one he himself represents. The real "sexual politics"—pace Kate Millett—is the way the regulation of sexual activity, sexual access, and marital arrangements serve to put people in their political and economic places. Had Symons recognized this, he might not, for example, have tried to show that rape has more to do with a desire for sex than with a desire for power but would instead have addressed himself to how and why the two are intertwined. For a case close to home, I recommend Eldridge Cleaver's *Soul on Ice*. For a case away from home, there are ethnographies of South American Indian groups among whom the institution of gang rape has been reported. Among the Mundurucú of Brazil, gang rape has been seen both by the ethnographers and by the Mundurucú themselves as a way of keeping the women in line. One way Mundurucú women get out of line is by being sexually forward and promiscuous. One can see how Symons would account for a husband's interest in having such behavior punished (though not why he should agree to this particular form of redress) and for a nonhusband's interest in punishing someone else's wife in such a way, but all this means is that someone's interest is bound to be served by whatever sex happens to be going on. The Mundurucu case also reminds us that if a woman does not actively seek sex with a variety of partners some mechanism other than an innate lack of desire may be at work.

If Symons has failed to see sex as what we might call, in the words of the French ethnologist Marcel Mauss, a "total social fact," the fault is not entirely his. Social scientists themselves have failed to accord it the significance it deserves in their empirical research and theory building. (The same point might be made with respect to the paucity and uneven quality of the cross-cultural data on erotic activity and attitudes presented in Symons's book. Symons has industriously sought out good material on this subject, but there just wasn't that much of it to find. True, this may not be the easiest subject to study, but that is not the whole story.)

What is Symons's fault is his attempt to avoid seeing *anything* as a social fact. Observing, quite correctly, that concepts like "society" and "culture" have commonly been reified in social scientific writings, Symons has apparently decided that they are more trouble than they are worth. He does not seriously entertain the idea that they may be necessary analytic tools for understanding human action. On the contrary, whereas Durkheim told us that a belief in God is really a belief in society, Symons in effect tells us that a belief in Durkheim is really a belief in God. After all, who ever "saw" a "society"?

Consider the argument in which Symons claims that the sexual double standard is not determined by "culture" but by "the cumulative history of individuals attempting to influence one another through language" (p. 230). It is hard to imagine what Symons's understanding of "language" is, and this reader would just as soon not try to find out. The point is that the only perspective Symons seems able to take toward human action is that of individual interest. Since one cannot derive the culturally patterned social arrangements within which human beings define and pursue their purposes from some prior principle of individual interest, reproductive or otherwise, Symons, understandably enough, cannot accommodate any concept like "society" or "culture" in his view of the human scene. His work is thus not likely to commend itself to social scientists unless they decide to pack it all in and return to utilitarianism.

Lest I be suspected of a dogmatic opposition to viewing human behavior in biological perspective or looking for universals, let me emphasize that the real issue is how much we learn about what. In this light, I would submit that, if there is in fact a basic and recurrent "men's problem," Symons may have gotten it backwards. Instead of seeing the quintessential male odyssey as an attempt to spread one's seed as far and as wide as possible, we might see it as the quest for paternity, for parlaying what may be a minimal physical investment into a maximal social asset. We may then question Symons's sociobiological transmogrification of the old husbands' tale that marriage is essentially a women's institution that men have to be tricked into somehow. On the contrary, marriage can be seen less as the way women get sexual partners to help take care of the kids—after all, others can do that—than as the way for men to be not merely genitors but fathers.

This approach goes a lot farther than Symons's in accounting for such widespread human institutions as bride-wealth, bride service, and male rituals in which men "give birth" to the next generation of males—rituals that seem at once to assert the primacy of social reproduction over natural reproduction and yet reveal an attempt to mimic, to capture, the female role. In fact, one can view sociobiology itself from this angle, as a symbolic appropriation of what, on the surface, appears to be women's dominant role in reproduction. For the sociobiologist (who is, as it happens, usually male), women are reproductively dependable, but boring. The real action is with the men. Some of them may be total losers, but they are the only ones who can be big winners. I hasten to add that inquiring into the possible ideological significance of sociobiological theories does not in and of itself address the question of their correctness; that must be done on other grounds. However, it is a perfectly legitimate enterprise, since social scientists are as justified in interpreting human behavior, including that of natural scientists, from the vantage points of their own disciplines as biologists are in attempting to analyze human behavior in biological terms.

Despite all the problems with Symons's book, it nonetheless contains much of interest. He presents a lucid outline of major concepts in sociobiology and also offers some useful criticisms of the way in which such terms as "altruism" and "selfishness" have been transferred from ordinary language to scientific discourse. His chapter on the female orgasm is particularly worthy of attention. His thesis that the female orgasm is not "adaptive" in the strict evolutionary sense may be open to debate, but he presents a cogent and persuasive argument. Some of the opposition to it will come from those who have a need to sanctify their pleasures by recourse to "science" or "nature"—but that is their problem. I should think that all students of human sexuality, whatever their theoretical persuasions, would want to read this book, since, in science as in sex, one can find something intriguing and provocative without necessarily considering it to be right.

