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0521423686 - Sex and Gender Hierarchies - Edited by Barbara Diane Miller

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A generation of feminist research has explored the extent to which the roles – and expectations – of women and men vary across cultures. In this volume, leading anthropologists reflect on the evidence and theories, broadening the conventional field of comparison to include female/male relationships among non-human primates and introducing fresh case studies which range from lemurs to hominids, from Japanese peasants to male strippers in Florida, from skeletal remains of a Korean queen to mother/child conversations in Samoa. They document the rich and often surprising diversity in sex and gender hierarchies among both human and non-human primates.

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Edited by

Barbara Diane Miller

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University of Pittsburgh*



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Contents

<i>List of figures</i>	<i>page</i> ix
<i>List of contributors</i>	x
<i>Preface</i> Barbara Diane Miller	xiii
<i>A note on the discussions at Mijas</i> Elizabeth Colson	xv
Part 1 Overview and theoretical perspectives	1
1 The anthropology of sex and gender hierarchies Barbara Diane Miller	3
2 Sex differences and gender hierarchies among primates: an evolutionary perspective Adrienne L. Zihlman	32
3 The evolution of human gender hierarchies: a trial formulation Marvin Harris	57
Part 2 Sex and gender hierarchies from conception to death	81
4 Blood: cultural effectiveness of biological conditions Brigitta Hauser-Schäublin	83
5 Reproduction and gender hierarchy: amniocentesis in America Rayna Rapp	108
6 Variations in male-female dominance and offspring care in non-human primates Patricia Chapple Wright	127
7 Indexing gender Elinor Ochs	146
8 The politics of polygyny in Mende education and child fosterage transactions Caroline Bledsoe	170
9 Sexual repression, social control, and gender hierarchy in Sambia culture Gilbert Herdt	193
10 Primatological perspectives on gender hierarchies Joan B. Silk	212
	vii

viii	<i>Contents</i>	
11	Conjugal power in Tokugawa Japanese families: a matter of life or death G. William Skinner	236
	Part 3 Complexity and change in gender hierarchies	271
12	Skeletal evidence for sex roles and gender hierarchies in prehistory Mark Nathan Cohen and Sharon Bennett	273
13	Gender hierarchy and the queens of Silla Sarah M. Nelson	297
14	Gender hierarchy in Burma: cultural, social, and psychological dimensions Melford E. Spiro	316
15	Turning the tables? Males strippers and the gender hierarchy in America Maxine L. Margolis and Marigene Arnold	334
16	Women in Samoan history: a further critique of Derek Freeman Eleanor Leacock	351
17	Sanskritization as female oppression in India Gerald D. Berreman	366
	<i>Index</i>	393

Figures

7.1	Indexing gender in Japanese	<i>page</i>	151
7.2	Language and gender		153
7.3	Verbal strategies that constitute mothering		157
10.1	Sexual dimorphism in body weight plotted against the adult sex ratio among non-human primates		218
10.2	Comparison of sexual dimorphism in body weight for primates that normally live in pair-bonded, multi-male, or single-male groups		219
10.3	Extent of sexual dimorphism among contemporary humans compared with extent of sexual dimorphism in pair-bonded, multi-male, and single-male groups of non-human primates		225
10.4	Hypothetical numbers of mates per male in (a) pair-bonded, (b) multi-male, and (c) single-male groups		227
10.5	Expected number of wives per male in societies in which (a) monogamy is common, (b) polygyny is limited, and (c) polygyny is common		228
11.1	Dimensions of the marriage-age matrix		250
11.2	All registered births of second and later parity arrayed according to characteristics of existing offspring at time of each birth, Tokugawa Japanese villagers		253
11.3	Sex of first registered birth, Tokugawa Japanese villagers		256
11.4	Mean age at death, ever-married men, Tokugawa Japanese villagers		260
11.5	Mean age at death, ever-married women, Tokugawa Japanese villagers		261
11.6	Proportion of ever-married women who died as widows, Tokugawa Japanese villagers		262
17.1	Schematic view of gender hierarchy in India		371

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xi

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To the memory of Eleanor Leacock,
pioneer in the anthropology of gender

Preface

There is little need these days for another general collection of anthropological essays on sex and gender. But this volume is important because it comprises chapters from all of anthropology's four fields – archaeology, physical anthropology, social/cultural anthropology, linguistics – and is explicitly motivated by the goal of creating a stronger discipline and better understanding of gender through cross-field comparisons and sharing. Another distinguishing feature of these essays is their focus on hierarchy between and within groups of males and females. Rather than merely documenting male–female differences our concern is with analyzing systematic relationships of inequality based on sex and gender.

