

Simians, Cyborgs, and Women

The idea that nature is constructed, not discovered – that truth is made, not found – is the keynote of recent scholarship in the history of science. Tracing the gendered roots of science in culture, Donna Haraway's writings about scientific research on monkeys and apes is arguably the finest scholarship in this tradition. She has carefully studied the publications, the papers, the correspondence, and the history of the expeditions and institutions of primate studies, uncovering the historical construction of the pedigrees for existing social relations – the naturalization of race, sex, and class. Throughout this book she is analysing accounts, narratives, and stories of the creation of nature, living organisms, and cyborgs (cybernetic organisms: systems which embrace organic and technological components). She also looks critically at the immune system as an information system, and shows how deeply our cultural assumptions penetrate into allegedly value-neutral medical research. In several of these essays she explores and develops the contested terms of reference of existing feminist scholarship; and by mapping the fate of two potent and ambiguous words – 'nature' and 'experience' – she uncovers new visions and provides the possibility of a new politics of hope.

Her recent book, *Primate Visions*, has been called 'outstanding', 'original', 'brilliant', 'important' by leading scholars in the field. *Simians, Cyborgs, and Women* contains ten essays written between 1978 and 1989. They establish her as one of the most thoughtful and challenging feminist writers today.

Donna Haraway is a historian of science and Professor at the History of Consciousness Board, University of California, Santa Cruz. She received her doctorate in biology at Yale and is the author of *Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology* and *Primate Visions: Gender, Race, and Nature in the World of Modern Science*.

Simians, Cyborgs, and Women

The Reinvention of Nature

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For my parents,
Dorothy Maguire Haraway (1917–1960)
and
Frank O. Haraway

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Introduction

This book should be read as a cautionary tale about the evolution of bodies, politics, and stories. Above all, it is a book about the invention and reinvention of nature – perhaps the most central arena of hope, oppression, and contestation for inhabitants of the planet earth in our times. Once upon a time, in the 1970s, the author was a proper, US socialist-feminist, white, female, hominid biologist, who became a historian of science to write about modern Western accounts of monkeys, apes, and women. She belonged to those odd categories, invisible to themselves, which are called 'unmarked' and which are dependent upon unequal power for their maintenance. But by the last essays, she has turned into a multiply marked cyborg feminist, who tried to keep her politics, as well as her other critical functions, alive in the unpromising times of the last quarter of the twentieth century. The book examines the breakup of versions of Euro-American feminist humanism in their devastating assumptions of master narratives deeply indebted to racism and colonialism. Then, adopting an illegitimate and frightening sign, the book's tale turns to the possibilities of a 'cyborg' feminism that is perhaps more able to remain attuned to specific historical and political positionings and permanent partialities without abandoning the search for potent connections.

A cyborg is a hybrid creature, composed of organism and machine. But, cyborgs are compounded of special kinds of machines and special kinds of organisms appropriate to the late twentieth century. Cyborgs are post-Second World War hybrid entities made of, first, ourselves and other organic creatures in our unchosen 'high-technological' guise as information systems, texts, and ergonomically controlled labouring, desiring, and reproducing systems. The second essential ingredient in cyborgs is machines in their guise, also, as communications systems, texts, and self-acting, ergonomically designed apparatuses.

The chapters comprising Part One of this book examine feminist struggles over the modes of producing knowledge about, and the meanings of, the behaviour and the social lives of monkeys and apes. Part Two explores contests for the power to determine stories about 'nature' and 'experience' – two of the most potent and ambiguous words in English. Part Three focuses on cyborg embodiment, the fate of various feminist concepts of gender, reappropriations of metaphors of vision for feminist ethical and epistemological purposes, and the immune system as a biopolitical map of the chief

systems of 'difference' in a postmodern world. Throughout these diverse contents, this book treats constructions of nature as a crucial cultural process for people who need and hope to live in a world less riddled by the dominations of race, colonialism, class, gender, and sexuality.

Inhabiting these pages are odd boundary creatures – simians, cyborgs, and women – all of which have had a destabilizing place in the great Western evolutionary, technological, and biological narratives. These boundary creatures are, literally, *monsters*, a word that shares more than its root with the word, to *demonstrate*. Monsters signify. *Simians, Cyborgs, and Women* interrogates the multi-faceted biopolitical, biotechnological, and feminist theoretical stories of the situated knowledges by and about these promising and non-innocent monsters. The power-differentiated and highly contested modes of being of these monsters may be signs of possible worlds – and they are surely signs of worlds for which we are responsible.

Simians, Cyborgs, and Women collects essays written from 1978 through 1989, a period of complicated political, cultural, and epistemological foment within the many feminisms which have appeared in the last decades. Focusing on the biopolitical narratives of the sciences of monkeys and apes, the earliest essays were written from within US Eurocentric socialist-feminism. They treat the deep constitution of nature in modern biology as a system of production and reproduction, that is, as a labouring system, with all the ambiguities and dominations inherent in that metaphor. How did nature for a dominant cultural group with immense power to make its stories into reality become a system of work, ruled by the hierarchical division of labour, where the inequities of race, sex, and class could be naturalized in functioning systems of exploitation? What were the consequences for views of the lives of animals and people?

The middle set of chapters examines contests for narrative forms and strategies among feminists, as the heteroglossia and power inequities within modern feminism and among contemporary women became inescapable. The section concludes with an examination of ways of reading a modern Nigerian-British author, Buchi Emecheta, as an example of contests among differently situated African, Afro-American, and Euro-American critics over what will count as women's experience in the pedagogical context of a women's studies classroom. What kind of accountability, coalition, opposition, constituencies, and publishing practices structure particular readings of such an author on such a topic?

Part Three, 'Differential Politics for Inappropriate/d Others', contains four essays. The phrase, 'inappropriate/d others', is borrowed from the Vietnamese film-maker and feminist theorist, Trinh T. Minh-ha. She used the term to suggest the historical positioning of those who refuse to adopt the mask of either 'self' or 'other' offered by dominant narratives of identity and

politics. Her metaphors suggest a geometry for considering the relations of difference other than hierarchical domination, incorporation of 'parts' into 'wholes', or antagonistic opposition. But her metaphors also suggest the hard intellectual, cultural, and political work these new geometries will require, if not from simians, at least from cyborgs and women.

The essays show the contradictory matrices of their composition. The examination of the recent history of the term sex/gender, written for a German Marxist dictionary, exemplifies the textual politics embedded in producing standard reference-work accounts of complicated struggles. The Cyborg Manifesto was written to find political direction in the 1980s in the face of the hybrids 'we' seemed to have become world-wide. The examination of the debates about 'scientific objectivity' in feminist theory argues for a transformation of the despised metaphors of organic and technological vision in order to foreground specific positioning, multiple mediation, partial perspective, and therefore a possible allegory for feminist scientific and political knowledge.

Nature emerges from this exercise as 'coyote'. This potent trickster can show us that historically specific human relations with 'nature' must somehow – linguistically, ethically, scientifically, politically, technologically, and epistemologically – be imagined as genuinely social and actively relational; and yet the partners remain utterly inhomogeneous. 'Our' relations with 'nature' might be imagined as a social engagement with a being who is neither 'it', 'you', 'thou', 'he', 'she', nor 'they' in relation to 'us'. The pronouns embedded in sentences about contestations for what may count as nature are themselves political tools, expressing hopes, fears, and contradictory histories. Grammar is politics by other means. What narrative possibilities might lie in monstrous linguistic figures for relations with 'nature' for ecofeminist work? Curiously, as for people before us in Western discourses, efforts to come to linguistic terms with the non-representability, historical contingency, artefactuality, and yet spontaneity, necessity, fragility, and stunning profusions of 'nature' can help us refigure the kind of persons we might be. These persons can no longer be, if they ever were, master subjects, nor alienated subjects, but – just possibly – multiply heterogeneous, inhomogeneous, accountable, and connected human agents. But we must never again connect as parts to wholes, as marked beings incorporated into unmarked ones, as unitary and complementary subjects serving the one Subject of monotheism and its secular heresies. We must have agency – or agencies – without defended subjects.

Finally, the mapping of the biopolitical body considered from the perspective of contemporary immune system discourse probes again for ways to refigure multiplicities outside the geometry of part/whole constraints. How can our 'natural' bodies be reimagined – and relived – in ways that

transform the relations of same and different, self and other, inner and outer, recognition and misrecognition into guiding maps for inappropriate/d others? And inescapably, these refigurings must acknowledge the permanent condition of our fragility, mortality, and finitude.

Throughout these essays, I have tried to look again at some feminist discards from the Western deck of cards, to look for the trickster figures that might turn a stacked deck into a potent set of wild cards for refiguring possible worlds. Can cyborgs, or binary oppositions, or technological vision hint at ways that the things many feminists have feared most can and must be refigured and put back to work for life and not death? Located in the belly of the monster, the 'First World' in the 1980s and after, how can we develop reading and writing practices, as well as other kinds of political work, to continue to contest for the material shapes and meanings of nature and experience? How might an appreciation of the constructed, artefactual, historically contingent nature of simians, cyborgs, and women lead from an impossible but all too present reality to a possible but all too absent elsewhere? As monsters, can we demonstrate another order of signification? Cyborgs for earthly survival!

Part One

Nature as a System of Production and Reproduction

Chapter One

Animal Sociology and a Natural Economy of the Body Politic: A Political Physiology of Dominance

I want to do something very important. Like fly into the past and make it come out right.

Marge Piercy, Woman on the Edge of Time

The concept of the body politic is not new. Elaborate organic images for human society were richly developed by the Greeks. They conceived the citizen, the city, and the cosmos to be built according to the same principles. To perceive the body politic as an organism, as fundamentally alive and as part of a large cosmic organism, was central for them (Collingwood, 1945). To see the structure of human groups as a mirror of natural forms has remained imaginatively and intellectually powerful. Throughout the early period of the industrial revolution, a particularly important development of the theory of the body politic linked the natural and political economy on multiple levels. Adam Smith's theory of the market and of the division of labour as keystones of future capitalist economic thought, with Thomas Malthus's supposed law of the relation of population and resources, together symbolize the junction of natural forces and economic progress in the formative years of capitalist industrialism. The permeation of Darwin's evolutionary theory with this form of political economy has been a subject of considerable analysis from the nineteenth century to the present (Young, 1969). Without question, the modern evolutionary concept of a population, as the fundamental natural group, owes much to classical ideas of the body politic, which in turn are inextricably interwoven with the social relationships of production and reproduction.

The union of the political and physiological is the focus of this chapter. That union has been a major source of ancient and modern justifications of

domination, especially of domination based on differences seen as natural, given, inescapable, and therefore moral. It has also been transformed by the modern biobehavioural sciences in ways we must understand if we are to work effectively for societies free from domination. The degree to which the principle of domination is deeply embedded in our natural sciences, especially in those disciplines that seek to explain social groups and behaviour, must not be underestimated. In evading the importance of dominance as a part of the theory and practice of contemporary sciences, we bypass the crucial and difficult examination of the *content* as well as the social function of science. We leave this central, legitimating body of skill and knowledge to undermine our efforts, to render them utopian in the worst sense. Nor must we lightly accept the damaging distinction between pure and applied science, between use and abuse of science, and even between nature and culture. All are versions of the philosophy of science that exploits the rupture between subject and object to justify the double ideology of firm scientific objectivity and mere personal subjectivity. This anti-liberation core of knowledge and practice in our sciences is an important buttress of social control.¹

Recognition of that fact has been a major contribution by feminist theorists. Women know very well that knowledge from the natural sciences has been used in the interests of our domination and not our liberation, birth control propagandists notwithstanding. Moreover, general exclusion from science has only made our exploitation more acute. We have learned that both the exclusion and the exploitation are fruits of our position in the social division of labour and not of natural incapacities.² But if we have not often underestimated the principle of domination in the sciences, if we have been less mesmerized than many by the claims to value-free truth by scientists as we most frequently encounter them – in the medical marketplace (Gordon, 1976; Reed, 1978) – we have allowed our distance from science and technology to lead us to misunderstand the status and function of natural knowledge. We have accepted at face value the traditional liberal ideology of social scientists in the twentieth century that maintains a deep and necessary split between nature and culture and between the forms of knowledge relating to these two putatively irreconcilable realms. We have allowed the theory of the body politic to be split in such a way that natural knowledge is reincorporated covertly into techniques of social control instead of being transformed into sciences of liberation. We have challenged our traditional assignment to the status of natural objects by becoming anti-natural in our ideology in a way which leaves the life sciences untouched by feminist needs.³ We have granted science the role of a fetish, an object human beings make only to forget their role in creating it, no longer responsive to the dialectical interplay of human beings with the surrounding world in the

satisfaction of social and organic needs. We have perversely worshipped science as a reified fetish in two complementary ways: (1) by completely rejecting scientific and technical discipline and developing feminist social theory totally apart from the natural sciences, and (2) by agreeing that 'nature' is our enemy and that we must control our 'natural' bodies (by techniques given to us by biomedical science) at all costs to enter the hallowed kingdom of the cultural body politic as defined by liberal (and radical) theorists of political economy, instead of by ourselves. This cultural body politic was clearly identified by Marx: the marketplace that remakes all things and people into commodities.

A concrete example may help explain what I see as our dangerous misunderstanding, an example which takes us back to the point of union of the political and physiological. In *Civilization and Its Discontents*, Freud (1962) developed a theory of the body politic that based human social development on progressive domination of nature, particularly of human sexual energies. Sex as danger and as nature are central to Freud's system, which repeats rather than initiates the traditional reduction of the body politic to physiological starting points. The body politic is in the first instance seen to be founded on natural individuals whose instincts must be conquered to make possible the cultural group. Two recent neo-Freudian and neo-Marxist theorists have ironically reworked Freud's position in illuminating ways for the thesis of this essay: one is Norman O. Brown, the other Shulamith Firestone. Freud, Brown, and Firestone are useful tools in a dissection of the theories of the political and physiological organs of the body politic because they all begin their explanations with sexuality, add a dynamic of cultural repression, and then attempt to liberate again the personal and collective body.

Brown (1966), in *Love's Body*, developed an elaborate metaphorical play between individual and political bodies to show the extraordinary patriarchal and authoritarian structure of our conceptions and experiences of both. The phallus, the head; the body, the state; the brothers, the rebellious overthrow of kingship only to establish the tyranny of the fraternal liberal market – these are Brown's themes. If only the father was head, only the brothers could be citizens. The only escape from the domination that Brown explored was through fantasy and ecstasy, leaving the body politic unchallenged in its fundamental male supremacy and in its reduction to the dynamic of repression of nature. Brown rejected civilization (the body politic) in order to save the body; the solution was necessitated by his root acceptance of Freudian sexual reductionism and the ensuing logic of domination. He turned nature into a fetish worshipped by a total return to it (polymorphous perversity). He betrayed the socialist possibilities of a dialectical theory of the body politic that neither worships nor rejects

natural science, that refuses to make nature and its knowledge into a fetish.

Firestone (1970), in the *Dialectic of Sex*, also faces the implications of Freud's biopolitical theory of patriarchy and repression but tries to transform it to yield a feminist and socialist theory of liberation. She has been immensely important to feminists in this task. I think, however, that she committed the same mistake that Brown did, that of 'physiological reduction of the body politic to sex', which fundamentally blocks a liberating socialism that neither fatalistically exploits the techniques given by sciences (while despairing of transforming their content) nor rejects a technical knowledge altogether for fantasy. Firestone located the flaw in women's position in the body politic in our own bodies, in our subservience to the organic demands of reproduction. In that critical sense she accepted a historical materialism based on reproduction and lost the possibility for a feminist-socialist theory of the body politic that would not see our personal bodies as the ultimate enemy. In that step she prepared for the logic of the domination of technology – the total control of now alienated bodies in a machine-determined future. She made the basic mistake of reducing social relations to natural objects, with the logical consequence of seeing technical control as a solution. She certainly did not underestimate the principle of domination in the biobehavioural sciences, but she did misunderstand the status of scientific knowledge and practice. That is, she accepted that there are natural objects (bodies) separate from social relations. In that context, liberation remains subject to supposedly natural determinism, which can only be avoided in an escalating logic of counterdomination.

I think it is possible to build a socialist-feminist theory of the body politic that avoids physiological reductionism in both its forms: (1) capitulating to theories of biological determinism of our social position, and (2) adopting the basically capitalist ideology of culture against nature and thereby denying our responsibility to rebuild the life sciences. I understand Marxist humanism to mean that the fundamental position of the human being in the world is the dialectical relation with the surrounding world involved in the satisfaction of needs and thus in the creation of use values. The labour process constitutes the fundamental human condition. Through labour, we make ourselves individually and collectively in a constant interaction with all that has not yet been humanized. Neither our personal bodies nor our social bodies may be seen as natural, in the sense of existing outside the self-creating process called human labour. What we experience and theorize as nature and as culture are transformed by our work. All we touch and therefore know, including our organic and our social bodies, is made possible for us through labour. Therefore, culture does not dominate nature, nor is nature an enemy. The dialectic must not be made into a dynamic of growing domination.⁴ This position, a historical materialism based on production,

contrasts fundamentally with the ironically named historical materialism based on reproduction that I have tried to outline above.