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Oceanic Peoples

The Prehistory of Polynesia. JESSE D. JENNINGS, Ed. Harvard University Press, Cambridge, Mass., 1979. viii, 400 pp., illus. $35.

The editor of this attractive collection of original essays tells us it was assembled at the behest of Harvard University Press to fill the need for an "organized presentation of findings" on the origins and prehistory of the Polynesian-speaking peoples of the South Seas. "I invited the individuals whose chapters follow to join me in the preparation of a book that

would be written simply, as free of jargon as possible, to serve as an introductory text for undergraduate students and perhaps as a guide for use in graduate seminars. It ought also serve to introduce the lay reader to the entire field." While these aims sound suspiciously like what you must say to a publisher these days to get any work of multiple authorship accepted, that in no way detracts from their legitimacy. Does the resulting volume live up to its goals?

The Prehistory of Polynesia has 15 chapters by 13 authors, as well as an introduction and an epilogue by Jesse Jennings, the editor. The introduction is more a résumé of the contributions than an explanation why knowledge about these small numbers of people living on tiny islands scattered across an ocean that covers one-third of the earth's surface has scholarly, scientific value in addition to its obvious romantic charm. Peter Bellwood's opening chapter, "The Oceanic context," far from setting the scene for those that follow as its title intimates, reads like a lecture in a survey course on the peoples and cultures of the world. Bellwood gives us no general sense of problem, or of intellectual excitement, or of conflicting interpretations and scholarly debate.

A news story, they say, should answer five questions. Some archeologists pursue answers to *when?* and *where?* more vigorously than to *who?*, *what?*, and *why?* Roger Green tries in the second chapter, entitled "Lapita," to answer all five with regard to the makers of the ancient pottery in the Pacific that goes by that name. Theirs is an important story because the Lapita makers who settled Fiji, Tonga, Futuna, and Samoa may have been the ancestors of the modern-day Polynesian-speaking peoples. Unfortunately, Green often appears to confuse his own interpretations with what are sometimes naïvely called the facts of prehistory. For instance, the discovery of Lapita pottery archeological sites on raised coral platforms, marine terraces, and marine sand beaches seems remarkable to him, though human settlement in such locations is so usual in the Pacific that, as he documents, it is rare to find Lapita deposits undisturbed by later human and natural events. Early in his discussion, he asserts that Lapita settlements were situated as they were "so as to maintain ready access to the sea and permit launching and beaching of large canoes." By the end of the chapter, this undocumented claim has been transmuted even farther from the established facts: the people behind the pottery "represent a migration of an innovative kind that has allowed the filling of little-used niches in Melanesia, as well as the pioneering of unsettled islands beyond."

Green's contribution lies in showing us how serious are the gaps and flaws in the archeology of Lapita pottery. Unhappily, he pushes the evidence, such as it is, quite hard in a single direction. He proposes that remains (apparently sporadic) of imported pottery, obsidian, and other raw materials at Lapita sites indicate "a network of reciprocal exchanges between related communities that maintained frequent contact." He has a habit of giving his conclusions first in a way that makes them seem irresistible and then following them up with summary observations about data that appear to lead in the interpretative direction he now favors. One should, for example, first read carefully the paper he refers to on new sites with Lapita pottery before trying to decide about the proposed west-to-east trend in "distance decay" within the Lapita pottery design system. Further, it is unclear how effectively he interprets the theory behind network analysis when he attempts to analyze the distribution of known Lapita sites between New Britain and the Samoan Islands as a network of regional "centers" (his figure 2.12).

The next seven chapters by six different authors review the archeological record for major island areas in Polynesia: Fiji (Everett Frost), Samoa and Tonga (Janet Davidson), the Marquesas (Yosihiko Sinoto), Easter Island (Patrick McCoy), Hawaii (David Tuggle), the Society Islands (Kenneth Emory), and New Zealand (Davidson). On the whole, these are good summaries by people who

A petroglyph boulder at Tipaerui, Tahiti. The figure "appears to be twins attached back to back, with an appendage that looks like an umbilical cord." According to "a local tradition . . . it was carved in memory of the wife and twin children of one Tetaurii, but it may well be a place where attached twins were delivered at birth. Also noteworthy is the fact that the human figure is represented by double lines outlining the body, a technique used in the Marquesas." [Bishop Museum photograph by K. P. Emory, from *The Prehistory of Polynesia*]

clearly ought to know what they are talking about. Notable for their clarity, depth, and intelligence are the chapters by Davidson and McCoy. Frost's story of the Fijians is flawed by the facile assumption that they are a "mixture" of Melanesian and Polynesian elements. This old-fashioned idea prejudices him to look more for "ceramic intrusions from Melanesia" in "the [*sic*] Fijian archaeological sequence" than for patterns of local cultural evolution and local variability from one part of the archipelago to another, patterns that are certainly observable in the biological and most recent linguistic evidence.

