

Preface

Before concluding, let me refer briefly and superficially to what is perhaps the core of all investigations of culture: can we ever arrive at any satisfactory knowledge of what constitutes human nature? To say with Boas and so many ethnologists and sociologists that the cultural pattern hides this knowledge from us forever is a counsel of despair. Some significant light can surely be shed, even if today the technique for this type of investigation has not as yet been perfected.

— Paul Radin, *The Method and Theory of Ethnology*

A book that is simultaneously about incest, a Finnish anthropologist/philosopher, and an esoteric Chinese custom may seem at first to epitomize the sort of arcana for which anthropologists have a deservedly bad reputation. This book, however, does not. It is addressed to issues that are at the heart of several current social and scientific controversies. Unfortunately, it is not easy reading. Of necessity, the argument ranges over a wide intellectual terrain, passing in its course through the complexities of Chinese marriage customs, the organization of the household registers created on Taiwan by the Japanese colonial government, a theory of divorce inspired by British interpretations of African evidence, a number of debates concerning the social and biological determinants of fertility, a debate about the extent to which the quality of childhood attachments affects adult behavior, a controversy concerning the presence of critical periods in human development, the politically sensitive

question of the extent of innate differences between the sexes, a large literature on inbreeding avoidance among primates, an even larger literature dealing with the incestuous abuse of children, and, in the concluding chapter, the vexed question of how behavioral tendencies come to have moral representations. Worse yet for many readers, the evidence that powers the argument comes in the form of a phalanx of daunting tables comparing age-specific fertility and the 25-year probability of divorce among diversely defined populations. I have taken pains to guide the reader along the smoothest path, but this has created a problem of another kind. The first reader of the manuscript, Hill Gates, my wife, found the style of the argument less seductive than authoritarian. "One feels frog-marched through the pages." Her strong recommendation was that I write a preface explaining why the reader should submit to such abuse.

There are six reasons. First, the book shows what anthropology is capable of could we afford really adequate data—if we were funded as a science rather than at the level of a schoolteacher's summer vacation. By prevailing standards, I have had the benefit of generous support, as acknowledged below. Yet the fact is that this book could not have been written with the resources available to me as an anthropologist had it not been for the archives compiled by the Japanese colonial government on Taiwan. To duplicate these archives would cost at least as much as a nuclear-powered submarine or a linear accelerator. Social scientists are castigated by natural scientists for the weakness of their methods and conclusions, but the strength of those of the natural sciences comes at the cost of billions of dollars.

Second, the book demonstrates that the seemingly esoteric character of much anthropological research does not mean that it has little or no social value. The focus of this book is a form of marriage that is now defunct and was formerly found only in South China and parts of the Korean peninsula. Yet the conclusion points to a better understanding of and possible solution to the problem of incestuous child abuse. The concern that leads most funding agencies to concentrate their resources on studies of the perpetrators and their victims is laudable but naive. What is needed to deal with this and many similar problems is a program of research that proceeds from general hypotheses and is prepared to take advantage of relevant data wherever they are found.

Third, this study repudiates the view that the boundaries partitioning scholarly activity into "fields" are anything other than arbitrary and artificial. Sinology gave me access to my data and an understanding of the context that produced them; social anthropology provided the concepts needed to conceptualize the various forms of marriage found in China; demography contributed the quantitative techniques used to test my hypothesis; psychology helped me interpret the Westermarck hypothesis and draw out its testable implications; sociology suggested several alternative hypotheses and ways of evaluating them; biology inspired a critical revision of the hy-

pothesis as well as an explanation of its power; and philosophy of science prepared me to formulate problems in ways that make them solvable. My hope is that on reading this book, students in the social and biological sciences will realize that compressing knowledge into a "discipline" is as unnatural and unjustified as the old Chinese custom of foot binding.

Fourth, the book shows how little can be learned by studies limited to an analysis of the ideas a society presents as its valued heritage. The standard account of marriage in late traditional China—enshrined in many anthropological texts as well as in Confucian ideology—says that women were raised by their natal families, married and transferred to their husband's family at or shortly after puberty, bore children all of whom took their descent from their father, and, after a lifetime of conjugal loyalty, were enshrined as ancestors on their husband's family's altar. By this account, premarital sexual relations, adultery, and divorce were all very rare. The evidence of the 14,402 women included in this study shows, however, that very few women were in fact raised by their natal families, and that these few included many whose husbands married uxorilocally (which is to say that these men went to live in their wife's home and became members of her father's household). It also shows that premarital sexual relations and adultery were common, and that women who failed to bear male children were more likely than not to be divorced.