One area of the biobehavioural sciences has been unusually important in the construction of oppressive theories of the body politic: animal sociology, or the science of animal groups. To reappropriate the biosocial sciences for new practices and theories, a critical history of the physiological politics based on domination that have been central in animal sociology is important. The biosocial sciences have not simply been sexist mirrors of our own social world. They have also been tools in the reproduction of that world, both in supplying legitimating ideologies and in enhancing material power. There are three main reasons for choosing to focus on the science of animal, especially primate, groups.

First, its subject and procedures developed so as to span the nature-culture split at precisely the same time in American intellectual history, between 1920 and 1940, when the ideology of the autonomy of the social sciences had at last gained acceptance, that is, when the liberal theory of society (based on functionalism and hierarchical systems theories) was being established in the universities. Intrinsic to the new liberal relations of natural and social disciplines was the project of human engineering – that is, the project of design and management of human material for efficient, rational functioning in a scientifically ordered society. Animals played an important role in this project. On the one hand, they were plastic raw material of knowledge, subject to exact laboratory discipline. They could be used to construct and test model systems for both human physiology and politics. A model system of, for example, menstrual physiology or socialization processes did not necessarily imply reductionism. It was precisely direct reduction of human to natural sciences that the post-Spencerian, post-evolutionary naturalist, new ordering of knowledge forbade. The management sciences of the 1930s and after have been strict on that point. It is part of the nature-culture split. On the other hand, animals have continued to have a special status as natural objects that can show people their origin, and therefore their pre-rational, pre-management, pre-cultural essence. That is, animals have been ominously ambiguous in their place in the doctrine of autonomy of the human and natural sciences. So, despite the claims of anthropology to be able to understand human beings solely with the concept of culture, and of sociology to need nothing but the idea of the human social group, animal societies have been extensively employed in rationalization and naturalization of the oppressive orders of domination in the human body politic.⁵ They have provided the point of union of the physiological and political for modern liberal theorists while they continue to accept the ideology of the split between nature and culture.

Second, animal sociology has been central in the development of the most

thorough naturalization of the patriarchal division of authority in the body politic and in the reduction of the body politic to sexual physiology. Thus this area of the natural sciences is one we need to understand thoroughly and transform completely to produce a science that might express the social relations of liberation without committing the vulgar Marxist mistake of deriving directly the substance of knowledge from material conditions. We need to understand how and why animal groups have been used in theories of the evolutionary origin of human beings, of 'mental illness', of the natural basis of cultural co-operation and competition, of language and other forms of communication, of technology, and especially of the origin and role of human forms of sex and the family. In short, we need to know the animal science of the body politic as it has been and might be.⁶ I believe the result of a liberating science of animal groups would better express who the animals are as well; we might free nature in freeing ourselves.

Third, the levels at which domination has formed an analytical principle in animal sociology allow a critique of the embodiment of social relations in the content and basic procedures of a natural science in such a way as to expose the fallacies of the claim to objectivity, but not in such a way as to permit facile rejection of scientific discipline in our knowledge of animals. We cannot dismiss the layers of domination in the science of animal groups as a film of unfortunate bias or ideology that can be peeled off the healthy objective strata of knowledge below. Neither can we think just anything we please about animals and their meaning for us. We come face to face with the necessity of a dialectical understanding of scientific labour in producing for us our knowledge of nature.

I will restrict my analysis primarily to a few years around the Second World War and to work on a single group of animals – the primates, in particular, the rhesus monkey, native to Asia but present in droves in scientific laboratories and research stations world-wide. I will focus principally on the work of one person, Clarence Ray Carpenter, who helped found the first major research station for free-ranging monkeys as part of the school of tropical medicine affiliated with Columbia University off Puerto Rico on the tiny island, Cayo Santiago, in the late 1930s. These monkeys and their descendants have been central actors in dramatic reconstructions of natural society. Their affiliation with tropical medicine in a neo-colonial holding of the United States, which has been so extensively used as an experiment station for capitalist fertility management policies, adds an ironic backdrop appropriate to our subject.

Men like Carpenter moved within a complex scientific world in which it would be incorrect to label most individuals or theories as sexist or whatever. It is not to attach simplistic labels but to unwind the specific social and theoretical structures of an area of life science that we need to examine the

interconnections of laboratory heads, students, funding agencies, research stations, experimental designs, and historical setting. Carpenter earned his PhD at Stanford for a study of the effects on sexual behaviour of the removal of the gonads of male pigeons in mated pairs. He then received a National Research Council Fellowship in 1931 to study social behaviour of primates under the direction of Robert M. Yerkes of the Laboratories of Comparative Psychobiology at Yale University. Yerkes had recently established the first comprehensive research institution for the psychobiological study of anthropoid apes in the world. For Yerkes, apes were perfect models of human beings. They played a major part in his sense of mission to promote scientific management of every phase of society, an idea typical of his generation.

It has always been a feature for the use of the chimpanzee as an experimental animal to shape it intelligently to specification instead of trying to preserve its natural characteristics. We have believed it important to convert the animal into as nearly ideal a subject for biological research as is practicable. And with this intent has been associated the hope that eventual success might serve as an effective demonstration of the possibility of re-creating man himself in the image of a generally acceptable ideal. (Yerkes, 1943, p. 10)⁷

He, then, designed primates as scientific objects in relation to his ideal of human progress through human engineering.

Yerkes was interested in the apes in two main regards – their intelligence and their social-sexual life. For him intelligence was the perfect expression of evolutionary position. He saw every living object in terms of the outstanding problem of experimental comparative psychology in America since its inception around 1900: the intelligence test. Species, racial, and individual qualities were fundamentally tied to the central index of intelligence, revealed on the one hand through behaviour-testing and on the other through the neural sciences. He had designed the army intelligence tests administered to recruits in the First World War, tests seen to provide a rational basis for assignment and promotion, to indicate natural merit fitting men for command (Yerkes, 1920; Kevles, 1968).⁸ His role in the war was entirely compatible with his role as an entrepreneur in primate studies. In both cases he saw himself and his scientific peers working to foster a rational society based on science and preserved from old ignorance, embodied especially in religion and politics.

The social-sexual life of primates was for Yerkes thoroughly intertwined with their intelligence. Mind would order and rule lower functions to create society. In a classic study of the origin of the body politic, Yerkes (1939) observed that female chimpanzees who were sexually receptive were allowed by the dominant males to have food and 'privileges' to which they were

ordinarily not entitled. Primate intelligence allowed sexual states to stimulate the beginnings of human concepts of social right and privilege. The sexual reductionism hardly needs emphasis. His study linking sex and power was typical of work in the 1930s, and hardly different from much to this day. In an early feminist critique, Ruth Herschberger (1948) marvellously imagined the perspective of Josie, the female chimpanzee whose psychosexual life was of such concern to Yerkes. Josie seems not to have seen her world in terms of trading sex for 'privilege', but to Yerkes that economic link of physiology and politics seemed to have been scientifically confirmed to lie at the organic base of civilization.

In addition to direct investigation of physiological sex and social behaviour in human beings' closest relatives, Yerkes exercised, along with his peers, a tremendous influence on the overall direction of the scientific study of sex in this country. He was for twenty-five years chairman of the Rockefeller Foundation-funded National Research Council Committee for Research on Problems of Sex (CRPS). This committee, from 1922 until well after the Second World War when federal funding became massively available for science, provided the financial base for the transformation of human sex into a scientific problem. Fundamental work on hormones and behaviour, sex-linked differences in mental and emotional qualities, marital happiness, and finally the Kinsey studies were all funded by the Committee for Research on Problems of Sex. It played a key role in opening up sexual topics for polite discussion and respectable investigation in an era of undoubted prurience and ignorance.⁹

However, the opening was double edged; the committee, in its practice and ideological expressions, was structured on several levels according to the principle of the primacy of sex in organic and social processes. To make sex a scientific problem also made it an object for medical therapy for all kinds of sexual 'illness', most certainly including homosexuality and unhappy marriages. The biochemical and physiological basis of the therapeutic claims immensely strengthened the legitimating power of scientific managers over women's lives. The committee closed the escape holes for those who rejected the American Freud's kind of sexual reductionism: whether from the psychoanalytic or physical-chemical directions, sex was safely in the care of scientific-medical managers. Monkeys and apes were enlisted in this task in central roles; as natural objects unobscured by culture, they would show most plainly the organic base in relation to which culture emerged. That these 'natural objects' were thoroughly designed according to the many-levelled meanings of an ideal of human engineering has hardly been noticed.

Carpenter arrived at Yale's primate laboratories already enmeshed in the web of funding and practice represented by the CRPS. His PhD work had been funded by the committee, his post-doctoral fellowship granted by

essentially the same men, and his host, Yerkes, was the central figure in a very important network of scientific assumptions and practices. Those scientific networks crucially determined who did science and what science was considered good. From his education, funding, and social environment, there was little reason for Carpenter to reject the basic assumptions that identified reproduction and dominance based on sex with the fundamental organizing principles of a natural body politic. What Carpenter added, however, was significant. Methodologically, he established the demanding skill of naturalistic observation of wild primates in two extraordinarily careful field studies, one on New World howler monkeys and one on Asian gibbons. These studies are worthy of note because they are simultaneously excellent, commanding work and fully reflective of social relations based on dominance in the human world of scientists.¹⁰ Theoretically, Carpenter tied the interpretations of the laboratory disciplines of comparative psychology and sexual physiology to evolutionary and ecological field biology centred on the concepts of population and community. In short, he started to link the elements of natural and political economy in new and important ways. The classic Darwinian conception of natural political economy of populations began to be integrated with the physiological and psychological sciences that greatly flourished in the early twentieth century. The integration would be complete only after the Second World War, when Sherwood Washburn and his students transformed physical anthropology and primate studies by systematically exploiting the evolutionary functionalism of the neo-Darwinian synthesis and the social functionalism of Bronislaw Malinowski's theory of culture.

In addition to linking levels of psychobiological analysis to modern evolutionary theory, Carpenter analysed primate groups with the tools of early systems theory that were simultaneously providing the technical base for the claim to scientific maturity of the social sciences based on concepts of culture and social group. Carpenter's early social functionalism – with all its remaining ties to an older comparative psychology and to developmental physiology (experimental embryology) – is crucial for examining the connecting chains from physiology to politics, from animal to human. Carpenter himself did not work within the doctrine of autonomy of natural and social sciences. Neither did he permit direct reduction of social to physiological or of human to animal. He elaborated analytical links between levels that were shared by both adherents and opponents of the crucial nature-culture distinction. Indeed, his primate sociology is a useful place to begin to unravel the many varieties of functionalism emerging within biological and social sciences between the two world wars, all based on principles of hierarchical order of the body and body politic. The functionalist disciplines underlay strong ideologies of social control

and techniques of medical, educational, and industrial management.

A single experimental manipulation embodies in miniature all the layers of significance of the principle of dominance in Carpenter's seminal work on the animal body politic. In 1938 he collected about 400 rhesus monkeys in Asia and freed them on Cayo Santiago. After a period of social chaos, they organized themselves into six groups containing both sexes and ranging in size from three to 147 animals. The monkeys were allowed to range freely over the thirty-seven-acre island and to divide space and other resources with little outside interference. The first major study undertaken of them was of their sexual behaviour, including periodicity of oestrus, homosexual, autoerotic, and 'nonconformist' behaviour. Carpenter's conclusions noted that intragroup dominance by males was strongly correlated with sexual activity, and so presumably with evolutionary advantage. All the sexist interpretations with which we have become monotonously familiar were present in the analysis of the study, including such renderings of animal activities as, 'Homosexual females who play masculine roles attack females who play the feminine role prior to the formation of a female-female consort relation' (Carpenter, 1964, p. 339).

In harmony with the guiding notion of the ties of sex and dominance in the fundamental organization of the rhesus groups, Carpenter performed what on the surface is a very simple experiment, but one which represents the whole complex of layered explanation of the natural body politic from the physiological to the political. After watching the undisturbed group for one week as a control, he removed the 'alpha male' (the animal judged most dominant on the basis of priority access to food, sex, and so on) named Diablo, from his group. Carpenter then observed the remaining animals for one week, removed the number 2 male, waited another week, removed the number 3 male, waited, restored all three males to the group, and again observed the social behaviour. He noted that removal of Diablo resulted in immediate restriction of the territorial range of the group on the island relative to other groups. Social order was seriously disrupted. 'The group organization became more fluid and there was an increase in *intra-group conflict* and fights . . . After a marked disruption lasting three weeks, the group was suddenly restructured when the dominant males were released' (1964, p. 362). Social order was restored, and the group regained its prior favourable position relative to other groups.

Several questions immediately arise. Why did Carpenter not use as a control the removal of other than dominant males from the group to test his organizing hypothesis about the source of social order? Literally, he removed the putative head from the collective animal body. What did this field experiment, this decapitation, mean to Carpenter?

First, it must be examined on a physiological level. Carpenter relied on

biological concepts for understanding social bodies. He drew from theories of embryological development that tried to explain the formation of complex whole animals from simpler starting materials of fertilized eggs. One important embryological theory used the concept of fields organized by axes of activity called dominance gradients. A field was a spatial whole formed by the complex interaction of gradients. A gradient was conceived, in this theory, to consist of an ordered series of processes from low to high levels of activity measured, for example, by differential oxygen consumption. Note that at this basic level dominance was conceived as a purely physiological property that could be objectively measured. The slope of a gradient could be shallow or steep. Several gradients making up a field would be organized around a principal axis of greatest slope, the organization centre. An organism grew in complexity through integrated multiplication of dominance systems. An appropriate experimental system within developmental physiology designed to test theories of fields, gradients, physiological dominance, and organization centres was the simple hydra. It had only one axis or possible gradient: head to tail. One could cut off the polyp's head, observe temporary disorganization of remaining tissue, and see ultimate re-establishment of a new head from among the physiologically 'competing' cells. Further, one could remove much or little from the head portion of the activity gradient and test the extent of ensuing organic disorganization.¹¹

Carpenter conceived social space to be like the organic space of a developing organism, and so he looked for gradients that organized the social field through time. He found such a physiological gradient of activity in the dominance hierarchy of the males of the social group. He performed the theoretically based experiment of head removal and 'observed' ensuing physiological competition among cells or organs (i.e., other points – animals – on the activity-dominance gradient) to re-establish a chief organization centre (achieve alpha male status) and restore social harmony. Several consequences flow from these identifications.

First, other groups of animals in the society could be ordered on activity axes as well; females, for example, were found to have a dominance hierarchy of less steepness or lower slope. Young animals had unstable dominance gradients; the observation underlying that interpretation was that ordinary dominance behaviour could not be reliably seen and that immature animals did not show constant dominance relations to one another. As unseen 'observations' became just as important as evidence as seen ones, a concept of latent dominance followed readily. From this point, it is an easy step to judgements about the amount of dominance that functions to organize social space (call that quantity leadership) and the amount that causes social disruption (call that pathological aggression). Throughout the period around the Second World War, similar studies of the authoritarian

personality in human beings abounded; true social order must rest on a balance of dominance, interpreted as the foundation of co-operation. Competitive aggression became the chief form that organized other forms of social integration. Far from competition and co-operation being mutual opposites, the former is the precondition of the latter – on physiological grounds. If the most active (dominant) regions, the organization centres, of an organism are removed, other gradient systems compete to re-establish organic order: a period of fights and fluidity ensues within the body politic. The chief point is that without an organizing dominance hierarchy, social order supposedly is seen to break down into individualistic, unproductive competition. The control experiment of removing other animals than the dominant males was not done because it did not make sense within the whole complex of theory, analogies to individual organisms, and unexamined assumptions.

The authoritarian personality studies bring us to the second level of explanation of the body politic implicit in Carpenter's experiment: the psychological. The idea of a dominance hierarchy was derived in the first instance from study of 'pecking orders' in domestic chickens and other birds initiated by the Norwegian Thorlief Schjelderup-Ebbe (1935) as early as 1913, but not incorporated into American comparative psychology in any important way until the 1930s. Then animal sociology and psychology, as well as human branches of the disciplines, focused great attention on ideas of competition and co-operation. Society was derived from complex interactions of pairs of individuals, understood and measured by psychological techniques, which constituted the social field space. One looked for axes of dominance as organizing principles on both the physiological and psychological levels.

The third and last level implicit in Carpenter's manipulation is that of natural political economy. The group that loses its alpha male loses in the competitive struggle with other organized organic societies. The result would be reflected in less food, higher infant mortality, fewer offspring, and thus evolutionary disadvantage or even extinction. The market competition implicit in organic evolutionary theory surfaces here. The theory of the function of male dominance nicely joins the political economy aspect of the study of animal behaviour and evolution (competitive, division of labour, resource allocation model) with the social integration aspect (co-operative co-ordination through leadership and social position) and with the purely physiological understandings of reproductive and embryological phenomena. All three perspectives link functionalist equilibrium social models – established in the social sciences of the period – to explicit ideological, political concerns with competition and co-operation (in labour struggles, for example). Since animal societies are seen to have in simpler form all the

characteristics of human societies and cultures, one may legitimately learn from them the base of supposedly natural, integrated community for humanity. Elton Mayo (1933) – the influential Harvard, anti-labour union, industrial psychologist-sociologist of the same period – called such a community the 'Garden of Industry'.¹²

The political principle of domination has been transformed here into the legitimating scientific principle of dominance as a natural property with a physical-chemical base. Manipulations, concepts, organizing principles – the entire range of tools of the science – must be seen to be penetrated by the principle of domination. Science cannot be reclaimed for liberating purposes by simply reinterpreting observations or changing terminology, a crass ideological exercise in any case, which denies a dialectical interaction with the animals in the project of self-creation through scientific labour. But the difficult process of remaking the biosocial and biobehavioural sciences for liberation has begun. Not surprisingly, one of the first steps has been to switch the focus from primates as models of human beings to a deeper look at the animals themselves – how they live and relate to their environments in ways that may have little to do with us and that will surely reform our sense of relation to nature in our theories of the body politic. These 'revisionist' scientific theories and practices deserve serious attention. Of them, 'feminist' perspectives in physical anthropology and primatology have stressed principles of organization for bodies and societies that do not depend on dominance hierarchies. Dominance structures are still seen and examined, but cease to be used as causal explanations of functional organization. Rather, the revisionists have stressed matrifocal groups, long-term social co-operation rather than short-term spectacular aggression, flexible process rather than strict structure, and so on. The scientific and ideological issues are complex; the emerging work is justly controversial.