The final six chapters are synoptic reviews treating the origins and interrelationships of the Polynesian-speaking peoples from different specialist perspectives: linguistics (Ross Clark), physical anthropology (William Howells), ecology (Patrick Kirch), settlement patterns (Bellwood), sailing (Ben Finney), and Melanesian archeology (Peter White). For many, these will be the most exciting chapters, because they deal with themes of broad interest. Howells comes to the spirited defense of an idea codified 150 years ago by Dumont d'Urville, long before the great antiquity of mankind in the Pacific region or the complexities and uncertainties of human population biology were recognized. This idea is the popular belief that modern Polynesian-speaking peoples belong to an ancient race that had "no important gene exchange with Melanesians before or enroute to their colonization of Polynesia proper." In truth, however, we don't know if precisely the opposite interpretation of current biological differences within and among "Polynesians" and so-called "Melanesians" is not just as likely. Indeed, deriving modern Polynesian-speaking peoples from a paleo-population living somewhere in Melanesia from whom at least some present-day Melanesian peoples are also similarly derived makes much better sense of more data from linguistics, biological anthropology, archeology, and other fields than does the interpretation Howells champions. In any case, as Bellwood says earlier in this volume, "One is forced to conclude that the original relations between Polynesians and Melanesians are obscure, to say the least."

What is missing from this volume is a strong sense of intellectual excitement and scientific purpose. As the chapters by Kirch and Finney in particular reveal, all the ingredients are there. Regrettably, the editor has not been able to serve them up in a gourmet meal of provocative delights. This collection is a worthy and useful addition to any Pacific specialist's library. Lay persons and students, however, will find that dollar for dollar there is more value in Peter Bellwood's *Man's Conquest of the Pacific* (Oxford University Press, 1979), however squarely that monumental survey falls into the "culture-historical" tradition justly criticized by anthropologists.

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Inhibitory Neurotransmitter

GABA. Biochemistry and CNS Functions. Proceedings of a symposium, Strasbourg, France, July 1978. PAUL MANDEL and FRANCIS V. DEFEUDIS, Eds. Plenum, New York, 1979. xii, 506 pp., illus. $42.50. Advances in Experimental Medicine and Biology, vol. 123.

Gamma-aminobutyric acid (GABA) is an inhibitory neurotransmitter that appears to be involved in a greater number of central neuronal synapses (and therefore in a greater number of complex functions) than any other transmitter candidate. It has also become increasingly implicated, through its apparent role in the action of anxiolytic, anticonvulsant, and convulsant drugs, in such disorders as seizures and anxiety.

This book contains short papers on the enzymology, metabolism, uptake and release, receptors, and behavioral correlates of GABA. Also included are reports of recent advances in some technically difficult aspects of neurochemistry: measurement of neurotransmitter release following sensory stimulation (no GABA release could be demonstrated, according to Abdul-Ghani *et al.*), brain circuitry and neurotransmitter interactions, and the relationship of GABA to behavior and neurological disorders (a subject deserving greater treatment). For most of these subjects, the book contains neither a comprehensive review nor many major new contributions. It does provide a forum for many European workers in the field, notably the Strasbourg people (nine papers), who may have been underrepresented in other books on GABA.

The papers on the GABA-metabolizing enzymes—L-glutamate decarboxylase (Sze; Blindermann *et al.*), GABA-transaminase (Maitre *et al.*; Schecter *et al.*), and succinic semialdehyde dehydrogenase (Cash *et al.*)—are informative, and those on GABA uptake (Schousboe; Hösli and Hösli) and turnover (Moroni) are quite well done. But the section on GABA receptors is certainly best.

The excellent discussion of pre- and postsynaptic inhibition (Krnjević) provides the only instance in the book of an informative and cogent review of the field. Certainly more neurophysiogical studies on the action of GABA ought to have been included; the only other contribution on the subject is an erudite analysis (of previously published data) by Werman. GABA-receptor binding studies are well covered, with complete discussions of drug specificity (Krogsgaard-Larsen and Arnt; Bowery *et al.*), brain regional differences (Enna), and assay techniques (Möhler; Maurer; Costa and Guidotti; and others). The paper by Costa and Guidotti provides the only treatment (and a very short one) of GABA-benzodiazepine receptor interactions, a subject dominating more recent GABA symposia and probably future GABA meetings as well.

The book deserves a place on library shelves along with the several other recent treatises on GABA. The coverage of the field is relatively incomplete, and advances since the papers were presented in July 1978 are of course not included, so interest in the volume will probably be limited to GABA specialists (of which, however, there are now quite a few).

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Disordered Materials

Electronic Processes in Non-Crystalline Materials. N. F. MOTT and E. A. DAVIS. Second edition. Clarendon (Oxford University Press), New York, 1979. xiv, 590 pp., illus. $65. The International Series of Monographs on Physics.

There are several branches of physics in which the first definitive textbook was by Mott and someone, and these books served for many years as the best introduction to their subjects. In a sense, this book by Mott and Davis stands with these earlier works because, although many comprehensive reviews of particular aspects of the subject have appeared since the first edition came out in 1971, there has been no one else bold enough to attempt an overall view of the subject of disordered materials—in fact, there