Fifth, the book disproves a basic assumption of Oedipal theory and thus, in my view, undermines Freudian psychology. In 1917, Edward Sapir, reviewing Oscar Pfister's *The Psychoanalytic Method*, noted that "Freudian psychology has travelled a course that might have been predicted with tolerable certainty. At first received with mingled derision and disgust, it has now attained a position not only of virtual security, but . . . of very genuine and widespread popularity." This worried Sapir. He suggested that "what is sorely needed at the present time, or will be before many years, is a thoroughly objective probing" aimed "at the rigorous elimination of all aspects of Freudian theory that seem dispensable or ill-substantiated."¹ I believe that objective probing is now complete. I also believe it shows that when the ill-substantiated aspects of Freudian psychology are eliminated, very little remains.

Sixth, and finally, the book gives a positive answer to Radin's question: Yes, we can arrive at satisfactory knowledge of what constitutes human nature. Franz Boas's counsel, like that of his many latter-day heirs, was a counsel of despair. Far from hiding from us the knowledge we seek, cultural patterns reveal as nothing else does our species-specific nature. This is not because culture is a weak determinant of behavior, but precisely because it is such a strong determinant. Riding roughshod over our natural inclinations, institutions like the one analyzed in this book reveal, by the wounds they inflict, our common nature. The unpalatable part of the lesson for most cultural anthropologists is that "the techniques for this type of investigation" turn out to be essentially the same as those employed by the natu-

ral sciences. My view as author is that this book goes a long way toward justifying the methodological perspective of what Ernest Gellner calls "atomistic metaphysics."²

An anonymous reviewer of the first edition of Westermarck's *The History of Human Marriage* decided that it was the result "of the earnest labour of the chief part of a lifetime."³ In fact, Westermarck was only 29 years old when the book appeared. The reviewer's remark is more appropriately applied to my effort than to Westermarck's. Conceived of during the course of field research begun when I was 25, this book will not finally appear in print until I am 63. I mention this because it is impossible for me to acknowledge the assistance of all the people who have contributed to an effort that has in fact occupied the chief part of a lifetime. I do not now remember the names of many of the students and colleagues with whom I have discussed my work, let alone who said what and with what effect on my work. The best I can do is to say that the book owes a great deal to former students and colleagues at Cornell University, the London School of Economics and Political Science, Stanford University, Oxford University, Taiwan National University, Cambridge University, and the Institute of Ethnology in Taiwan.

The people who have contributed most decisively to the argument of the book are my critics. I am grateful for their criticism but will not repeat their names here because they appear prominently in the text. However, I must take this opportunity to thank seven people who took the time to read a draft of this book and contributed to its style as well as to its content. They are Patrick Bateson (who reviewed the manuscript for Stanford University Press), Melissa Brown, Silver Damsen, Hill Gates, Anne Pusey, Paul Rosenblatt, and John Shepherd. I also want to thank in a different tone fourteen people whose contribution to my education made this book possible. They are, in the order in which I first knew them, Glen G. Guymon (who helped me take my first step toward academia), Sidney Mellor (who taught me to write and to care about writing), Alvin Hunter (who introduced me to both anthropology and psychology), Margery Wolf (who shared with me much of the field research on which this book is based and whose contribution will be obvious to everyone who has read her work), Lauriston Sharp (who suggested that I study China and taught me to write grant applications), Harold Shadick (who introduced me to Chinese culture and the Chinese language), Knight Biggerstaff (who tutored me in Chinese history and provided encouragement at a critical time), Robert J. Smith (who taught me anthropology and showed me the need for getting the context right), William W. Lambert (who taught me psychology and how me to ask questions that are worth answering), Wang Shih-ch'ing (who introduced me to Taiwanese culture), G. William Skinner (who gave me the advantage of another perspective on China), Maurice Freedman (who taught me British social anthropology and treated me as an academic son), Chuang Ying-chang (who as my collaborator in related projects contributed to the accu-

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A.P.W.