In our search for an understanding of a feminist body politic, we need the discipline of the natural and social sciences, just as we need every creative form of theory and practice. These sciences will have liberating functions in so far as we build them on social relations not based on domination. A corollary of that requirement is the rejection of all forms of the ideological claims for pure objectivity rooted in the subject-object split that has legitimated our logics of domination of nature and ourselves. If our experience is of domination, we will theorize our lives according to principles of dominance. As we transform the foundations of our lives, we will know how to build natural sciences to underpin new relations with the world. We, like Dawn in Marge Piercy's *Woman on the Edge of Time*, want to fly into nature, as well as into the past, to make it come out all right. But the sciences are collective expressions and cannot be remade individually. Like Luciente and Hawk, in the same novel, feminists have been clear that 'Nobody can

make things come out right'; that 'It isn't bad to want to help, to want to work, to seize history . . . but to want to do it alone is less good. To hand history to someone like a cake you baked' (Piercy, 1976, pp. 188–9).

Chapter Two

The Past Is the Contested Zone: Human Nature and Theories of Production and Reproduction in Primate Behaviour Studies

People like to look at animals, even to learn from them about human beings and human society. People in the twentieth century have been no exception. We find the themes of modern America reflected in detail in the bodies and lives of animals. We polish an animal mirror to look for ourselves. The biological sciences' focus on monkeys and apes has sought to make visible both the form and the history of our personal and social bodies. Biology has been pre-eminently a science of visible form, the dissection of visible shape, and the acceptance and construction of visible order. The science of non-human primates, primatology, may be a source of insight or a source of illusion. The issue rests on our skill in the construction of mirrors.

Primatology has focused on two major themes in interpreting the significance of animals for understanding human life – sex and economics, reproduction and production. The crucial transitions from a natural to a political economy and from biological social groups to the order of human kinship categories and systems of exchange have been basic concerns. These are old questions with complex relations to technical and ideological dimensions of biosocial science. Our understandings of both reproduction and production have double-edged possibilities. On the one hand, we may reinforce our vision of the natural and cultural necessity of domination; on the other, we may learn to practise our sciences so as to show more clearly the now fragmentary possibilities of producing and reproducing our lives without overwhelming reliance on the theoretical categories and concrete practices of control and enmity.

Theories of animal and human society based on sex and reproduction have been powerful in legitimating beliefs in the natural necessity of aggression, competition, and hierarchy. In the 1920s, primate studies began to claim that all primates differ from other mammals in the nature of their reproductive physiology: primates possess the menstrual cycle. That physiol-

ogy was asserted to be fraught with consequences, often expressed in the fantasy-inspiring 'fact' of constant female 'receptivity'. Perhaps, many have thought and some have hoped, the key to the extraordinary sociability of the primate order rests on a sexual foundation of society, in a family rooted in the glands and the genes. Natural kinship was then seen to be transformed by the specifically human, language-mediated categories that gave rational order to nature in the birth of culture. Through classifying by naming, by creating kinds, culture would then be the logical domination of a necessary but dangerous instinctual nature. Perhaps human beings found the key to control of sex, the source of and threat to all other kinds of order, in the categories of kinship. We learned that in naming our kind, we could control our kin. Only recently and tentatively have primatologists seriously challenged the indispensability of these sorts of explanations of nature and culture.

Biosocial theories focusing on production rest on a fundamental premise: humankind is self-made in the most literal sense. Our bodies are the product of the tool-using adaptation which predates the genus *Homo*. We actively determined our design through tools that mediate the human exchange with nature. This condition of our existence may be visualized in two contradictory ways. Gazing at the tools themselves, we may choose to forget that they only mediate our labour. From that perspective, we see our brains and our other products impelling us on a historical course of escalating technological domination; that is, we build an alienated relation to nature. We see our specific historical edifice as both inevitable human nature and technical necessity. This logic leads to the superiority of the machine and its products and ensures the obsolescence of the body and the legitimacy of human engineering. Or, we may focus on the labour process itself and reconstruct our sense of nature, origins, and the past so that the human future is in our hands. We may return from the tool to the body, in its personal and social forms. This chapter is about efforts to know the body in the biosocial conditions of production and reproduction. Our bodies, ourselves.

More particularly, this chapter is about the debate since approximately 1930 in primate studies and physical anthropology about human nature – in male bodies and female ones. The debate has been bounded by the rules of ordinary scientific discourse. This highly regulated space makes room for technical papers; grant applications; informal networks of students, teachers, and laboratories; official symposia to promote methods and interpretations; and finally, textbooks to socialize new scientists. The space considered in this chapter does not provide room for outsiders and amateurs. One of the peculiar characteristics of science is thought to be that by knowing past regularities and processes we can predict events and thereby control them. That is, with our sciences – historical, disciplined forms of theorizing about

our experience – we both understand and construct our place in the world and develop strategies for shaping the future.

How can feminism, a political position about love and power, have anything to do with science as I have described it? Feminism, I suggest, can draw from a basic insight of critical theory. The starting point of critical theory – as we have learned it from Marx, the Frankfurt school, and others – is that the social and economic means of human liberation are within our grasp. Nevertheless, we continue to live out relations of domination and scarcity. There is the possibility of overturning that order of things. The study of this contradiction may be applied to all our knowledge, including natural science. The critical tradition insists that we analyse relations of dominance in consciousness as well as material interests, that we see domination as a derivative of theory, not of nature. A feminist history of science, which must be a collective achievement, could examine that part of biosocial science in which our alleged evolutionary biology is traced and supposedly inevitable patterns of order based on domination are legitimated. The examination should play seriously with the rich ambiguity and metaphorical possibilities of both technical and ordinary words. Feminists reappropriate science in order to discover and to define what is 'natural' for ourselves.¹ A human past and future would be placed in our hands. This avowedly interested approach to science promises to take seriously the rules of scientific discourse without worshipping the fetish of scientific objectivity.

My focus will be four sets of theories that emphasize the categories of reproduction and production in the tangled web of the reconstruction of human nature and evolution. The first, centring on reproduction, was the work of Sir Solly Zuckerman. Born in 1904 in South Africa, he studied anatomy at the University of Cape Town, then earned his MD and BS at University College Hospital, London. He combined in complex and illuminating ways research interests in human palaeontology and physical anthropology, reproductive physiology and the primate menstrual cycle, and broad zoological and taxonomic questions focused on primates. His social base included zoological gardens and research laboratories in British universities and medical schools; his training and career reflect intersections of the perspectives of anatomist, biochemist, anthropologist, clinician, administrator, and government science adviser.² He has been the architect of an extremely influential theory that sexual physiology is the foundation of primate social order. He also offered a variation of the theory of the origin of human culture in the hunting adaption, which delineated crucial consequences for the division of labour by sex and the universal institution of the human family. In focusing on the sexual biology of monkeys, Zuckerman constructed a logic for setting the boundaries of human nature. In effect, Zuckerman claimed, the only universal for all the primates is the menstrual

cycle. Therefore, only on that basis may we make valid comparisons of human and non-human ways of life.

A second set of theories stressing reproduction is that of Thelma Rowell, now at the University of California at Berkeley. She earned her doctorate in the early 1960s under Robert Hinde of Cambridge, the man who also supervised Jane Goodall's dissertation on chimpanzees. That period saw the beginning of a still continuing acceleration of publication based on long-term field observations of wild primates. Rowell's training was in zoology and ethology. Her first intention was to write her thesis on mammalian (hamster) communication, using the ethological approach worked out particularly by Niko Tinbergen at Oxford. Because Tinbergen then felt the methodology to be inappropriate to the non-stereotypical social communication of mammals, Rowell pursued her ideas at Cambridge under Hinde, a major synthesizer of American comparative psychology and Continental ethology. Rowell's research (1966a, 1966b, 1970), which has used both traditions, has been concerned with primate communication, the baboon menstrual cycle, comparison of the naturalistic behaviour of monkeys with their behaviour in captivity and in laboratory experimental situations, and mother-infant socialization systems. An outspoken critic of the pervasive dominance concept, she has made social role and stress her overriding theoretical concerns.

Yet both Zuckerman and Rowell, who are very different, adopt varieties of biological and sociological functionalism that set limits on permitted explanations of the body and body politic. The most important is the functionalist requirement of an ultimate explanation in terms of equilibrium, stability, balance. Functionalism has been developed on a foundation of organismic metaphors, in which diverse physiological parts or subsystems are co-ordinated into a harmonious, hierarchical whole. Conflict is subordinated to a teleology of common interests.³ Both Zuckerman's and Rowell's explanations also reflect the ideological concerns of their society in complex ways, which can instruct feminist efforts to deal with biological and social theories.

The third and fourth sets of theories are reconstructions of human evolution. Both claim to reveal the meaning of crucial adaptations, both focus on production. They see adaptation as a concept relating to the interpretation of functional complexes, of ways of life in which behaviour and structure mutually inform each other. If both Rowell and Zuckerman restrict themselves (almost) to talking about monkeys, Sherwood Washburn of the University of California at Berkeley, his former student Adrienne Zihlman, and her colleague at the University of California at Santa Cruz, Nancy Tanner, argue about the connection of physical and social anthropology. Telling scientific tales about human nature, they are unapologetic about the

place of speculative reconstruction in the study of evolution. There can be no hiding behind mechanistic or purely structural explanations in evolutionary biology and anthropology. But since function is still pre-eminent, the resulting scientific approach may be called evolutionary functionalism.

The central figure in the third set of theories, Washburn, is most immediately associated with the modern man-the-hunter hypothesis. This maintains that the hunting adaptation has been the fundamental functional complex which set the rules for nearly all the history of the genus *Homo* until the very recent past. He has also generated the theory of tool use as the motor of evolution of the human body, including the brain and its power of language. His influential vision of the self-made species has earned him praise from such Marxist feminists as Eleanor Leacock (1972) and such Freudian feminists as Dorothy Dinnerstein (1977). He has also been an arch-villain of the piece because of the overwhelming concentration on males as practically the only active sort of human being. Washburn is, in my opinion, both more complicated and more important than either approach reveals. By developing functional anatomy as part of the synthetic theory of evolution and then extending the approach to the social behaviour of living primates, he has integrated sophisticated genetic theory and disciplined field and experimental methodology into the practice of evolutionary reconstruction.

Authors of the fourth set of theories, Zihlman and Tanner, have produced an excellent critique of Washburn's scientific sexism with the use of his own tools. They could not have thought as they do without the functional physical anthropology Washburn has advanced. Tanner and Zihlman have also added a new twist to feminist, scientific evolutionary reconstruction: the use of sociobiological concepts. The pleasure and irony of their approach is that the ideas of some of the most explicitly sexist theories have been enlisted to tell another story. Yet at this level, the feminist debate is still about the nature and existence of human universals. Theories of origins quickly become theories of essences and limits (see Figure 1).

The 1930s was a decade of exciting advance in the study of sexual endocrinology. Early in the decade Solly Zuckerman produced a powerful theory of the physiological basis of mammalian society in general and primate society in particular. He repeatedly asserted that he intended only to adopt a zoologist's perspective on animal sociology and to avoid extrapolation to human, cultural, language-mediated behaviour. Yet his work informed investigations into the origin of human organization and the use of primates in studies of it. He gave the concept of dominance an up-to-date scientific legitimation, for example, connecting it to the new endocrinology. Dominance was closely linked, in his theory, to male competition for control of resources (females). Females then emerged as natural raw material for the

FIGURE 1: RELATIONS OF MAJOR FIGURES

focus on Reproduction Zuckerman —→ Rowell		focus on Production Washburn —→ Tanner and Zihlman	
1 physiological functionalism	1 social functionalism	1 hunting adaptation	1 gathering adaptation
2 analysis of individual	2 analysis of group	2 neo-Darwinian synthesis	2 sociobiological genetic tools
common themes		common themes	
1 restrict analysis to animals		1 basic interest in hominid line	
2 global expalantory concept		2 evolutionary functionalism	
	Z — dominance		+
	R — stress		social functionalism
3 primary identification as biologists, primatologists		3 primary identification as anthropologists	
4 study of menstrual cycle		4 study of fossil structures as clue to social behaviour (function)	

The categories in this figure do not indicate rigid separations, but emphases found in the writings of each worker.

—→ = some direct debt.

imposition of male order through the consequences of reproductive physiology. The human innovation was the practice of control of the natural physiological economy. In brief, domination changed levels with culture.

Zuckerman's starting point for considering the causes of primate sociability was twofold: (1) debate in the anthropological community – represented by Malinowski's *Sex and Repression in Savage Society* and Freud's *Totem and Taboo* and *Civilization and Its Discontents* – on the cultural domination of instinct in the formation of the human level of organization; and (2) a new biological discipline, relating hormones and behaviour, rooted in neural and reproductive physiology and comparative and behaviourist psychology. Zuckerman adopted a firm physiological and medical orientation in both areas. He criticized all existing theories of animal organization for their anthropomorphic and teleological overtones. For the older evolutionary meaning of functional adaptation, Zuckerman substituted a physiological approach, which rested on studies of particular mechanisms in anatomical and biochemical terms. Function meant mechanism. Behaviour and society were to be related to mechanistic physiology, and taxonomy was to be

reformed on that basis as well. The taxonomic project was undertaken in the book, *Functional Affinities of Man, Monkeys, and Apes* (1933). Here, Zuckerman constructed his 'hunting hypothesis' to account for the transition from nature to culture.

But first we must look at his general theory of non-human primate society, found in *The Social Life of Monkeys and Apes* (1932). Zuckerman imposed an important limit on primatology. He did not recognize a ladder of perfection of living primates representing stages of mental function and corresponding degrees of social co-operation (which always meant hierarchical organization) through which human beings must have passed. Thus, 'Only the behavior common to all apes and monkeys can be regarded as representing a social level through which man once passed in the prehuman stages of his development' (1932, p. 26). Only one thing met this requirement: 'When all questions of its applications to human behavior are laid aside, and when teleological speculation is disregarded, the chief subject matter of a scientific mammalian sociology is seen to be ecology, reproductive physiology, and those influences which can be classed together as due to the variations of the individual' (1932, p. 28). That nod to ecology is the last we hear of it until a precipitating cause was later required to fire the cultural answer (hunting) to primate sexuality. Individual variation explained details, 'But social behavior – the interrelation of individuals within a group – is determined primarily by the mechanisms of reproductive physiology' (p. 29).

Zuckerman had already told the reader that his excursion into mammalian sociology had begun in response to the urgings of anthropologists; he aimed to replace anecdotal accounts of animal societies with hard physiology. He simply assumed that the important bone of contention was the nature and origin of the human family, itself the origin of society. At this point, Zuckerman turned to the writings of the American mammalogist, G. S. Miller, who had criticized Malinowski's contention that the human family was unique, that kinship represented the crucial human-animal break. Malinowski regarded the human physiology on which the institution of kinship (i.e., fatherhood) was imposed as unique. In contrast, Miller and Zuckerman agreed that one found all the biological essentials of the human family (namely, constant female receptivity) in mammalian reproductive association. Zuckerman simply developed that viewpoint into an analysis of consequent social forms among primates. Freud's story of the origin of civilization in repression was prefigured on the prehuman level. Zuckerman's story was based on the comparative physiological anatomy of the different primate groups informed by his recent discovery of the baboon menstrual cycle and on the behaviour of a colony of Hamadryas baboons on Monkey Hill in the London Zoo since 1925. The zoo behavioural observations were supplemented by nine days of field study of a different species

(the chacma baboon) in South Africa during an excursion to collect anatomical material for the study of reproduction.

The logic of his story, though exquisitely simple, influenced a whole domain of advanced research and provided the logical ground for the new science of hormones and behaviour to encompass the study of social order. Animals, except when they come together to reproduce, are solitary because the basic model of life is competition for scarce resources. Reproductive association is fraught with danger because here competitive success requires the co-operation of other animals. Males fight to obtain the maximum number of reproductive opportunities. These elements Zuckerman retained from Darwin. They did not appear teleological to him in the same way as discussions of animal altruism and co-operation. Males dominate females to preclude another source of competitive insubordination. After the bare essentials of reproduction are accounted for, animals separate to avoid further inevitable injuries from sexual battles. Sexual periodicity (seasonality) evolved to protect the animal atoms from each other during the rest of the year. Mother-young groups hardly constitute society. In any case, these groups are general to mammals and cannot explain primate societies. Different degrees of long-term heterosexual association were rigorously related to requirements of reproduction in the particular ecological environments available to the animals. Males compete to accumulate the means of (re)production, through which alone they can increase their genetic capital in evolution. Females are the means of evolutionary production and the source of surplus value. As dominance became the universal medium of exchange among males and the measure of value, the political and natural economy of Hobbes's *Leviathan* has found its twentieth-century biological expression. The economic order is exclusively physiological in all but human beings, where cultural ownership of females and property is also to be found.

