secondary forest. Unfortunately, the cognized part of this explanation is far better supported with evidence than are the operational, or real-world, processes. The aspect of the "partnership" that we need more information on is that between inherent soil quality (which is described only in ethnoscientific categories) and the frequency and intensity of cropping, itself affected by the spatial location of cultivators relative to land resources. Ross's subjective impression was that, as a result of residential behavior, population pressure does not exist in the pagan areas of North Malaita, but he fails to explain the apparent success and persistence of a quite different, aggregated settlement pattern by Christians in this area. Similarly, he feels that the walking distance from house to garden seldom becomes "unreasonable" for the Baegu, and so this is not considered a significant factor in residential relocation. Because of the author's disciplinary background and the constraints of fieldwork time, it is not surprising that hard data on these and other important operational processes are completely lacking.

In any field project which has the broad intellectual aims that an ecological perspective demands, gaps in data or emphasis are bound to occur unless a whole team of specialists can be properly integrated and directed—in itself an extremely

difficult operation. Only the epidemiological aspects of Baegu ecology have benefited from such support, as a result of Ross's collaboration with a Harvard Biomedical Expedition. It is a measure of the author's breadth of interest and understanding that even in areas where his judgments are necessarily intuitive, the argument that he presents is in general highly convincing. His book is well organized, is written with clarity and a sense of humor, and is richly illustrated with diagrams, maps, and photographs. It deserves a readership far wider than one composed entirely of area specialists.

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**The Economics of Natural Environments: Studies in the Valuation of Commodity and Amenity Resources.** By John V. Krutilla and Anthony C. Fisher. Johns Hopkins University Press, Baltimore, 1975, xviii + 292 pp., maps, tables, graphs, notes, index, \$16.95 (cloth), \$4.50 (paper).

In this volume, Krutilla and Fisher have brought together a good deal of their recent work, some of which has already been published in the professional economics literature. Abbreviated versions of studies on prairie wetlands and the Alaskan pipeline, which were published by others in *Resources for the Future's Natural Environments Program*, are also included. The first third of the text focuses on institutional and theoretical topics. The remainder is case studies. Readers of Krutilla will recognize his hand in both parts, especially the conceptual framework in Chapter 2 and the Hells Canyon and White Cloud Peaks cases (Chapters 5-7). Fisher's contributions include especially control theory, econometrics, and related matters. References are given at the end of each chapter.

This book is written at a level suitable for trained economists. The authors characterize their work as a "first-generation" effort to bring the analysis of amenity use of natural resources into the body of economic theory and application. Their focus is on the economics of preservation vs. development of natural areas. Their contributions include especially the development of a rigorous analysis over time, based on a number of concepts not recognized in conventional benefit-cost analysis. The authors do much to counter the popular American view that, since land in its natural state has no value, development is called for in the interest of improved economic efficiency. Indeed, they show that the opposite is frequently the case, where the phrase "improved economic efficiency" is interpreted in the economist's sense to mean increased satisfaction of human wants from existing resources, human and natural.



The analysis is concerned especially with three influences on the economics of preservation and development: (1) technological change, (2) increasing demand for amenity use of natural environments, and (3) irreversibility of development. Technological change increases the possibilities of substitution among inputs and among products throughout the economy. Since technological advance affects only the commercial (not amenity) uses, this makes the extraction of a particular mineral at a particular natural area relatively less important than preservation of that site. At the same time, the demand for amenities from natural areas can be expected to grow with increased population, increased GNP, increased leisure, and greater awareness of the value of a natural experience. The combined effect of substitution possibilities and growing amenities demand makes preservation increasingly more valuable than development. To the extent that development is irreversible, the case against it is strengthened, particularly in the presence of uncertainty. Other theoretical considerations are also addressed. These include unsettled issues in the selection of a social rate of time discount and still more unsettled questions of how to frame intergenerational comparisons.

Over and above the conceptual biases toward development are the institutional biases. Most of the latter arise from understatement of conventional market costs of development. The authors make no attempt to catalog all of these, but note at various points that provisions of the U.S. Internal Revenue acts permit the expensing of capital expenditures by the minerals industry, that public investments in water resource projects are subsidized by artificially low interest rates, and that the U.S. Forest Service conducts "deficit" timber sales, where the proceeds do not cover the costs. In the Hells Canyon study, the availability of a federally provided electric power transmission system drove a wedge between private and social costs of development. In the prairie wetlands study, the absence of a mechanism for compensating private wetland owners for wild ducks led to an underproduction of the latter. In the Alaskan pipeline case, profit-maximizing calculations by private interests were shifted toward the socially more costly and environmentally more damaging trans-Alaskan route, and away from the trans-Canadian alternative (which was better on both grounds) by import quota-related distortions in petroleum markets.

Perhaps the subtlest and most important institutional biases are embodied in the priority of rights to use natural areas. Thus the Mining Act of 1872 makes possible transfer of ownership in fee simple from the government to private parties holding valid mineral claims. This situation is analyzed by means of a model in which property rights are assigned alternatively to either miners or amenity users (pp. 30-33, 266, 267). Whichever party does not have title (in this case, amenity users) will be the poorer as a result. They will therefore have a lower market demand (ability to pay) than they would had they been given the property right.