The authors of this volume met in Mijas, Spain, in January 1987 for several days of intensive discussion. The financial and organizational help of the Wenner-Gren Foundation for Anthropological Research, Inc., made our meeting possible and supported the preparation of the published papers. The conference occurred at the confluence of Lita Osmunden's and Sydel Silverman's leadership of the Wenner-Gren Foundation, and this project has benefited from the wisdom and guidance of both people.

The Graduate Group in Demography at the University of California-Berkeley (1987–8), the Population and Development Program, South Asia Program, and Department of Rural Sociology at Cornell University (1988–90), the Department of Sociology and Anthropology at the State University of New York at Cortland (1988–9), the Department of Anthropology at Ithaca College (1989–90), and the Department of Anthropology at the University of Pittsburgh (from 1990 on) provided pleasant and stimulating settings as well as institutional support during this book's preparation. A small grant in 1986 from the Appleby-Mosher Fund of the Maxwell School of Citizenship and Public Affairs at Syracuse University supported background library research.

Richard Shweder, co-editor of the Society for Psychological Series, possesses an unusual breadth of intellectual vision and therefore appreciated the diversity of views presented in this book. My editors at Cambridge University Press provided superb guidance throughout the production process: Jessica Kuper saw the value of the book and helped keep it on

course in many ways, Frances Brown was a keen-eyed and conscientious copy-editor, and Jayne Mathews was patient yet firm about turnaround. Near the end, my friend Alf Hildebeitel provided pertinent advice about why one should not prepare an authors index.

The manuscript was word-processed by Esther Gray, of the Metropolitan Studies Program of Syracuse University, in her highly professional way and Colleen Mylan and Cecelia Dugan, of the Department of Anthropology at the University of Pittsburgh, word-processed the index with care. The figures were prepared by Marie J. Zeidler, and the artwork was funded by the Asian Studies Program and the Department of Anthropology at the University of Pittsburgh. My thanks to all these institutions and individuals.

One editorial goal for a collection of essays is that the chapters will speak in “one voice” stylistically, and I hope this unified stylistic voice has been achieved. But in terms of the content and perspective of each chapter, unique voices ring out loud and clear. Each author has his or her own purposes, and each pursues that purpose in a different way. In organizing the Mijas conference, I sought no unified vision of anthropology but instead chose authors representing diversity in their theoretical approach, empirical foundations, and analytic technique. This is not to say that all bases are thereby covered. Any such process of selection must unfortunately involve omissions. The book does provide a rich menu of studies from a variety of contexts – both human and non-human, ancient and contemporary, pre-industrial and post-industrial. A range of topics are explored – mothering and child care, work, health, eroticism and sexuality, intra-family relationships and power, public power, ritual and belief, and change in all of these.

The Mijas conference was in many ways risky and experimental. It convened scholars whose ideas are so diverse and sometimes antagonistic that they may not otherwise have ever sat at the same table to discuss their views. The meeting involved its own hierarchical dynamics based on the culture of human interaction at an academic colloquium and often clearly tied to gender. Were we authors to meet again, our discussions would be different because we have benefited immeasurably from lessons gained at Mijas. At a second meeting, we would be better able to bridge gaps between the fields and see the crossovers, linkages, and contrasts that at first came only with some difficulty. We have learned much from this project about the discipline of anthropology as a whole, and we hope this book will contribute to the growth of anthropology and its role in promoting understanding of gender hierarchies in all their complex and changing manifestations.

Barbara Diane Miller

A note on the discussions at Mijas

Elizabeth Colson

I am concerned here with certain implications of what was said at Mijas, rather than with the contents of the conference papers. We were asked to consider how the exploration of a particular subject, gender hierarchies, could contribute to, first, the development of better theory and methodology in anthropology in general, and second, the pulling together of physical anthropology, archaeology, linguistics, and social/cultural anthropology, on the assumption that those working in each subdiscipline would come to recognize the relevance to their own research of knowledge generated in the other fields.