To Zuckerman, the main event in social evolution had been the elimination of extreme seasonality and the introduction of year-long association based on the continuous sexual 'receptivity' of females. First the oestrus and then the menstrual cycle introduced regular repeating bouts of sexual intercourse. Monthly cycles replaced seasonal ones, and a social revolution ensued. Continuous association required strong control mechanisms if the animals were to survive it. So developed the 'harem', exemplified by the London Hamadryas which Zuckerman observed personally during 1929-30 and for which he had records dating to the establishment of the colony in 1925. Especially since the London Hamadryas did not survive on Monkey Hill - nearly all were killed in brutal fights and only one infant was reared successfully - it was important for Zuckerman to establish that captive baboons in extreme conditions of sexual imbalance and crowding still revealed the essential structure of primate society in nature. The traditional

physiological argument was used: extreme circumstances are the best windows to the normal because they highlight basic mechanisms which would otherwise be obscured. Hierarchy and deadly competition were crucial regulators of primate society, not creations of human captors. It was also important for him to convince his readers that variations in social form among primates, none of whom had been studied in other than a casual way in the wild, were only details imposed upon the fundamental physiologically determined family. Within the pattern of dominance, such behaviour as female 'prostitution' (which Zuckerman and Miller defined as presenting for non-sexual reasons) was explained as the beginning of the trading of sexual favours for otherwise competitively unobtainable goods. Grooming, feeding order, vocal and gestural expression, allotment of social space, and many other aspects of social behaviour were all derived from the physiologically determined harem organization of primates. Zuckerman was unequivocal:

The argument outlined above goes far toward explaining the broad basis of subhuman primate society. The main factor that determines social grouping in subhuman primates is sexual attraction . . . The limit to the number of females held by any single male is determined by his degree of dominance, which will again depend not only on his own potency, but also upon his relationship with his fellow males. (Zuckerman, 1932, p. 31)

Of course, human beings share with other primates 'a smooth and uninterrupted sexual and reproductive life' (p. 51); yet human beings and their families exist in the realm of culture, buffered, if not exempt, from the physiologist's gonadectomies and injections. How did the physiologist re-enter the kingdom of culture with his medical tools for producing family health and behavioural adjustment within social hierarchy defined as co-operation? Through hunting, through the taste for meat. Returning to *Functional Affinities*, we meet Zuckerman in his guise as physical anthropologist who unites the physiological to the ecological in order to generate a large-brained hunting animal who needs more complex forms of male co-operation and female fidelity in order to feed the family. The reproductive unit remains on the throne as the fundamental core of social association in the cultural form of kinship, the basic object of the social science of cultural anthropology. Again, Zuckerman's logic is elegantly simple. Some unknown ecological changes produced selection pressure for prehumans to exploit new sources of food, to rework the age-old unspecialized feeding patterns, and to introduce sexual division of labour as a necessary consequence of the requirements of large-scale meat-eating. Food-sharing necessitated the human form of the family, which for Zuckerman meant selection pressure for 'overt monogamy' and conceptual recognition of significant social relations (ownership of women) even when no one was

around to enforce them. The passivity of females in such major transformations was an unexamined assumption. So developed marriage and the hunting band of males, with all the startling consequences for the brain and its products of speech and culture.

Zuckerman hinted at the later form of the hunting hypothesis that emphasized the tool-using adaptation in the origin of the self-made species. But more important was the fact that the all-male band – the human form of co-operation signalling the divorce of culture from nature – became a scientific, even a physiological, object in Zuckerman's hands. The valuable female continued to pose the threat of disorder through sexuality. Co-operation came to mean conscious male regulation of previously natural hierarchy and competition, which in turn had been the fruits of permanent female sexuality. These themes were not new with Zuckerman, but the way he integrated them into modern physiological disciplines was. Further, his biological ideology did not violate, but actually reinforced, the important doctrine of the autonomy of biological and social science, of animal and human order. Zuckerman left full room for functionalist social anthropology. He only reformed Malinowski's physiology.

Zuckerman's importance in the development of primate behaviour studies themselves has not been his scant empirical observations, but his provision of a theory that met the needs of rapidly advancing new disciplines. At the same time, he rescintized conventional prejudices with the liberal ideology that claimed that culture was autonomous from previous forms of biological determinism. That same liberal ideology legitimated a logic of scientific control over 'nature', now rationalized as a material given reduced either to pre-rational danger or ordered resource. The alienating core of this is not obscure. Zuckerman set questions for workers to follow that even in their asking reinforced scientific beliefs about natural male competition and dangerous female sexuality. His tie of sexuality to dominance in ways acceptable to the physiological and behavioural sciences of the 1930s helped establish the status of dominance as a trait or fact rather than a concept. Primatologists have continued to ask about the selective advantage of dominance behaviour and have tended to assume, rather than test, a correlation of breeding advantage with an entity called dominance. Not until 1965, with a paper by two of Sherwood Washburn's students, was his theory of the origin of primate society in year-round female sexuality convincingly laid to rest.⁴ Zuckerman's mode of blending covert Freudianism, biochemical mechanisms, and studies of social behaviour has had a long and influential life.

At first glance, the only comparisons of Thelma Rowell with Zuckerman must be in contrast. Though she praised Zuckerman for his groundbreaking work on the baboon menstrual cycle and refrained from very severe

criticism of him in her historical paper on the dominance concept, her whole work seems to have been in opposition to his ideas and his methods. She does not claim scientific purity for a language bathed in multiple waves of meaning in the common tongue as well as in scientific tradition. Her arguments have been zoological and explicitly sociological rather than an extrapolation from reproductive physiology. She is known for her care in dividing cage space for captive monkeys to permit more naturalistic behaviour, and for excellent field studies, rather than for physiological arguments about provoked extreme behaviour as the window to the normal. Rather than emphasizing primate universals, Rowell's papers are permeated by particularism, by counsels to notice complexity, by insistence on variability in a manner reminiscent of early proponents of the culture concept and cultural particularism. Moreover, Rowell is working in very different scientific and ideological circumstances. She benefits from and contributes to the now extensive literature based on direct field studies of primates, studies which themselves referred back to Zuckerman but went beyond that to which he had access. This body of literature has tended to reject Zuckerman's doctrines on sex but to retain focus on dominance. Finally, Rowell writes to an audience sensitized to the feminist implications of biosocial theory. It is not an accident that she emphasizes female behaviour and active social roles and finds dominance to be, at best, a convenient expression for predicting the frequency of some learned behaviours.

But it would be a dangerous mistake to see Rowell's work as simply exemplifying normal scientific progress in rooting out unnecessary prejudice while accumulating better data. Nor does her work simply substitute more satisfying (to me) female prejudices for Zuckerman's infuriating male consciousness. In fact, Rowell and Zuckerman are like each other in a crucial way, which I believe indicates part of the nature of the ideological function of impeccable work in perfectly controlled laboratory science. In 'stress', Rowell does have a global category of explanation corresponding to Zuckerman's sexual physiology. Like sex or dominance, stress is a category that incorporates general social belief into the extracts in the biochemist's test-tube. Stress may be studied on the level of adrenal function, on the level of mental illness, or on the level of 'explanation' of life in modern capitalism. If dominance was the crucial concept in the 1930s, in a context of extraordinary scientific and popular concern for the foundation of social co-operation and competition in a time of world crisis, then stress has been the favourite concept, in the guise of a thing, in more recent times of serious threat to privileged social order. Dominance is not dead, but stress is really more useful in social theory. It has a further referent, namely, to the concept of a social system and to structural functionalism as the principal mode of sociological explanation. The physical metaphors of systems theories – like

tolerance, stress, balance, and equilibrium – lead us to many levels of meaning. One which we must note is that of the idea of ‘obsolescence’ of certain biological systems and the medical function of relieving stressed, perhaps obsolescent, behaviour patterns. A second level of meaning implicit in systems functionalism is the imperative of ‘reproduction’ of the system as a social whole and as a breeding population. Behaviour can be explained, then, ultimately in terms of system maintenance or pathological failure to achieve such stability.

In 1974, Rowell summarized the arguments against use of the dominance concept to understand social structure. She gave two major lines of approach: (1) presenting all putative dominance behaviours as learned responses easily accounted for by current theories in animal psychology; and (2) removing the basis for considering dominance as a trait or adaptive complex subject to selection pressures. That is, so-called dominance behaviours do not seem to relate to reproductive success. In addition to amusing points about the slippery nature of concepts like ‘latent dominance’, which enter arguments to fill gaps in observation, Rowell asserts that conditions of observation introduce the determinants in which one should expect social animals to learn responses called dominance. Hierarchy for Rowell is primarily an artefact of methods of observation. Reinforcing this position is the discovery that different measures of dominance do not correlate highly with each other, and hierarchies worked out by different measures do not reveal the same social structure. Thus it is hard to see what observed behaviours related to dominance have to do with evolution, which requires a genetic basis for selection. In Rowell’s words, ‘the function of dominance becomes a non-question’ (1974, p. 151; italics altered).

But function does remain the essential question, the grail that unifies the actors in this chapter. For Rowell, function must be seen in terms of the concept of social system. Communication analysis, studies of mother–young interactions, role change in relation to age and sex class, social subsystems based on matriline (kinship), territory and hierarchy as spatial order, and variation of social structure in response to environmental variables: these become the areas of interest, the analytical objects that bear on the structural-functional explanation of system in terms of function. Rowell’s theoretical stance is most plain in her very useful, linguistically sophisticated book, *Social Behaviour of Monkeys* (1972). The problem of the social system is the problem of multiple-variable analysis in fluid structures in dynamic equilibrium through time and space. The debt of animal sociology to human sociology and anthropology from Bronislaw Malinowski, L. J. Henderson, and Talcott Parsons has hardly begun to be noticed, much less critically examined.

How does stress relate to the social system? Ironically, through the

concept of subordination hierarchies. Animals would be compared to each other on a scale of susceptibility to stress. Very sensitive animals would be easily roused to fear, flight, or cringing postures. Such sensitivity would reasonably be associated with high levels of adrenal-stimulating hormones. So the capacity to produce ACTH in ‘stressful’ situations would be reasonably postulated to have a genetic foundation. These are testable propositions, at least in principle. Calm animals might be called ‘dominant’ by observers simply because they move freely in social space and take freely from available resources, while their nervous comrades cringe or move away. Rowell would see the poor huddling beast as the stimulus or cause of the resulting ‘hierarchy’. Such a social scheme should be called a subordination order. A variety of response thresholds to stressful situations would be adaptive in the social group in nature. Both nervous and calm animals would have a role in efficiently monitoring the environment for danger or for maintaining intragroup peace. The genetic diversity in the population underlying the differences in response stress would be kept in evolution. One must note how functionalist notions of social role for overall system balance, genetic concepts for biochemical and hormonal function, and psychological approaches to dominance–subordination all converge in the central idea of stress.

Stress as a global, multi-layered concept embedded in functionalist explanation provides the critical tie between Thelma Rowell and Sherwood Washburn. The tie is represented by David Hamburg, later president of the Institute of Medicine of the National Academy of Sciences, in the 1960s chairman of the Department of Psychiatry at the Stanford School of Medicine, and collaborator with Sherwood Washburn in building primate studies around modern medical and evolutionary questions. In Hamburg’s and Washburn’s work, the darker side of functionalist explanation is starkly revealed; the metaphoric structure surrounding stress ceases to be more congenial than dominance. Hamburg has been a principal figure in evolutionary theories of emotional adaptive configuration, which lead to the notion of our obsolescent biology. Medical management of emotions maladaptive in ‘modern society’ seems justified to relieve pathological stress and maintain the social system. ‘Modern society’ itself seems given by some sort of technological imperative laid over our limiting biological heritage. Primate studies are motivated by, and in turn legitimate, the management needs of a stressed society. The animals model our limitations (adaptive breakdowns) and our innovations (tool use).

Social functionalism and evolutionary functionalism come together in the study of selection for behaviours and emotional patterns that maintain societies as successful breeding populations over time. The imperative is reproduction – of the social system and of the organisms who are its

member-role actors. In general, animals have to like to do what they must do to survive in their evolutionary history. Evolutionary theory here joins a sociology of systems and a psychology of personality and emotion in modern versions of a pleasure calculus connected to the organic, motivational base of learning theory. Rowell summarizes:

A zoologist, however, must always return to the question of selective advantages . . . It is so very obvious that monkeys enjoy being together that we take it for granted. But pleasure like every other phenomenon of life is subject to, and the result of, evolutionary pressure – we enjoy a thing because our ancestors survived better and left more viable offspring than their relations who did not enjoy (and so seek) comparable stimuli . . . This is speculation; but it is by research which examines the function of social systems of monkeys and other animals that we shall be able to understand fully their mechanisms. (1972, pp. 174, 180)

Washburn and Hamburg have shared the same analysis, but have applied it to another concept, again often perceived as a thing, in the vocabulary of meaning-laden scientific words: aggression, especially male aggression. Through this concept we must make a transition from explanations based on theories of reproduction to those based on production in human evolution and primate behaviour studies. Clearly, reproduction and production are complements, not opposites. But we must see how Washburn reached a 'man-the-hunter' theory from consideration of the economic functions of the species, while Zuckerman traced primate order through reproductive physiology, and Rowell led us to understand the junction of sociological and evolutionary notions of reproducing systems.

Washburn and Hamburg (1968) developed themes initiated in their collaboration in 1957, when Washburn spent a year as a fellow at the Stanford Center for Advanced Study in the Behavioral Sciences, and furthered in 1962–63 when Washburn and Hamburg organized at the centre a full year of conferences and collaboration among the new, exciting, world-wide community of primatologists. In 'Aggressive behavior in Old World monkeys and apes', the two collaborators introduced their work as part of the study of the forces that produced humankind. They wished to pay attention to unique human biology and unique conditions of human evolution. They saw aggression as a fundamental adaptation or functional complex common to the entire primate order, including human beings. 'Order within most primate groups is maintained by a hierarchy, which depends ultimately primarily on the power of males . . . Aggressive individuals are essential actors in the social system and competition between groups is necessary for species dispersal and control of local populations' (1968, p. 282). The biology of aggression has been extensively studied and

seems, they argue, to rest on similar hormonal and neural mechanisms, modified in primates, and especially in humans, by new brain complexes and extensive learning. In non-human primates, aggression is constantly rewarded, and, the authors maintain, aggressive individuals (males) leave more offspring. So they argue for selection of a system of co-adapted genes involving complex feedback among motor anatomy, gestural anatomy, hormones, brain elements, and behaviour. Presumably, all parts of the aggressive complex evolve. The functions requiring aggression did not abate for humankind, Hamburg and Washburn believe. Protection, policing, and finally hunting all required a continued selection for male organisms who easily learned and enjoyed regulated fighting, torturing, and killing:

Throughout most of human history societies have depended on young adult males to hunt, to fight, and to maintain the social order with violence. Even when the individual was cooperating, his social role could be executed only by extremely aggressive action that was learned in play, was socially approved, and was presumably gratifying. (1968, p. 291)

But with the advance of civilization, this biology has become a problem. It is now often maladaptive because of our accelerating technological progress. Our bodies, with the old genetic transmission, have not kept pace with the new language-produced cultural transmission of technology. So now, when social control breaks down, we must expect to see pathological destruction. Hamburg and Washburn's examples here are Nazi Germany, the Congo, Algeria, and Vietnam! The lesson is that we must face our *nature* in order to control it. 'There is a fundamental difficulty in the fact that contemporary human groups are led by primates whose evolutionary history dictates, through both biological and social transmission, a strong dominance orientation' (1968, p. 295). This logic has been developed to posit a need for scientifically informed, rational controls to replace pre-scientific customs: 'But an aggressive species living by prescientific customs in a scientifically advanced world will pay a tremendous price in interindividual conflict and international war' (p. 296). The lesson here, the liberal scientist argues, is not to favour a particular social order – those are political and value questions – but to establish the preconditions for all advanced society, namely, scientific management of now inefficient, maladaptive, obsolescent biology. We are only one product, and one subject to considerable breakdown. On the personal level, psychiatric therapy is a species of repair work; on the social level, scientific policy dictates we use our skill to update our biology through social control. Our system of production has transcended us; we need quality control.

But before despairing that society is doomed to hierarchies and dominance relations regulated by scientific management, let us ask more closely

what convinces Washburn and Hamburg that we, or at least males, have a woefully aggressive nature. After all, human males do not have the so-called fighting anatomy of many primate males – the dagger-like canines, associated threat gestures so appropriate for ethological analysis, great difference in male and female body size, or extra structures such as a mane to enhance one's threatening aspect. Nor do we have appeasement gestures to placate aggressors. Why argue that we do have an aggressive, authority-requiring brain? The line leading to the genus *Homo*, Washburn judges, was bipedal and tool-using very early. Selection pressures favoured increased tool use, which in turn made possible the hunting way of life, evolution of a big brain, and language. Human males no longer fought with teeth and gestures but with words and handmade weapons. We lack big canines because we make knives and hurl insults. The selection pressures requiring aggression did not abate, but the structural basis for the function evolved in harmony with the whole adaptational complex of a new way of life. This argument itself relates to Washburn's basic reformulation of physical anthropology, beginning in the 1940s, as part of the synthetic theory of evolution, and to his successful efforts to promote primate behaviour studies in the study of human evolution.