This logic is applied by Krutilla and Fisher to wealth effects arising out of liability for economic externalities as well as to wealth effects from conventional

aspects of property rights. Unfortunately, I find some difficulties in these applications. The two-party negotiations of the model may or may not correspond to the reality of decision making. If the Minerals Act is amended, property rights will, more likely, be retained by the government rather than reassigned to amenity users. This means that there are three parties in the decision-making process. The government is rationing uses between the other two. Relative changes in wealth status arise from subtle aspects of priority rights, rather than from a more absolute concept of property. And whatever the case with the Minerals Act, application of the wealth effect to nonmining users will generally occur in the context of government rationing decisions rather than by direct negotiations among users.

In discussing damages, the authors refer to "the assignment of liability for the damages suffered by one group of users of common property resources from preemptive, incompatible uses by another group" (p. 266). Preemptive, incompatible use by one individual is present in all private goods consumption, and economic theory gives no grounds for assigning liability, so long as the opportunity costs of acquisition are paid. The consumption of an apple or an orange by one individual preempts others from consuming the same and no liability is incurred to the others. As long as all (social) opportunity costs are included in government assessments against public land users, wherein is Krutilla and Fisher's case different?

Moreover, if true externalities are present (i.e., if the act of using on the part of one gives rise to damages to another, over and above mere preemption), then there is a need to assign liability. But economic theory gives some guidance on which liability assignments are likely to produce greater economic efficiency. If liability is assigned to the damage-creating party, production functions more consistent with competitive markets result (Tybout, 1972).

The authors have almost nothing to say about congestion in their opening theoretical discussion, although it appears explicitly (and unavoidably) in three of their case studies: Hells Canyon, White Cloud Peaks, and Mineral King. In the Hells Canyon case, a "judgment" concept of capacity is used to produce a change in growth rates of amenity demand. In the White Cloud Peaks case, a more sophisticated concept of congestion, measured by expected numbers of trail encounters and camp encounters, is used. The larger the number of either of these kinds of encounters, the lower the value of the "wilderness" experience. The authors define optimal capacity as "the point at which the incremental cost to others from an additional user just equals incremental benefit" (p. 165). It can be shown that this concept of optimality can be achieved by charging user fees but not, in general, by other means (Tybout, 1976). However, the authors do not propose to charge user fees, and it is not national policy to attempt to control congestion by the use of fees. Unless some method of controlling congestion is employed, calculations of the kind done by Krutilla and Fisher will not have practical significance in bringing about optimal use.

The Mineral King study illustrates this point, although in a way not intended by the authors. Two alternative uses, preservation and developed recreation (skiing), are considered. The authors find, by a simultaneous system application of the travel-cost method, that attendance at the proposed ski resort would be 194,000 visitors on a winter weekend, whereas capacity had been pegged at 25,000. They then calculate annual benefits from capacity attendance in accordance with the postulate that "access to the area [is] rationed in such a way that each prospective visitor [has] an equal chance of admission . . ." (p. 209). The effect of this approach is to multiply the estimate of unconstrained benefits by the ratio 25,000/194,000. They compare the resulting benefits with the project costs and then add the displaced wilderness values, which must, of course, also be included as costs of development. The comparison favors preservation, but this is open to question by virtue of the apparent arbitrariness in their treatment of congestion. I cannot think of any practical method of limiting access that would approximate random selection. Nor do the authors explain why they attach enough empirical significance to their postulated method of rationing to base calculations of benefits on it.

A more likely method of rationing would be according to willingness to pay or willingness to make sacrifices of some other sort. If admission to the developed ski site were estimated on the basis of a demand and supply equilibrium produced by high-enough admission fees (or ski tow charges, or room rents, or any of many other ways that excess demand can be turned to the advantage of the seller), the effect would be to selectively give access to those with the highest value of service. This in turn would produce benefit estimates higher, conceivably much higher, than the authors use. Whether benefits so calculated would be high enough to reverse their finding for preservation is an open question.

For any project where congestion is a problem, it is necessary to know how access will be rationed before one can estimate benefits. If rationing takes the form of admission fees, that is one thing. If it takes the form of waiting in line, that is another. If it takes the form of capacity to endure crowds when one came for solitude, that is still a third. Benefits will, in general, be different in each of the three cases. It can be shown that the second and third forms of rationing are inferior to the first (Tybout, 1976). Whether the third is inferior to the second, or *vice versa*, is an empirical matter, about which no general statement can be made.

A final observation on Krutilla and Fisher's book is that methodological contributions are made in the case studies. The simultaneous system travel-cost method in the Mineral King study has already been mentioned. The Hells Canyon benefit-cost analysis was framed in such a way as to take account of changes over time in the relative proportions of thermal power and hydropower in the Northwest power grid with and without development. The absence of congestion data in the White Cloud Peaks study was remedied by the adaptation of

results from a similar location. The models employed in the prairie wetlands and Alaskan pipeline cases are of interest for their own sakes.

The authors observe in their preface that "this volume doubtless raises more issues than it is capable of resolving satisfactorily" (p. v). So it is with first-generation works. But whatever the ratio of issues resolved to issues raised, this book is a step forward in our efforts to base the preservation-development decision on more comprehensive economic concepts. The more comprehensive the concepts, the better the case for preservation.

### REFERENCES

- Tybout, R. A. (1972). Pricing pollution and other negative externalities. *Bell Journal of Economics and Management Science* 3: 252-266.
- Tybout, R. A. (1976). Quasi-public goods; Pricing the commons. In Thrall, R. M., Heady, E., Schad, T., Schwartz, A. K., Thompson, R. G., (eds.), *Economic Modelling for Water Policy Evaluation*, Vol. 3, Amsterdam, North Holland/ TIMS Studies in Management Science.

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