Gender hierarchies were a good choice for such a discussion. We found commonalities that concerned all four subdisciplines. At the same time we were also made aware of the difficulties that lie in the way of communication or collaboration: differences in vocabulary, methodology, and theoretical assumptions.

“Gender” caused relatively few problems as a concept with which all could work, although primatologists may well prefer to use “sex” rather than “gender,” given their concentration on biological phenomena. Even so, Zihlman and Wright convinced me that using “gender” and “gender relations” does not distort the reporting of primate behavior, given that so much of primate social life appears to be constructed.

“Hierarchy” was more problematic and raised a number of issues which we touched on, but did not fully explore. The primatologists preferred to talk in terms of dominance and submission, for these are terms which can be used to rate particular encounters and refer to the situational nature of their observations. Hierarchy implies something institutionalized which governs relationships over time between individuals who consistently accept placement in a system of ranking. It was noticeable that Zihlman and Wright appeared more willing to use hierarchy when describing intra-sex relationships among non-human primates, apparently because such relationships are more stable over time than inter-sex relationships because males and females, much of the time, may go their separate ways and have no need of consistent ranking.

Hierarchy, in fact, has connotations that have consequences for any methodology adopted, because of the kinds of evidence that must be collected if hierarchies are to be demonstrated. This kind of evidence is denied to the primatologist, the prehistoric archaeologist and the sociolinguist involved with the observational study of language learning. On the other hand, these three subdisciplines have a basic methodology in common, for their methods are those of the natural historian. Wright and Zihlman, as primatologists, and Ochs, as a sociolinguist, rely upon detailed observation of sequences of actions and interactions, either physical or vocal, but have no direct access to the meanings the actors impute to what they do. They look and listen but are unable to question those they observe. While archaeologists are precluded from observing behavior, and so have different methods and different problems from the primatologists and linguists, they too work in the tradition of natural history and obtain their data without the intervention of the screen of meaning thrown up by actors other than themselves and their fellow professionals. Nelson, who deals with an era when writing already supplements the archaeological record, found it possible to speak of hierarchies, whereas Cohen could not.

If hierarchy is more than dominance based on physical strength, if it involves legitimation as well as pecking order, then the term commits the primatologists, prehistoric archaeologists, and sociolinguists concerned with language acquisition to extrapolating well beyond their data. Nelson, as an archaeologist who can rely upon what actors said as well as upon the refuse born of their activities, could look in the written record for evidence of how men and women viewed each other in early Korea. She could query whether the centralization of power, with its implication of marked inequalities of rank, necessarily also implied that gender relationships were structured hierarchically. Cohen, as a prehistoric archaeologist, preferred to deal in less abstract terms, asking about specific evidence that males and females at particular times in history were subject to different deprivations or advantages: differential access to food, greater vulnerability to violence, differential exposure to specified sources of stress.

Linguists, however, provide a link between the concerns of primatologists and social/cultural anthropologists. Both groups found immediate relevance in Ochs' ability, as a sociolinguist, to provide linguistic cues for the rating of the presence or absence of dominance and subordination in particular encounters, and for pinpointing the initiator of attempts to shift control. The prehistoric archaeologists, debarred as they are from contact with their actors' discourse, found less relevance in this contribution of the linguist, but Nelson could see the relevance for interpreting the early written records.

Unlike their colleagues in the other three subdisciplines, the social/cultural anthropologists rely strongly upon questions put to actors: what are you doing, why are you doing it, what do you think will happen as a consequence, how do you categorize yourself and your fellow actors, including those invisible to the observer, etc., etc. Inevitably the number of variables multiplies.

Representatives of the other three subdisciplines demanded greater precision in definition, in the setting out of criteria on which the presence or absence of hierarchy could be empirically determined. The social/cultural anthropologists responded that what happens includes the actors' definitions, their sets of criteria, and these cannot be known in advance. Herdt, Hauser-Schäublin, and Spiro added that to understand what is happening one also needs to consider the individual historical experience that empowers meaning with emotion. Margolis and Rapp pointed to how what one sees takes meaning from the larger social context in which, for example, gender relationships are set. Newly introduced forms of technology, and any other form of activity, become quickly permeated with existing meanings. They spoke to the interconnectedness across whole ranges of activity, showing how particular actions can come to stand for larger systems of relationships. Therefore, we do not observe events circumscribed in time since situations have a past and future, and meaning in part refers to that continuity.