Washburn earned his PhD in physical anthropology at Harvard in 1940. His training was in traditional anthropometric methods and primate anatomy, and he taught medical anatomy at Columbia College of Physicians and Surgeons until 1947, when he moved to the University of Chicago. He had accompanied the 1937 Asia Primate Expedition, from which C.R. Carpenter produced the first monograph on gibbon behaviour and social system. But Washburn felt Carpenter then had little sense of the exciting possibilities of the concept of social system. His own task on the expedition was anatomical collecting, that is, shooting specimens. By the mid-1940s Washburn was practising physical anthropology as an experimental science; by 1950 he was developing a powerful programme for reinterpreting the basic concepts and methods of his field in harmony with the new population genetics, systematics, and palaeontology of Dobzhansky, Mayr, and Simpson. By 1958, he had a Ford Foundation grant to study the evolution of human behaviour in a complex manner, including provision for field studies of baboons in East Africa. A year later, now at Berkeley, he developed funding for one of the first experimental primate field stations in the United States. From the beginning of his career, he lectured, wrote popular texts, made pedagogical films, reformed curricula on all educational levels, and promoted successful careers of now well-known figures in evolution and primatology.

This is not the place to explore the origins of Washburn's ideas, nor his organization of a very large research and education programme, but only to note essential features in relation to the hunting thesis and primate

behaviour.⁵ The purpose is to begin to recognize how Washburn's career as a careful, experimental scientist has been part of the scientific and social controversies on human nature as the foundation for the human future. We must understand how Washburn could simultaneously be the co-author of the article on evolution of aggression, an opponent of sociobiology, alternately a hero and villain for Robert Ardrey, a favourite of some Marxist feminists, and the teacher of both sociobiological feminist Adrienne Zihlman and of sociobiological sexist Irven DeVore. He is rightly all these things and unusually consistent and unified in his methods, theories, and practices. Perhaps the key to Washburn is that he has produced a fundamental theory with tremendous implications for the practice of many sciences and for the rules of speculative evolutionary reconstruction. In Kuhnian terms, Washburn seems to have something basic to do with scientific paradigms. In Marxist terms, he has to do with alienated theorizing of the established disorder.

Washburn's fundamental innovation in physical anthropology was evident in the publication of his widely reprinted papers, 'The new physical anthropology' (1951a) and 'The analysis of primate evolution with particular reference to man' (1951b). He applied the new population genetics to the study of primate evolution. For Washburn population genetics meant that the process of evolution was the crucial problem, not the fossil results. Therefore, selection and adaptation were his central concepts. Adaptive traits could only be interpreted by understanding conditions or forces capable of producing the traits. The first problem that confronted the physical anthropologist was how to identify a 'trait'. Washburn practised a new kind of theoretical and practical dissection of the body into 'functional complexes', whose meaning had to be sought in their action during life. For example, instead of measuring the nose, he analysed the forces in the central region of the face from chewing and growth. That task required model experimental systems of living animals. Instead of setting up scales of evolution based on brain enlargement, he analysed regions of the body involved in adaptive transformations related to locomotion, eating, and similar functions. In sum, 'The anatomy of life, of integrated functions, does not know the artificial boundaries which still govern the dissection of a corpse' (1951a, p. 303).

Washburn was part of a larger revolution in physical anthropology accompanied by the discovery of new fossils, dating techniques, experimental possibilities, and more recently, molecular taxonomy. One of the revolution's central objects was the small-brained South African human-ape, *Australopithecus*. 'The discovery of the South African man-like apes, or small-brained men, has made it possible to outline the basic adaptation which is the foundation of the human radiation' (1951b, p. 70). The origin of the

human radiation was like any other mammalian group's, though its consequences were decidedly novel. 'But the use of tools brings in a set of factors which progressively modifies the evolutionary picture. It is particularly the task of the anthropologist to assess the way the development of culture affected physical evolution' (1951b, p. 71). Evolutionary and social functionalism again come together; both, for Washburn, are analyses of the meaning of living systems, of action, of ways of life. From the 1950s Washburn maintained that functional anatomy and the synthetic theory of evolution laid to rest for ever the old conflicts of physical and social anthropology.

In 1958, Washburn and his former student, Virginia Avis, contributed a paper to a symposium on behaviour and evolution, which had been organized, beginning in 1953, to effect a synthesis of comparative psychology and the synthetic theory. Washburn's emphasis on the importance of behaviour made his interest in the psychological consequences of evolutionary adaptation natural. In that paper, 'The evolution of human behavior', Washburn and Avis (1958) developed the consequences of the hunting adaptation, including enlarged curiosity and mobility, pleasure in the hunt and kill, and new ideas about our relation to other animals. Perhaps most important, 'Hunting not only necessitated new activities and new kinds of cooperation but changed the role of the adult male in the group . . . The very same actions which caused man to be feared by other animals led to more cooperation, food sharing, and economic interdependence within the group' (pp. 433-4). The human way of life had begun.

From seeing behaviour first as motor activity and then as psychological orientations, it was a short, logical step to looking at behaviour as social system. Beginning in 1955, almost casually, Washburn investigated not only actions of individual organisms but of social systems. The baboon studies of Washburn and Irven DeVore, with all their emphasis on male roles in protection and policing as models of pre-adaptations to a human social system, were appropriate outgrowths of evolutionary functional anatomy. Differences between human and monkey society were always highlighted; Washburn never engaged in chain-of-being reconstructions. He looked at animal social systems the same way as he looked at forces determining growth in kitten skulls – as model systems for particular problems in interpreting skull formation in fossils. His was an experimental, comparative biological science based on function. But the baboon model system drove home a lesson: troop structure came from dominance hierarchies of males. Hunting transformed such structures but only to produce the special roles of the co-operating male band. The reproductive function of females, and the social continuity of matriline, remained a conservative pattern reinforced by bigger-headed, more dependent infants.

The classic paper which brings together the anatomical, psychological,

and social consequences of hunting in setting the rules for culture based on human nature is 'The evolution of hunting', by Washburn and C. S. Lancaster (1968). This paper has earned Washburn his poor reputation in socialist and feminist circles. Its appearance in a symposium emphasizing the hunting nature of man in the midst of years of challenge to sexual, economic, and political power is part of the social situation of contemporary evolutionary reconstruction. Washburn is not an ideologue; he is a scientist and educator. That is the point. Interpreting human nature is a central scientific question for evolutionary functionalism. The past sets the rules for possible futures in the 'limited' sense of showing us a biology created in conditions supposedly favouring aggressive male roles, female dependence, and stable social systems appropriately analysed with functional concepts. Telling stories of the human past is a rule-governed activity. Washburn's science changed the rules of the game to require argument from the conditions of production.

In 'Women in evolution. Part I: innovation and selection in human origins', Nancy Tanner and Adrienne Zihlman (1976)⁶ play by the new rules but tell of a different human nature, of different universals. They focus less on tools as such and more on the labour process, that is, on a new productive adaptation – gathering. They immediately place themselves within the recent population genetic developments of sociobiology. Their study explores a natural economy in terms of investment strategies for the increase of genetic capital. Yet Tanner and Zihlman deliberately appropriate sociobiology for feminist ends. They no more make themselves ideologues than Washburn has, but their practice of science is controversial both for internal reasons of debated evidence and argument, and for political reasons. They do not, at any point, leave the traditional social space of science. They can stay there, in part, because sociobiology is not necessarily sexist in the sense that Irven DeVore or Robert Trivers (1972) have made it, any more than the concept of stress necessarily leads to Hamburg's particular ideas on aggression and human obsolescence. Further, it is not easy to imagine what evolutionary theory would be like in any language other than classical capitalist political economy.⁷ No simple translation into other metaphors is possible or necessarily desirable. Tanner and Zihlman bring us face to face with fundamental questions that have barely been phrased, much less answered. How should we theorize our experience of the past and of 'nature' in new ways to build adequate concepts for scientific practice and social transformation? This question stands in a complicated relation with the internal craft rules for working within the natural sciences.

Tanner and Zihlman begin by announcing the goal of understanding human nature in terms of processes 'which shaped our physical, emotional, and cognitive characteristics' (1976, p. 585). They note the obvious fact that the hunting thesis has largely ignored the behaviour and social activity

of one of the two sexes, and is therefore deficient by ordinary criteria of evolutionary functionalism. Behaviour does not fossilize for either sex, so the problem is one of rational reconstruction, of choosing hypotheses.

Specifically, we hypothesize the development of gathering [both plant and animal material] as a dietary specialization of savanna living, promoted by natural selection of appropriate tool using and bipedal behavior. We suggest how this interrelates with the roles of maternal socialization in kin selection and of female choice in sexual selection. We emphasize the connections among savanna living, technology, diet, social organization, and selective processes to account for the transition from a primate ancestor to the emergent human species. (Tanner and Zihlman, 1976, p. 586)

This paper is clearly a normal outgrowth for Zihlman of her 1966 presentation on bipedal behaviour, in the context of hunting, to a Washburn-organized symposium of the American Anthropological Association. Titled 'Design for Man', the session included Hamburg on emotions as adaptational complexes and the problem of maladaptive, obsolete patterns.

Like Washburn, Tanner and Zihlman argue from animal model systems and from the most recent genetic theory applied to populations. They see chimpanzees as the most closely similar of all living animals to the stem population that probably gave rise to apes and hominids. So chimpanzees make better mirrors, or models, than baboons do for glimpses of the evolution of the human way of life. The authors add to the traditional genetic parameters of the synthetic theory (drift, migration, and so on), the sociobiological genetic concepts of inclusive fitness, kin selection, sexual selection, and parental investment. Understanding changes in gene frequencies of populations from selection pressures operating on individuals remains the goal. They note lots of tool use by chimpanzees, with a sex difference in the behaviour. Females make and use tools more often, although the males seem to hunt more readily. Rigid dominance hierarchies do not occur, although the concepts of high ranks and influence seem useful. The social structure is flexible, but not random. Social continuity seems to flow through continuing associations of females, their young, and associates.

The transitional population to hominids is imagined to have moved into the savannah, a new adaptive zone. 'A new way of life is initiated by a change in behavior; the anatomical changes follow' (Tanner and Zihlman, 1976, p. 586). The new behaviour was greatly enlarged dietary choice accompanied by tool use. Gathering was the early critical invention of hominids. Food-sharing with ordinary social groups of females and offspring (including male sharing with these groups) resulted. Digging sticks, containers for food, and above all, carrying devices for babies were extremely likely early technological innovations related to the new diet and sharing habits.

Knowledge of a wide range of plants and animals, as well as their seasons and habits, became important. Selection pressure for symbolic communication increased. The predation dangers of the savannah were probably dealt with by cunning not fighting, so hominids reduced the need for baboon-like dominance and male fighting anatomy. The flexible chimp social structure probably became even more opportunistic, allowing better understanding of the basis for human cultural diversity. Like Rowell, Tanner and Zihlman take every opportunity to emphasize human possibility and variety. Gathering of plants and animals was unlikely to maintain much selection pressure for an aggressive biology. Cognitive processes, on the other hand, were greatly elaborated in the new productive mode.

At this point, Tanner and Zihlman make use of mother-centred units to introduce kin and sexual selection and parental investment. New selection pressures put a premium on great sociability and co-operation. Babies were harder to raise, and bisexual co-operation would be useful. Males learned the friendly interaction patterns, even with strangers, which became crucial to the human way of life based on linguistic communities, small bands, and frequent outbreeding. But maintenance of a fighting anatomy including big canines and stereotyped threat gestures would be incompatible with the new functional behaviours. Females would mate more readily with friendly, non-threatening males. Female sexual choice has been shown to be general in mammalian groups, and the hominid stem was not likely to have been an exception. Two things leap at the reader who has followed Zuckerman's and Washburn's hunting arguments. First, female receptivity has been renamed female choice, with large genetic consequences. Second, the anatomy of the reduced canines is reinterpreted when different behaviour and different functions are postulated.

Tanner and Zihlman believe anthropology as a whole is better served by their different reconstruction, based on similar evidence.

Observers usually begin from their own perspective, and so inadvertently the question usually has been: how did the capacity and propensity for adult Western male behaviors evolve? This viewpoint offers scant preparation for comprehending the wide range of variability in women's roles in non-Western societies or for analyzing the changes in the roles of men and women which are currently occurring in the West. (Tanner and Zihlman, 1976, p. 608)

In other words, evolutionary reconstructions condition understanding of contemporary events and future possibilities. Tanner and Zihlman, in their interpretation of the tool-using adaptation, avoid telling a tale of obsolescence of the human body caught in a hunting past. The open future rests on a new past.

Focusing on the categories of reproduction and production, I have traced four major positions on human history and human nature. All were argued strictly within the boundaries of modern physiology, genetics, and social theory. All four hinged on the concept of function and recognized the 'liberal' doctrine of the autonomy of nature and culture. It has been against the rules to argue from a position of biological reductionism. But the goal of each tale has been a picture of human universals, of human nature as the foundation for culture. Ironically, reconstructions of human nature useful to feminists were derived from two of the theories most despised by socialist-feminist thought: functionalism and sociobiology. They have been criticized as ideological justifications of unjust economic and political structures, as rationalizations for the reproduction of present relations of the body and body politic. Obviously, as Rowell, Tanner, and Zihlman show, these theories can be deployed for other ends: to stress human and animal variability, complexity, capacity for change. Feminists can engage seriously, then, in the biosocial debate from within the sciences.

We must, however, be acutely aware of the dangers of using old rules to tell new tales. This is compatible with a larger refusal to pretend that science is either only discovery, which erects a fetish of objectivity, or only invention, which rests on crass idealism. We both learn about and create nature and ourselves. We must also see the biosocial sciences from the point of view of the process of resolving the contradiction between, or the gap between, human reality and human possibility in history. The purpose of the sciences of function is to produce both understanding of meaning and predictive means of control. They show both the given and the possible in a dialectic between the past and the future. Often, the future is given by the possibility of a past. Sciences also act as legitimating meta-languages that produce homologies between social and symbolic systems. That is acutely true for the sciences of the body and the body politic. In a strict sense, science is our myth. That claim does not in any way vitiate the discipline scientific practitioners impose on each other to study the world. We can both know that our bodies, other animals, fossils, and what have you are proper objects for scientific investigation, and remember how historically determined is our part in the construction of the object. It is not an accident of nature that our social and evolutionary knowledge of animals, hominids, and ourselves has been developed in functionalist and capitalist economic terms.⁸ Feminists must not expect even arguments that answer clear sexist bias within the sciences to produce adequate final theories of production and reproduction as well. Such theories still elude us, because we are now engaged in a political-scientific struggle to formulate the rules through which we will articulate them. The terrain of primatology is the contested zone. The future is the issue.

Chapter Three

The Biological Enterprise: Sex, Mind, and Profit from Human Engineering to Sociobiology

Life can be moulded into any conceivable form. Draw up your specifications for a dog, or a man . . . and if you will give me control of the environment, and time enough, I will clothe your dreams in flesh and blood . . . A sensible industrial system will seek to put men, as well as timber, stone, and iron, in the places for which their natures fit them, and to polish them for efficient service with at least as much care as is bestowed upon clocks, electric dynamos, or locomotives.

Frank Parsons, Human Engineer, 1894

Now they swarm in large colonies, safe inside gigantic, lumbering robots, sealed off from the outside world, communicating with it by remote control. They are in you and me; they created us, body and mind; and their preservation is the ultimate rationale for our existence. They have come a long way, those replicators. Now they go by the name of genes, and we are their survival machines.

Richard Dawkins, Sociobiologist, 1976

Part of remaking ourselves as socialist-feminist human beings is remaking the sciences which construct the category of 'nature' and empower its definitions in technology. Science is about knowledge and power. In our time, natural science defines the human being's place in nature and history and provides the instruments of domination of the body and the community. By constructing the category nature, natural science imposes limits on history and self-formation. So science is part of the struggle over the nature of our lives. I would like to investigate how the field

of modern biology constructs theories about the body and community as capitalist and patriarchal machine and market: the machine for production, the market for exchange, and both machine and market for reproduction. I would like to explore biology as an aspect of the reproduction of capitalist social relations, dealing with the imperative of biological reproduction. That is, I want to show how sociobiology is the science of capitalist reproduction.

TABLE 1: TRANSFORMATIONS IN LIFE SCIENCE IN THE TWENTIETH CENTURY

Pre-Second World War Represented by R.M. Yerkes	Post-Second World War Represented by E.O. Wilson
psychobiology	sociobiology
human engineering	communication control
organism	cybernetic machine
physiology	systems theory
intelligence	information
person	gene
personality sciences	population genetics and ecology
sex and mind	genes and survival machines
instinct and engineering	constraints and choice or redesign of trajectories
time-motion studies	ergonomics
human relations management	sociotechnical systems management
adaptivity	optimization
eugenics for race hygiene	sexual investment strategies for genetic profit
nervous system for integration	sensory channels and processing centres for environmental tracking
endocrine system for integration	chemical communication for environmental tracking
homoeostasis	feedback and other control system mechanisms
superorganism	population

All items in the left-hand list are appropriate to a bioscience of organisms, in which the model of scientific intervention is medical and clinical. The nature of analysis is organic functionalism, and ideological appeals are to the fulfilment of the 'person'. All the items in the right-hand list are appropriate to an engineering science of automated technological devices, in which the model of scientific intervention is technical and 'systematic'. The nature of analysis is technological functionalism, and ideological appeals are to alleviation of stress and other signs of human obsolescence.