Berreman, Spiro, Herdt, and Hauser-Schäublin were particularly concerned with the way this interconnectedness serves to stabilize gender relations into hierarchies. The last three dealt more with how the hierarchy is legitimized through ideology and ritual. To Berreman, any form of hierarchy or institutionalized inequality is a form of domination which can be maintained only by the exercise of force: therefore, legitimacy is the ideology created by the oppressors and is always transparent to the oppressed. Those who may interact can and do impute very different meanings to what is happening.

In less extreme form this was a recurrent theme: hierarchy is not a unitary phenomenon in any society because its meaning shifts according to placement in the system and, for that matter, actors may also disagree about placement. If much of the existing ethnographic description tends to speak for some omnipotent representative culture bearer, so much the worse for that ethnography. As Sapir pointed out in the 1920s, each experiencing individual exists within a unique cultural world. When we reached this point, however, Harris and Leacock reminded us that whatever meanings individuals may attribute to what they do, they must act within institutionalized frameworks which determine opportunities and limit action, and these frameworks are subject to historical processes. As

technology changes, so does the possibility for accumulating and exerting political power, and this process in turn affects other fields of relationships, including those of gender. Myths arise to explain the distribution of power and have their own ability to generate action. Spiro suggested that these myths can then spread, even to regions with very different economies and political organization.

Wright tried to engage the social/cultural anthropologists in the examination of variations in gender relationships according to such variables as ecology, population size, indices of political centralization, degrees of specialization of labor. By and large they did not respond, although in the papers before us we had a demonstration of the difficulties of such an exercise. Spiro, Berreman, Herdt, and Hauser-Schäublin reported the existence of a complex of ideas about gender which includes the belief that women are polluting and males vulnerable since semen can be depleted, and consequently that males need to be segregated and purified from female contacts. This pattern is found among state societies of high complexity, such as India and Burma, and in much of Papua New Guinea, where hierarchical political control was minimal; in temperate and tropical climates, where division of labor was elaborated and where it was minimal; in areas with highly developed priesthoods, and again where priesthood barely existed. Moreover, the complex existed both where in fact women were subject to real exclusions and subordination, and where women had much freedom and exercised dominance over household affairs, as in Burma.

In other words, although we can predict many things about a society if we know a few things about technology, centralization of political power, and the basic units of kinship, we cannot predict from these how men and women will regard each other, or how they will deal with one another. And, despite the persuasiveness of Spiro and Herdt, I doubt that we can predict such relationships from knowing how children are treated in infancy – equally dependent infants grow into very different kinds of adults.

Bledsoe and Leacock suggested some of the reasons for our difficulties in dealing with gender hierarchies as static phenomena for purposes of comparison when they focused on the way humans develop strategies for coping with such systems of social relations as they find, seek to alter these to their own advantage, and in doing so change the system. They left open the question of the relationship between ideologies and strategies, the degree to which ideologies can control when in fact advantages have shifted. One could also ask whether many people care about what others think if this does not interfere with what they themselves wish to do, a question that Spiro raised and left unanswered.

The conference suggested some interesting lines on which to work in examining degrees of freedom, although this left the archaeologists somewhat unhappy since they can extrapolate most freely if behavior is highly predictable. Recent research on primates indicates that sexual behavior is more variable among chimpanzees than among other non-human primates, that among them dominance/submission patterns are less consistent across genders, and that behavior in general is more variable. Zihlman emphasized flexibility and situational opportunism as characteristics of our closest primate kin. These are also the mark of the human species and have given it evolutionary advantage. Highly specialized, rigid behavior has its costs. Evolution, if it favors diversity, implies choice, and therefore the unlikelihood of long-term functional adjustments. Ideology, or myth, then is always out-of-date and always an extreme statement of an untenable position. Perhaps this is why it is enunciated so passionately by those who are given the leisure through the division of labor to elaborate it.