Between the First World War and the present, biology has been transformed from a science centred on the organism, understood in functionalist terms, to a science studying automated technological devices, understood in terms of cybernetic systems. Organic form, with its hierarchical and physiological co-operation and competition based on 'natural' domination and division of labour, gave way to systems theory with its control schemes based on communications networks and a logical technology in which human beings become potentially outmoded symbol-using devices. Life science moved from physiology to systems theory, from scientific medicine to investment management, from Taylorite scientific management and human engineering of the person to modern ergonomics and population control, from psychobiology to sociobiology.

This fundamental change in life science did not occur in a historical vacuum; it accompanied changes in the nature and technology of power, within a continuing dynamic of capitalist reproduction. This chapter sketches those changes in an effort to investigate the historical connection between the content of science and its social context. The larger question informing this critique is how to develop a socialist-feminist life science.¹

Because science is part of the process of realizing and elaborating our own nature, of constituting the category of nature in the first place, our responsibility for a feminist and socialist science is complex. We are far from understanding precisely what our biology might be, but we are beginning to know that its promise is rooted in our actual lives, that we have the science we make historically. As Marx showed for the science of wealth, our reappropriation of knowledge is a revolutionary reappropriation of a means by which we produce and reproduce our lives. We must be interested in this task.

This chapter compares and contrasts the biologies of Robert Mearns Yerkes and E.O. Wilson to show the transformation of biology from a science of sexual organisms to one of reproducing genetic assemblages. Throughout I focus on the machine and market as organizing ideas in life science. Table 1 outlines the categories of comparison. It is important to note that this chapter does not claim that Yerkes and Wilson singly built intellectual systems with conscious relations to the needs of patriarchal capital; rather it examines them as representing important formations, so as to give an idea where to continue a critical reading of classical biology in the process of formulating another biology.²

Yerkes was committed to development of personality sciences based on the model of physiology and scientific medicine.³ As the goal of scientific management in industry in that period was the microcontrol of individual workers, establishment of co-operative hierarchies, and clear separation of control functions from manual work, Yerkes' psychobiology was founded on

the individual organism and hierarchies of intelligence and adaptivity that were appropriate to the creation of rationally managed, modern societies. He built a complex evolutionary picture of the relation of sex and mind, raw material and engineering, instinct and rational control, that was appropriate to a genuinely usable capitalist science.

But by the end of his career around 1940, Yerkes' science was already outmoded. It was being replaced by a different engineering perspective, based not on physiology, but on the physical sciences' analysis of information and energy in statistical assemblages.⁴ The physiology of sexual organisms gave way to biochemistry, structural analysis, and molecular genetics of information machines: integrons, replicators, self-assembling biological subsystems such as viruses and cell organelles and populations – the new books of nature to be read by mathematics. It is not an accident that modern genetics is pursued as a linguistic science, with attention to signs, punctuation, syntax, semiotics, machine read-out, directional information flow, codons, transcription, and so on (Jacob, 1974; Watson, 1976). The social goal of the new life science was clearly statistical control of the mass through sophisticated communications systems. Similarly, the damping and control of variation, prediction of large-scale pattern, and development of optimization techniques in every kind of system became a basic strategy of social institutions. Further, everything has become a system. The search has been for evolutionary stable strategies for maximizing profit. In life science, sociobiology is a mature fruit of this approach; it is genuinely a new synthesis that makes many distinctions between natural and social science outmoded.⁵

Robert Mearns Yerkes (1876–1956), in a lifetime of committed work in psychobiological research and science promotion and management, established the first comprehensive permanent laboratory for the study of anthropoid apes as models for human beings. Between 1924 and 1942, through Yale University and the Rockefeller Foundation, Yerkes assembled the funding, animals, researchers, buildings, maintenance staff, and publications which have made it possible to breed, rear, and study chimpanzees in captivity. He also made the first field studies of wild primate behaviour possible (Hilgard, 1965). On a wider level, Yerkes worked to establish the utility of primates for interpreting the place of human beings in scientifically managed corporate capitalism – called nature. His investigations in mental and sexual psychobiology included designing tests for all aspects of mental functions in organisms ranging from daphnia and dancing mice to psychopaths, soldiers, and corporate managers. Yerkes also examined natural dominance and co-operation in the evolutionary interrelation of sexual instinct and rational mind.⁶ This work was a central part of his explicit project of scientific engineering as a proper replacement for the irrationalities of received culture.⁷

Yerkes had no interest in rationalizing conservative social forms. Science has constructed nature as a category facilitating redesign of natural objects, including society. Yerkes saw nature and society in managed capitalist terms. Nature was a problem in test design. Adaptivity meant solving the problem of the rational control of nature on the level of individual organisms and their social analogues – families, labour groups, and other superorganisms.⁸ The scientific frameworks for interpreting primate behaviour and biology have changed radically since the early years of Yerkes' work before the First World War. Knowledge of primates has corresponded to general developments in biology, psychology, and sociology, as well as to political conflict. The ways arguments have been constructed for relating primate science to human needs have also changed. But a constant dimension of primate studies has been the naturalization of human history; that is, making human nature the *raw material* rather than the *product* of history. Engineering is the guiding logic of life science in the twentieth century.

Human engineering sought to construct a control hierarchy, modelled on the individual organism with the nervous system on top. This organismic model facilitated the conception of society as a harmonious, balanced whole with proper distribution of function. The interrelations of nervous and reproductive systems, the two main integrative mechanisms of the organism, provided a microcosm of life, including social life (superorganism). The principal scientific goal was a biological theory of co-operation based on management hierarchies. What had to be managed were organic life, instinct, sex. At the top of the organism-pyramid was mind, permitting altruism to mitigate the excesses of competition. Psychobiology, as sociobiology later, was faced with rationalizing altruism in a competitive world – without threatening the basic structure of domination.

ROBERT YERKES: THE PRIMATE LABORATORY AS PILOT PLANT FOR HUMAN ENGINEERING

It has always been a feature of our plan for the use of the chimpanzee as an experimental animal to shape it intelligently to specification instead of trying to preserve its natural characteristics. We have believed it important to convert the animal into as nearly ideal a subject for biological research as is practicable. And with this intent has been associated the hope that eventual success might serve as an effective demonstration of the possibility of re-creating man himself in the image of a generally acceptable ideal.

Robert Yerkes, Chimpanzees, A Laboratory Colony

By the 1930s, human engineering in the form of personnel management integrated the methods of the physical, biological, and social sciences in order to produce harmony, team work, adjustment. The structure of co-operation involved the entire complex division of labour and authority in capitalist production and reproduction. Co-operation most certainly included rational organization of hand and head, of subordination and dominance, of instinct and mind. Motivation of co-operation was a management problem (Mayo, 1933; Baritz, 1960; Bravermann, 1974).

It was also a biomedical problem, necessitating detailed physiological knowledge of the 'irrationalities', which could become pathological – instinct, personality, and culture. These three were closely tied to organic sex, and led to the proliferation of scientific disciplines such as endocrinology, gender-differentiated personality studies, Freudian psychotherapy, anthropology based on personality and culture, eugenic doctrines of race hygiene, and sexual counselling through the birth control movement.⁹ Despite controversies among all these approaches, they shared a grounding in organic functionalism based on sexuality. Engineering meant rational placement and modification of human raw material – in the common interest of organism, family, culture, society, and industry. Human engineering was a kind of medical encouragement of natural homeostatic mechanisms of intelligent integration. The life sciences which studied organic capacity and variation from a physiological viewpoint provided the scientific underpinnings for the application of human engineering. Yerkes helped build those sciences.

Yerkes received his PhD at Harvard in 1902. Before the First World War, his research in Cambridge and Boston concerned the sensory psychophysiology and mental capacity of a wide range of organisms. Sensory physiology was intimately related to modes of 'adaptivity', or learning, in both individual and evolutionary frameworks. Early in his career Yerkes was interested in extending his work to primates, and envisaged a comprehensive primate research station which would include physiology, learning, and social behaviour. Yerkes worked within the framework of comparative psychology, which studied evolution of animal behaviour as a chain of being, a series of increasingly complex physiological organizations, best shown in growth of intelligence. Having defined intelligence as problem-solving behaviour, Yerkes relied on the construction of testing apparatus for comparing learning strategies of different species and individuals within species. The relation to a hierarchically conceived physiology as the model for this psychology cannot be overstressed. As scientific medicine was based on experimental physiology, so too psychological therapies relied on experimental psychology (Yerkes, 1913, 1921).

In studying adaptivity of primates, Yerkes (1927b, 1928) developed the

notion of three stages of complexity, which he actually called monkeying, aping, and thinking. His pre-war ideational studies of the orangutan Julius and of patients in the Boston Psychopathic Hospital were part of the development of tests applicable to all sorts of problems of organic inventory. The First World War supplied an opportunity for demonstrating the utility of this psychophysiological natural science. Yerkes is well known for helping devise the intelligence tests for conscripts; these test results were frequently used for immigration restriction and other racist purposes during and after the war. It is less well known that Yerkes designed his tests under the auspices of the army surgeon general and conceived the work as part of the medical management of society (Kevles, 1968; *Ann Arbor Science for the People*, 1977, pp. 21–57; Cravens, 1978, pp. 80–5, 181–8).

After the war, Yerkes remained in Washington, DC, forming an economic and political base for his lifelong goal of a primate research station. From 1919 until accepting a professorship in Yale University's new Institute of Psychology in 1924, he worked within the National Research Council of the National Academy of Sciences.

Two committees formed under the auspices of the National Research Council (NRC) are relevant to the themes of this chapter: the Committee on Scientific Aspects of Human Migration (CSAHM) and the Committee for Research on Problems of Sex (CRPS). Yerkes was chairman of both, the CSAHM from 1922 to 1924 and the CRPS from 1922 to 1947. Both committees were set up to study human variability for purposes of rational social management policy. Neither committee worked from a population perspective, but rather from a physiological model of organic capacity, variation, and health. Widespread population genetic and ecology approaches to demography and to sexuality only emerged after the Second World War and were related to the elaboration of communications technology and information sciences.

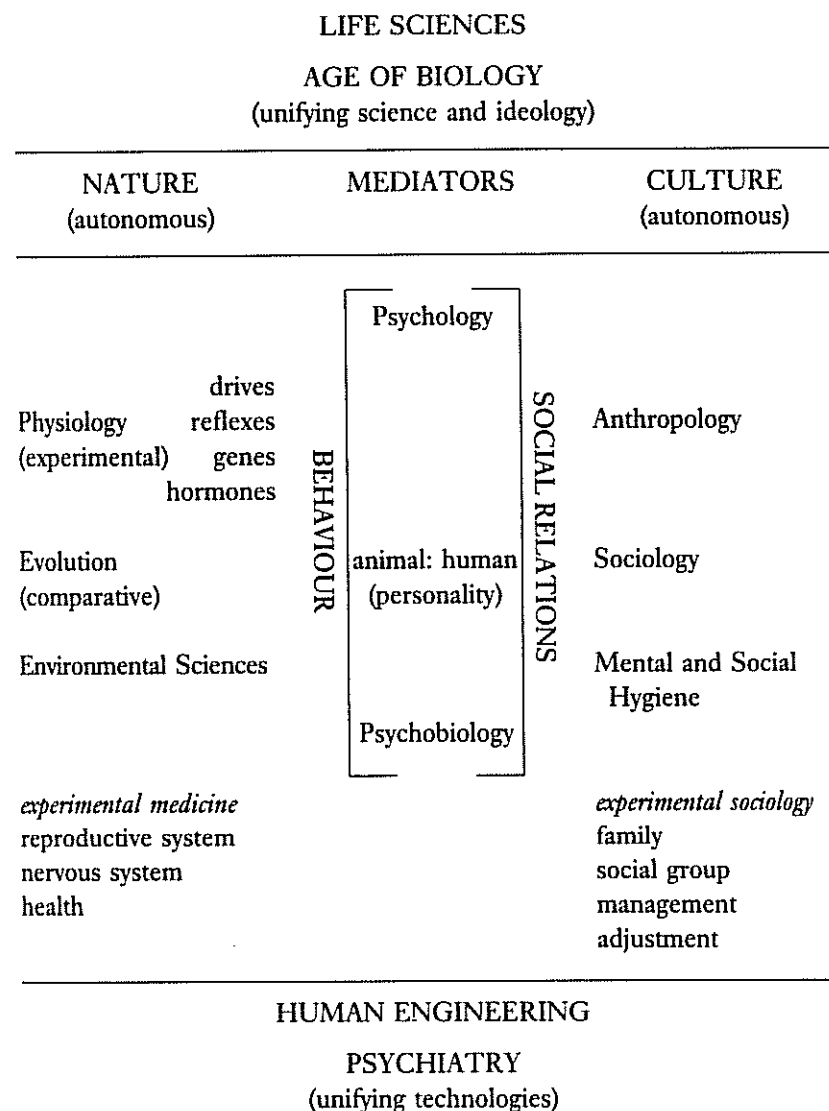
The Committee for Research on Problems of Sex grew out of efforts by the New York City Bureau of Social Hygiene to establish a structure of pure research for enlightened social policy on matters such as sex education, family counselling, eugenics, venereal disease, divorce, and birth control.¹⁰ The NRC committee was part of an effort to relate medical-physiological research to social issues. The committee sponsored work in four categories, *not* including direct action agencies:¹¹ (1) biology of sex (systematic, genetic, and physiological aspects); (2) physiology of reproduction; (3) infrahuman psychobiology of sex; and (4) human psychobiology of sex, including anthropological and social-psychological approaches. Two assumptions stand out in the records of the sex committee. First, social practice had to be based on basic research conducted and controlled by independent specialists; the parent philanthropy had no direct say about funding once the

committee was established. Second, the sex instinct was perceived to underlie the whole pyramid of life and human sciences and to be the key to understanding culture and personality. The CRPS did not conceive of science as rationalizing sexual repression. Quite the opposite: the committee in large measure played a liberalizing role.¹² It was committed to facilitating rational social engineering. Animal models for human organic capacity and variation allowed human engineering to be an experimental natural science. In that sense, Yerkes built his primate laboratory as a pilot plant for human engineering.

In consultation with a powerful old friend and colleague, Yale University President James Rowland Angell, Yerkes planned the Institute of Psychology at Yale as the home for his primate research. The Institute housed a range of graduate research on general problems of adaptation; its staff was made up of former members of the Committee on Scientific Aspects of Human Migration.¹³ These men brought with them a commitment to the scientific management of race, sex, and class, based on sciences of heredity, drives, learning, and environment, all in a biomedical context grounded in physiology. In 1924, Yerkes moved to New Haven. His early facilities consisted of his farm in New Hampshire and a converted old building at Yale, where four young chimpanzees grew up in full view of modern science. Their psychosexual and ideational development were the primary concerns. Mind and sex were a natural pair (Bingham, 1928).

In 1929, Yerkes achieved his dream, a \$500,000 grant from the Rockefeller Foundation for a permanent, large research facility on great apes. Grant proposals and Foundation correspondence were full of the relevance of the project to human social and psychological issues.¹⁴ No other goal could justify the large expense of using chimpanzees as research animals. The resulting Yale Laboratories of Primate Biology existed in three parts: (1) special laboratories for short-term work in New Haven needing special apparatus, with close co-operation with John Fulton's Department of Physiology in the Medical School; (2) a breeding colony of thirty to forty animals in Orange Park, Florida, where long-term sexual and ideational psychobiological observation and experimentation would be possible; and (3) special provision for studies of wild primates in their natural habitat, to provide base line information on the natural social physiology of the organisms.¹⁵ Research centred on the idea of evolution, and all but ignored the idea of populations. Animal behaviour was not a genetic science in Yerkes' and his contemporaries' hands. Or rather, the comparative psychologists used the word *genetic* always in the sense of the genesis of individual capacities. All this would change with the post-Second World War synthesis of ethology, neural biology, and population genetics and ecology. Figure 2 shows the picture of life science that Yerkes knew around 1930.

FIGURE 2



The life sciences focused on organisms, personalities, and cultures, around 1930. Both sides of the figure are rooted in organismic, functionalist doctrines; both involve differentiated roles for basic and applied sciences, modelled on experimental medicine.

People associated with the primate laboratories at Yale maintained two organizing ideas rooted in organismic physiology. The first was domination, which included brain region dominance, dominance in competitive inter-

actions between individuals, dominance as a personality trait related to leadership, and dominance hierarchies as social structure. Dominance was perceived as inherent to individual organisms; it was probably inheritable, just like eye colour or IQ. The second idea was co-operation – from homeostatic mechanisms at all levels, to deliberate modification of dominance in the interests of higher organization, to everyday rules for running the laboratory. Co-operation and dominance were closely connected on an organic level as forms of integration.

A choice opportunity presented itself for the experimental investigation of dominance in the context of family-centred experimental sociology. The experiment tested co-ordination of sexual drive, status hunger, masculine and feminine personality types, and evolutionary transformation to higher forms of social control. This study carried noteworthy implications for counselling and human social services by relating drive and personality to social order.

In the course of tests for delayed response and representational processes, as part of the study of the phylogeny of language, Yerkes observed that sexual periodicity and dominance–subordination appeared to influence which animal of a caged pair would come to the food chute to be examined. Yerkes (1939) then conducted competitive food experiments on four kinds of caged companions: mates, two mature females, mature with immature females, and two immature females. Pieces of banana were presented one at a time in a series of ten through a chute in the cage. Along with other information, the observer recorded which animal of the pair would take the piece. Results were correlated with sexual status of the females in terms of dominance–subordination and response by ‘right or privilege’. Right or privilege meant that in the period of maximum genital swelling of the female, that is, when the female was on heat, the ordinarily dominant male granted her the privilege of taking the banana, although dominance itself was not seen to reverse. Yet the female acted as if by right. Yerkes recognized various problems with the data: for example, observations were made in only one case for an entire cycle, and variation of the response pattern virtually swamped the postulated regularities. Tests of statistical significance were not reported. In female pairs, sexual swelling affected performance on the food priority test, but the animal offering sexual favours would be either the previously ‘dominant’ or previously ‘subordinate’ chimp. The sexual market among females was disorderly. Even among ‘mates’, it seemed presence or absence of prior ‘friendship’ greatly affected the results. But Yerkes spent most of the paper describing in detail a pair which showed clear substitution of right and privilege for dominance. The tone was simultaneously tentative and expectant that these observations were the beginning of very important studies. Yerkes’ experimental social physiology, which explored the sexual

market as fundamental to the origin of human cultural co-operation in the institution of marriage (and marriage’s ‘pathological’ form – prostitution) has a long history (Herschberger, 1948, pp. 5–14).

Dominance as a drive was not sex specific, in Yerkes’ opinion. It was the organism’s basic hunger for social status. ‘Assuming that dominance is hereditary and that inheritance is independent of sex, men and women might be expected to become creative leaders with approximately equal frequency’ (Yerkes, 1939, pp. 133–4). Culture accounted for actual observed predominance of male leaders. But the association of ‘leadership’ and biological dominance was considered natural. Yerkes was liberal-to-moderate on the sex role controversies of the day and made clear his opinion that human females should have greater ‘opportunity’ than allowed by tradition. The issue here is not whether Yerkes or other spokespeople for comparative psychobiology were or were not liberals in their own time, but the logic of naturalization of the issues in terms of hierarchy from instincts to rational control, through personality and associated educational and medical therapies. With the weakening of religion, comparative life science became the new bedrock for value decisions, the more evolutionarily adaptive ground for judgement. With respect to the division of labour in the family, which was the model for the division of labour in all of society, the logic of naturalization provided a cornerstone of historical explanation based on reproduction. The dynamic was management, *not* repression.

To make the above point concrete, let us follow Yerkes through his analysis of the implications of the interweaving of sex hungers with dominance drives. First, Yerkes put the entire investigation of sex drive and dominance–subordination explicitly in the context of pressing contemporary debates. Yerkes assumed that feminism was equivalent to the proposition that males and females were biologically ‘equal’; that is to say, he assumed that the concept of rights in political philosophy was properly rooted in natural economy. On ‘scientific grounds’, Yerkes firmly rejected the proposition that males were mentally superior, or, for that matter, naturally dominant. Males and females had the same psychological (ideation) and drive (motivation) structure. But as a consequence of hormonal structures there were differences in expression of drives. The result was personality. Life science required a physical marker for the internal state. Yerkes’ work articulated the relation of psychobiology to contemporary biology and physiology of sex, the first two categories of the Committee for Research on Problems of Sex’s promotional programme. If the division of labour in society could be correlated with the differences in drive expression, then the feminists of Yerkes’ time were misguided (Yerkes, 1943, p. 69).

‘Many clear-cut sex contrasts appear in the varied and complex expressions of dominance and subordination, leadership and control, aggression

and defense. To these,' Yerkes (1943, p. 71) wrote, 'as uniquely important in the further description of masculinity and femininity, attention is especially invited.' In the context of discussing differentiated techniques of social control adopted by males and females, Yerkes described biologically determined differences in drive expression. The existence of chimpanzee differences in 'techniques of social control' suggested that human modes were also psychobiologically legitimated and inevitable.

In a word, the masculine behavior is predominantly self-distracting; the feminine, primarily favor-currying and priority-seeking . . . To the observer the male seems often to be trying hard to blot out awareness of his subordination; the female, by contrast, to be hopefully trying to induce the male to give place to her at the chute . . . As for the females, wiles, trickery, or deceitful cunning, which are conspicuous by their absence in the male list, are favorite resources. But even more so are sexual allure and varied forms of solicitation . . . That the female is, chameleon-like, a creature of multiple personality, is clear from our observations. (Yerkes, 1943, p. 83)

Yerkes based these 'observations' on the experimental sociology of the food chute test. He did not leave the lesson for the *limits* of cultural formation of personality, and therefore of possible social change, to the imagination:

I am impressed by the contrasted attitudes and activities revealed by the competitive food situation, and I offer them as evidence that male and female chimpanzees differ as definitely and significantly in behavioral traits as in physique. I am not convinced that by reversal of cultural influences the pictures characteristic of masculinity and femininity can be reversed. (Yerkes, 1943, p. 85)

This opinion should be evaluated in light of Yerkes' extraordinary belief in human malleability and perfectibility through engineering. 'Personality differences' should be managed, not foolishly denied.

Yerkes believed the personality studies using anthropoid material were especially favourable because of the absence of social taboos and personal inhibitions.

Therefore, I submit that such observational items as appear in this report, and in related studies of the psychology of sex in the anthropoid apes, should have exceptional value for those who concern themselves with problems of social behavior, and, especially at this juncture, for those psychopathologists who are intent on appraising, perfecting, and using psychoanalytical methods of observation and interpretation. (Yerkes, 1939, p. 130)

Though less differentiated than in the human species, personality 'clearly' existed among chimpanzees 'as the unit of social organization'. Personality meant the functional whole, 'the product of integration of all the psychological traits and capacities of the organism'. In a normal personality, inherited characteristics and basic organic drives were integrated with the conscious self. In sum, personality was an absolutely central scientific object for life and human science. To have a masculine or feminine personality was not a minor matter; on its proper development hinged the adjustment and happiness of the individual and the body politic. Yerkes did not want to underestimate diversity and variability. Comparative science was designed precisely to deal scientifically with variability. For drives as central as sex and dominance and for expressions as consequential as masculinity and femininity, nurture of personality was a matter for responsible scientific service. The possibility of prescription of social role on rational grounds was at stake. If drives and personality could be measured early, proper treatment could be initiated. Yerkes was cautious, but hopeful.

If in man dominance as personality trait is highly correlated positively with leadership, as it evidently is in chimpanzee; if it is a condition of or markedly favorable to individual initiative, inquiringness, inventiveness, and creativeness; and if, further, it should prove to be reliably measurable during childhood, it may very well come to possess conspicuous value as indicator of vocational aptitudes and social usefulness and therefore also as the basis for differential educational treatment and occupational choice. Even marital advice might be affected by it, for congeniality or social fitness may depend appreciably upon similarity or the reverse in dominance as personality trait of mates or companions. (Yerkes, 1939, p. 133).

It is significant that the culture concept depended on personality in the anthropology of the 1930s. We have moved with Yerkes from instinct, through personality, to culture, to human engineering. Scientists themselves interwove sex, mind, and society in a vocation of scientific service establishing a promising new life science of comparative primate psychobiology, reaching from learning through motivation to experimental sociology. Primatology served as a mediator between life and human sciences in a critical period of reformulation of the doctrines of nature and culture. Yerkes ordered his life in the belief this science would serve to foster a higher state of individual and social consciousness, the ideological goal of liberal humanism.

Before developing the second major section of this chapter, sociobiology, it is worth returning from Yerkes' mature positions in the late 1930s on

drive and personality in primates as models for humans, to his involvement in the early 1920s with industrial personnel research.

In his capacity as temporary chairman at the 1920 annual meeting of the Personnel Research Federation, Yerkes developed themes which permeated his work for human engineering. He began with a call to 'look confidently to disinterested research to guide our race to a wise solution' of the problem of whether 'the industrial system and its products [shall] be treated as ends or means to human welfare' (Yerkes, 1922, p. 56). He saw personnel research, the study of the human factor of production, as the key discipline of the new era. 'There is every reason to believe that human engineering will shortly take its place among the important forms of practical endeavor' (p. 57). Yerkes believed that industrial systems had evolved from slavery, to the wage system, to the present system based on co-operation and that only now could the value of the person be realized. Because personnel research took the person as the proper unit of production, that discipline led the way to the scientific nurture of intelligent co-operation to replace class strife between labour and maladaptive, evolutionarily out-moded *laissez-faire* capitalism. Yerkes and his liberal peers advocated studying traits of the body, mind, spirit, and character in order to fit 'the person' perfectly into the proper place in industry. Equality clearly did not mean organic sameness; therefore it must mean that 'in the United States of America, within limits set by age, sex, and race, persons are equal under the law and may claim as their right as citizens like opportunities for human service and responsibility' (Yerkes, 1922, p. 58).

By Yerkes' logic, equality was everyone's right to occupy one's natural place determined by disinterested science. *Differences* were the essential subject for the new science. Personnel research would provide reliable information for the employment manager and proper vocational counselling for the 'person'. The 'vocations' themselves were regarded as neutral products of industrial progress so that the problem was simply one of human inventory in a democracy. The unit of analysis was the person, transformed by the scientific concept of personality which tied physiology, medicine, psychology, anthropology, and sociology into the service of management. Further, 'the person', and 'personality', retained a strong anti-materialist meaning at the same time that the associated ideology permitted scientific reduction by objective methods – like intelligence testing, motivational research, and sexual psychobiology. The wedding of philosophical idealism and natural science produced well-behaved modern children in the factory and the home. In short, '[I]ndustry now has abundant opportunity to develop suitable methods of measuring persons with respect to qualities of character, mind, and body, and to make this information immediately available in

connection with placement, vocational choice, and guidance' (Yerkes, 1922, p. 60).

Although the person should be the *object* of scientific management – an essential structure of domination in the science of co-operation – the ideology of self-expression was also intrinsic to Yerkes' exposition. The harmony of self and social management hinged on capitalist doctrines of personality. Satisfaction of basic instincts, themselves known through science, was the essence of self-expression in this model. Science, not class conflict, could provide for further human adaptive evolution. To be socially useful the drive had to be a kind of organic instinct compatible with the biological evolution of co-operation that was at last finding adequate industrial development. Yerkes logically collapsed the scientific object of personality into the spiritual value of the person: 'It now remains for personnel research to effect a still more significant and beneficial revolution or reformation [than the invention of machines] by making available adequate knowledge of man in all his essential aspects and relations, and by bringing into clear relief the supreme value of the person' (1922, p. 63). In rationalizing the market exchange of marriage and the productive machine of industry, comparative psychobiology took its place among the life and human sciences theorizing nature and humanity according to the logic of capitalist patriarchy.

SYSTEMS ENGINEERING AND SCIENCES OF INVESTMENT MANAGEMENT: SOCIOBIOLOGY

Sex is an antisocial force in evolution . . . When sexual reproduction is introduced, members of the group become genetically dissimilar . . . The inevitable result is a conflict of interest . . . The outcomes of these conflicts of interest are tension and strict limits on the extent of altruism and the division of labor.

E.O. Wilson, *Sociobiology: The New Synthesis*

Organic engineering based on the person is not the dominant form of life science in the late twentieth century. It can even be argued that biology has ceased to exist and that the organism has been replaced by cybernetic systems, which have radically changed the connections of physical, life, and the human sciences.¹⁶ Such claims are made by sociobiologists, and I think they have a strong case. How did it happen? What is the result, especially for the relations of sex, mind, and profit? This chapter can explore only a fraction of the revolution in biology that has resulted in molecular biology,

population genetics and ecology of ecosystems, and sociobiology. By the mid-1930s, Yerkes' psychobiology, as well as the research programmes of many of his peers, was in trouble at the Rockefeller Foundation. Warren Weaver, the new head of the Division of Natural Sciences, had quite a different vision of the future of biology and of engineering as a life science. Weaver was both an instrument and a sign of much larger forces.¹⁷ By the early 1960s, the communications revolution was established in power; its effects can be followed in biology in four revealing, collective, authoritative texts, culminating in a well-published, state-of-the-art introductory biology text by E. O. Wilson and his colleagues.¹⁸ The themes of machine and market in the constitution of capitalist life science recur in the work of Wilson (born 1929, PhD from Harvard 1955) and his many peers. Sociobiology is a communications science, with a logic of control appropriate to the historical conditions of post-Second World War capitalism.

The communications revolution changed the strategy of control from organism to system, from eugenics to population management, from personnel management to organization structures (sociotechnical systems and ergonomics) based on operations research (Lilienfeld, 1978, ch. 4). A communications revolution means a re-theorizing of natural objects as technological devices properly understood in terms of mechanisms of production, transfer, and storage of information. Changes in the technology of actual communications systems provided part of the material foundation of fundamental scientific reformulations. War and problems of military management encouraged new developments in science. Operations research began with the Second World War and efforts to co-ordinate radar devices and information about enemy position in a total or systems way, which conceived of the human operator and the physical machinery as the unified object of analysis. Statistical models were increasingly applied to problems of simulation and prediction for making key decisions. After the war, the explosive development of electronics industries and communications technology was increasingly tied to strategies of social and military planning to devise and manage stable systems organized around several axes of variation.¹⁹ Knowledge about range of variation and interaction effects among classes of variables replaced concern for individual states. The computer, a communications machine, both effected and symbolized new strategies of control.

Let us grant that communication means control – but for what? And does that particular goal really allow the labelling of whole scientific structures as capitalist in any deep way? Without suggesting a final answer to the second question, let us look at the first. Complex stable configurations, stable evolutionary strategies, were essential to realization of profit in immensely complex economic and political circumstances. The problem which systems

theory addressed was the maintenance and maximization of profit in crisis-ridden post-Second World War capitalism. The range of intermediate structures between extraction of surplus value and realization of profit required a whole set of discourses and technologies that constituted the communications revolution.

No natural or human science has been unaffected by these technical and theoretical transformations. Precisely how each scientific discourse relates to these historical changes is a matter for detailed study; it is certain the connections will not often be direct or simple.²⁰ But it is a striking fact that the formal theory of nature embodied in sociobiology is structurally like advanced capitalist theories of investment management, control systems for labour, and insurance practices based on population disciplines. Furthermore, sociobiology, like all modern biologies, studies a control machine as its central object. Nature is structured as a series of interlocking cybernetic systems, which are theorized as communications problems. Nature has been systematically constituted in terms of the capitalist machine and market. Let us look first at the market.

The market is best approached in terms of the history of the concept of natural selection. Contemporaries realized that a Darwinian natural economy, the competitive struggle of all against all for profit, suggested troubling parallels to political economy. Darwin himself realized his debt to Thomas Malthus; scarcity was the motor of nature as well as of history (Malthus, 1798, pp. 26–30, 73–5, 98). Biological populations increased at a rate that guaranteed permanent scarcity, as well as permanent technical improvement in the means of production. Progress and scarcity were the twin forces in capitalist development.²¹ Reproduction of biological organisms seemed the basic process in both nature and history, and reproduction was inherently competitive. Scarcity seemed inevitably linked to a natural process, and not to a historical limiting form of appropriation of the product of human production. Reproduction, not production, seemed the proper focus for a natural science of society. Similarly, as Marx noted, bourgeois political economists focused on equal and competitive exchange in the market, while obscuring the relations of domination in production. Those relations were enforced by particular mechanisms (including technology) which were designed to transfer the locus of control away from the worker. All of this is familiar. From this point of view, sociobiology is merely an extension and development of the theory of natural selection.

Sociobiology (Wilson, 1975, p. 10) is a biological understanding of *groups* – societies and populations. As for all capitalist science, the fundamental problem needing explanation is the combination of individuals for the common good. From a starting point of atomic individualism, reproduced in Darwin's theory of natural selection, *altruism* needed explanation; it seemed

an irrationality for a consistent theory of selection. Altruism in sociobiology is defined as 'self-destructive behavior performed for the benefit of others' (Wilson, 1975, p. 578). How could individuals profit in the long run, if they wasted time and courted danger in self-destructive generosity? The problem seemed particularly acute in the most advanced natural societies – social insects and non-human primates, not to mention human orders. Sociobiology's solution is the quantitatively sophisticated extension of natural selection and population genetics, producing the notion of 'inclusive fitness: the sum of an individual's own fitness plus all its influence on fitness in its relatives other than direct descendants; hence the total effect of kin selection with reference to an individual' (Wilson, 1975, p. 586).

The ideas related to inclusive fitness – kin selection, sexual selection, parental investment – permitted a refocusing of an old argument; that is to say, at what level can selection occur (Wynne-Edwards, 1962; Trivers, 1971, 1972)? In particular, can the social group be the locus of selection? If so, is the group a kind of superorganism, physiologically as well as genetically analogous to an individual? The answer for sociobiology is no.²² Or rather, those suggestions no longer make sense. The genetic calculus of sociobiology concerns maximization strategies of genes and combinations of genes. All sorts of phenomenal orders are possible, from asexual individuals to cast-structured insect societies with only one reproductive pair, to role-diversified societies with many reproducing members. None of these orders is the central object of concern. That noumenal object is the gene, called by Richard Dawkins the 'replicator', within the gene pool. Sociobiology analyses all behaviour in terms of the ultimate level of explanation, the genetic market place.

Bodies and societies are only the replicators' strategies for maximizing their own reproductive profit. Apparent co-operation of individuals may be a perfectly rational strategy, if long-term cost-benefit analyses are made at the level of the genes. Such analyses call for the development and application of mathematical tools directly related to political economy and the technical demands made by that science. The novel dimension in late twentieth-century political and natural economy is the shared problem of understanding very complex forms of combination, which obscure the competitive bedrock of capitalism with phenomena like altruism and liberal corporate responsibility in transnational enterprises.

In 'nature' profit is measured in the currency of genes, and reproduction or replication is the natural imperative. But reproduction is not sex. In fact, sex is a dangerous modern innovation, one so challenging to older logics of individual profit-making as to require considerable attention. Like any other capitalist system, natural replication systems are compelled to make radical innovations all the time, or be outclassed by the dynamic competition. Sex is

such an advance. Societies can be rationalized by probing the consequences of individual advantage and inclusive fitness, but the most highly integrated societies, the insects, minimize the disruptive effects of sex. Sex is a constraint on the formation of societies because sexually reproducing individuals are not identical genetically. They therefore compete with different investment strategies (Wilson, 1975, p. 314 ff).

So why risk dangerous investment strategies? Because they speed innovation – the rapid production of new genotypes which can respond to environmental changes or other contingencies. Such diversification maximizes the chances of long-term success. Through *speedy* production of new genotypes, not primarily dependent on mutation, reproducers secure a competitive advantage. Naturally, sociobiology argues, there will be some circumstances in which the dangers of sexual competition outweigh the advantages of rapid diversification. Sociobiology aims at a quantitative assessment of appropriate strategies. If sex ceases to provide an edge, it will have to go. But any society with most of its members engaging in sexual reproduction cannot hope for real peace. The best to be anticipated is a harmonious management of competing investment strategies, in such a way that the system as a whole (natural evolution) is preserved.

A consequence of this analysis of sex is the attention given to competing interests of males and females in reproduction. Some of the best work on parental investment strategies has been done on birds, allowing an understanding of such issues as clutch size and male and female differences in behaviour (especially readiness to mate).²³ The claim has been made that sociobiology establishes the ultimate equality of males and females by showing that they compete equally – if by different strategies – in the only game that counts, amassing genetic profit. The different strategies are a function of the different energetic commitment to reproduction that the sexes make. Mates must regard each other as means of capital accumulation not reliably under control. The sex which commits huge energy resources to incubating and nurturing will develop coy behaviour and adopt a sceptical stance towards errant mates. These fundamental behaviours would almost certainly be genetically mandated and constrained (Dawkins, 1976).

In advertising Sarah Blaffer Hrdy's book on langur behaviour, in which she emphasized competitive reproductive strategies of males and females, Harvard University Press referred to that kind of natural history as feminist (Ford, 1976; Hrdy, 1977). It would be hard to find a more market-limited rationale for feminist political theory. Much of the application of sociobiology to human beings centres around sexual competition (Weinrich, 1977).

But let us leave the market, despite its wealth of unexplicated topics, and look at sociobiology's theorizing of nature as a communications or control machine. Again, I focus not on the application of sociobiology to human life,

but on the fundamental concepts of the science. The genes must make stable mediating devices; that is, they must produce machines *embodying* evolutionary stable strategies, just as capital requires capitalist institutions. Without mechanisms for transmission and replication, the genes are like hoarded money. The market demands a technology of production consistent with its own imperatives. Here we leave the realm of competition and exchange and enter the factories of life. What kind of mediating machines do the genes inform? Naturally, cybernetic systems.

Sociobiology studies two fundamental sorts of systems: populations and societies. Both are studied in terms of boundaries of information and energy flow. Information and energy are different faces of a common coin, a realization made possible by thermodynamics and information sciences. Populations are measured in terms of boundaries of gene flow over time; genes are materializations of information. Sociobiology studies societies in terms of zones of communication and exchange of information (Wilson, 1971, p. 224 ff; 1975, ch. 1). Individuals are systems common to sociobiology and other areas of life science. Individuals also are studied as part of structured flows of information and energy, interacting with other individuals; higher levels of order (societies, populations) result. Individuals are intermediate structures constructed, or rather instructed, by the genes.

What the genes really make are behaving machines. Thus behaviour becomes a central concern of sociobiology. Behaviour is the evolutionary pacemaker; it determines the rate of system change by its capacity to track and respond to variables. Dawkins, in his chapter 'The Gene Machine', discusses behaviour in terms of motion timed and controlled by a biological computer whose least element is the neurone (Dawkins, 1976, pp. 49-70). Genes are like programs for chess-playing computers; that is, genes build brains, effector organs, and sensory channels. Brains are processing devices with logical programs. Terms like 'imagination' (all mentalistic language) refer to forms of simulation made possible by advanced brains. The task of brains is the prediction of interlocking system contingencies, including the environment, and control of rate of motion. The system goal is maximization of genetic profit, necessitating the structuring of specific forms of control. Speed and capacity of processing are the basic parameters of the brain as control device.

Wilson (1975, ch. 7) calls social behaviour a tracking device for changes in the environment. He elaborates the concept of multi-level, hierarchically designed tracking systems. Relating the appropriate tracking mechanisms to the appropriate time scale, he works 'down' from levels of evolutionary adaptation (including morphogenetic changes and a hierarchy of organismic 'responses', from instinct-reflex systems to generalized learning systems) to individual adaptations (including learning, socialization, and play). Nothing

is as silly as arguing about nature and nurture. The question is which level of tracking device one is considering.

The important point to keep in mind is that such phenomena as the hormonal mediation of behavior, ontogenetic development of behavior, and motivation . . . are really only sets of adaptations keyed to environmental change of different durations. They are not fundamental properties of organisms around which the species must shape its biology . . . The phenomena cannot be generally explained by searching for limiting features in the adrenal cortex, vertebrate mid-brain, or other controlling organs, for the reason that these organs have themselves evolved to serve the requirements of special multiple tracking systems possessed by particular species. (Wilson, 1975, p. 145)

So, physiology is subordinate to another level of analysis, that of operations research directed at biological tracking devices much more sensitive than radar. This approach to behaviour, adaptation, and the brain, in operations terms analogous to those studied in the Second World War, stands in sharp contrast to Yerkes' psychobiological doctrines of mind, brain, and society. Biological inventory and personnel management have been superseded. The distance is large between persons or superorganisms (the mind co-ordinates sexual instinct to produce co-operation) on the one hand, and multiple tracking systems (with mind as the strategy of genes) on the other hand.

Communications theory is closely related to the sociobiological treatment of behaviour. From operations research to information sciences is a short step. Communication is sending and receiving meaningful signals, resulting in changed probabilities of behaviour. According to Wilson (1975, p. 201) a task of his science is to construct 'zoosemiotics'; that is, the study of general properties of communication.²⁴ Basic to that task is an analysis of *modes* of communication, which necessitates attention to sensory channels, whether auditory, tactile, acoustical, or chemical.

It is therefore legitimate to analyze advantages and disadvantages of the several sensory modalities as though they were competing in an open marketplace for the privilege of carrying messages. Put another, more familiar way, we can reasonably hypothesize that species evolve toward the mix of sensory cues that maximizes either energetic or informational efficiency, or both. (Wilson, 1975, p. 231)

It is in this context that we should consider one of Wilson's most important research contributions to sociobiology: a study of insect chemical communication mediated by pheromones. Pheromones are chemical substances, usually glandular in origin. 'One individual releases the material as a signal and another responds after tasting or smelling it' (1975, p. 591). Social

insects make extensive use of this mode. In about 1958, Wilson (1962; 1971, chs 12–14) adapted a mathematical technique to measure the amount of information transmitted by the fire ant odour trails and to compare it with the amount transmitted by the waggle dance of the honey bee. The general project was the translation of behaviour of all sorts into bits which could be treated by conventional information theory relating energy, capacity, noise, ambiguity, and so on. Wilson's goal was to understand communication as part of hierarchically graded evolutionary stable strategies, differentiated by time scale and material modality, in the interest of genetic fitness or maximization of genetic profit.

Territoriality and dominance systems are modes of communication which maintain stable configurations over intermediate time spans (Wilson, 1975, chs 12–13). Aggression, a form of competition, is basically a type of communication which must be analysed in terms of functional content and energetic efficiency. In principle, if found wanting by the evolutionary engineer, aggression, like sex, is dispensable. This is very unlikely; but outmoded expressions of aggression should be expected, providing models for social and psychological therapy in human orders. Obsolescence is a central theme in the biology of automated technological devices. The contrast with Yerkes' organismic psychobiology culminating in the person is evident. For a sociobiologist, dominance is not a trait, nor even an individual organismic predisposition, but a system property. The type of engineering intervention appropriate to sociobiology is systems analysis and design, not clinical diagnosis based on an analogy to physiology and scientific medicine. But both forms of engineering argue for a special role for the scientific expert in designing history (systems) on the human level.

The point of systems design is optimization. Optimization does not mean perfection. A system has to be good enough to survive under given conditions. Nature can be lazy, and seems to have abandoned a natural theological project of adaptive perfection. Yerkes sought to find perfection in adaptivity, but not the sociobiologists. Optimization does not mean maximum productive efficiency at all times. Insects in optimized societies can be lazy as well as industrious; it has been precisely measured. Crucial to system optimization are the *mass* effects of many variables, not perfection of the individual worker ant. So, Taylorite scientific management is inappropriate as an analogue to modern scientific study of the natural economy.

In the early 1960s, Wilson drew on the systems science of ergonomics that had been developed in human sociology of capitalist production.²⁵ Ergonomics is the quantitative study of the distribution of work, performance, and efficiency; it must take account of the history of systems because that history results in limits on available materials and in other constraints. In natural systems, those constraints would likely be built into the genetic

programmes. Existing systems of production in both natural and political economy are compromises; the engineer determines the best choice of possible trajectories, with no apologies to the utopian activist. Wilson applied ergonomic analysis to the problem of number, type, and timing of production of various castes in insect societies, in order 'to analyze optimality'. Such an analysis should reveal when and how many sexually reproducing forms will be found under particular environmental conditions for a given species.

First, consider the concept of cost in colony reproduction . . . The mature colony, on reaching its predetermined size, can be expected to contain caste ratios that approximate the *optimal mix*. This mix is simply the ratio of castes that can achieve the maximum rate of production of virgin queens and males while the colony is at or near its maximum size. It is helpful to think of a colony of social insects as operating somewhat like a factory constructed inside a fortress . . . [the] colony must send foragers out to gather food while converting the secured food inside the nest into virgin queens and males as rapidly and efficiently as possible. The rate of production of the sexual forms is an important, but not an exclusive, component of colony fitness. (Wilson, 1971, p. 342)

It would be hard to find a clearer example of an analysis of biological objects in terms of the systems sciences rooted in military combat, competitive sexuality, and capitalist production. Wilson's science of sociobiology no longer sees sex in terms of the problem of personality and personnel sciences applied to family, education, and industry. Yerkes' terms of reference have no place in the new biology of optimized communications systems assessed by a design engineer. The disquieting aspect of all this is that sociobiologists can and have correctly predicted insect caste distributions with these analyses.

Wilson concluded the chapter in *Sociobiology* on origins and evolution of communications by drawing attention to the central aspect of biology as an engineering science; that is, a science that studies systems design, with an eye to human-mediated improvement of potentially outmoded natural control systems. 'If the theory of natural selection is really correct, an evolving species can be metaphorized as a communications engineer who tries to assemble as perfect a transmission device as the materials at hand permit' (1975, p. 240). Phylogenetic constraints on the evolution of natural systems could, in the human case, be studied and perhaps redesigned. There would, however, be limits to design, limits crucial from a human political perspective that denies a natural necessity for hierarchical control systems and other modes of domination, for example, socialist-feminism.

The theoretical view of nature underlying genetic engineering and bioethics as a kind of quality control industry appears clearly in sociobiology.

On Human Nature emphasizes constraints and deeply established trajectories, but there is no logical, much less moral, barrier to a full engineering approach to outmoded systems.²⁶ In that sense, the status quo rationalizations of the book, though extensive and explicitly sexist, racist, and classist, are on the surface. The foundation of sociobiology is a capitalist and

TABLE 2
LIFE SCIENCE IN AND FOR CAPITALISM AND PATRIARCHY

Biology as an Engineering Science		
<i>Machine (production)</i>	<i>Control</i>	<i>Engineering</i>
machine as organism	functionalism	adjustment, inventory, and normalization of diversity
machine as cybernetic system	communication, information	expanded integration, redesign
Key biologies: physiology, cell and developmental biology, molecular biology		
Key machine subsystems: nervous system, reproductive system (mind and sex, culture and nature, intelligence and instinct)		
Basic metaphors: balance, equilibrium, stress		
Model for breakdown: obsolescence, defect, noise or disorder		
Basic ethics: bioethics as quality control		
Basic processes permitting an engineering perspective: breakdown and assembly, re-assembly, self-assembly (e.g., viruses, membranes, visual system, organelles); regulation and control (linguistics, new logics, electronics industries and sciences providing basic biological categories)		

Biology as an Investment Science	
<i>Market (exchange)</i>	<i>Management of Portfolio</i>
Key biologies: genetics, population biology, ecology and evolution	
Strategy: individual self-interest, maximization of profit, accumulation, diversification	
Basic scandal: altruism	
Basic ethics: contract compliance and opportunism	
Basic processes permitting an investment perspective: competition and co-operation both as forms of maximization strategy, game strategies, contract and exchange at the origin of all society (key industries providing biological categories: insurance, consulting, advertising)	

patriarchal analysis of nature, which requires domination, but is very innovative about its forms. The limits to engineering redesign in sociobiology are set by the capitalist dynamic of private appropriation of value and the consequent need for a precise teleology of domination. The fundamental sexism is less in rationalization of sex roles as genetically predisposed, than in the basic engineering logic of 'human' domination of 'nature'. The humanism of sociobiology, which Wilson correctly cites in his defence, is precisely the core of his science's sexism.²⁷ In addition of course, sociobiological reasoning applied to human societies easily glides into facile naturalization of job segregation, dominance hierarchies, racial chauvinism, and the 'necessity' of domination in sexually based societies to control the nastier aspects of genetic competition. But, ironically, sociobiology is probably less tied to explicit sexism and racism than psychobiology and other organic functionalist biologies were. Sociobiology is a radical engineering science which can readily cleanse its objects of obsolescent flaws in natural design. The deities of the organic body are not sacred to the new designers of evolutionary stable strategies. It is no wonder that Wilson (1978, p. 209) ends *On Human Nature* with a rejection of Pandora and an appeal to renew worship of Prometheus, the titan who symbolizes human liberation through domination. In Greek, *Prometheus* means *forethought*, an optimal result for a communications science.

CONCLUSION: IS FEMINIST-SOCIALIST SCIENCE POSSIBLE?

Nature is, above all, profligate . . . [Its schemes] are the brainchild of a deranged manic-depressive with limitless capital. Extravagance. Nature will try anything once. That is what the form of the insect says. No form is too gruesome, no behavior too grotesque. If you're dealing with organic compounds, then let them combine. If it works, if it quickens, set it clacking in the grass; there's always room for one more; you ain't so handsome yourself. This is a spendthrift economy; though nothing is lost, all is spent.

Annie Dillard, *Pilgrim at Tinker Creek*

We have seen two varieties of biology as an engineering science in relation to the knowledge and practices of patriarchal capitalism. There has been no clear distinction between objective science and abusive ideology because the

relations of knowledge and historical determinants require more complex concepts. In an important sense, science, like capital, has been progressive. The computer is not just a machine built according to laws of domination related to labour and war. Communications sciences, including sociobiology, are human achievements in interaction with the world. But the construction of a natural economy according to capitalist relations, and its appropriation for purposes of reproducing domination, is deep. It is at the level of fundamental theory and practice, not at the level of good guys and bad guys.

A socialist-feminist science will have to be developed in the process of constructing different lives in interaction with the world. Only material struggle can end the logic of domination. Marx insisted that one must not leap too fast, or one will end in a fantastic utopia, impotent and ignorant. Abundance matters. In fact, abundance is essential to the full discovery and historical possibility of human nature. It matters whether we make ourselves in plenty or in unfulfilled need, including need for genuine knowledge and meaning. But natural history – and its offspring, the biological sciences – has been a discipline based on scarcity. Nature, including human nature, has been theorized and constructed on the basis of scarcity and competition. Moreover, our nature has been theorized and developed through the construction of life science in and for capitalism and patriarchy. That is part of the maintenance of scarcity in the specific form of appropriation of abundance for private and not common good. It is also part of the maintenance of domination in the form of escalating logics and technologies of command-control systems fundamental to patriarchy. To the extent that these practices inform our theorizing of nature, we are still ignorant and *must* engage in the practice of science. It is a matter for struggle. I do not know what life science would be like if the historical structure of our lives minimized domination. I do know that the history of biology convinces me that basic knowledge would reflect and reproduce the new world, just as it has participated in maintaining an old one.

Part Two

Contested Readings: Narrative Natures