A SOCIOLOGICAL STUDY OF FEMINIST APPROACHES TO BIOLOGY

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ABSTRACT

This research aims to evaluate the attempts of Anglo-Saxon feminists to elaborate a new practice for the natural sciences. It focuses on biology, a discipline which extends beyond the realm of social science, and on which basis feminist critics have undertaken to reform the norms of scientific practice and to recast scientific epistemology. The central question of this research is: Is a feminist science of biology possible, both epistemologically and as a social practice? If so, what would it be like; and under what kind of practical conditions?

The arguments of this thesis are developed in three steps. The first part consists of an analytical assessment of the strengths and weaknesses of the feminist critiques and suggestions to reform the scientific norms of biological research, including what many of them see as 'context-bound scientific canons' such as 'objectivity'. These criticisms thus range from theory choice to the very epistemological foundations of biology which are all conceived of as contributing to the development of spurious explanations of women's biology and behaviours. This first part more specifically highlights the paradox entailed by the need for feminists to justify their new epistemology on the basis of frameworks borrowed from a sociology of knowledge that emphasizes a (de)constructionist or a relativist stance on the legitimation of scientific knowledge, while, at the same time, they are forced to argue that feminist biology is 'better' and 'truer' from all points of view. I suggest that feminist science could be vindicated without resorting to the constraints of a local, context-bound stance on rationality and epistemology which appears hardly congenial to the resolution of the current scientific divergences between feminist and 'mainstream' biologists.

The second part investigates the contribution of sociologists of knowledge and philosophers, focusing more specifically on Habermas, Hesse, and Gellner. It aims at shedding light on the particularities of both the ontologies and social norms and values that

distinguish the epistemologies of the social and natural sciences. It is believed that these aspects need to be discussed more fully in order to elicit the models of explanations used in biology and the criteria of validation that feminists could not dispense with in their projects of implementing the practice and knowledge produced by feminist biologists. Then, upon an examination of the epistemological issues raised in biology per se and of the scientific critique advanced by radical scientists and 'dialectical biologists', it is suggested that it is mainly because of the hybrid cognitive nature of the life sciences that a chasm between feminist discourse about science and feminist practice of biology emerged. In other words, the strong reliance of biological mode of enquiry on both the values and ontology predicating critical theory and interpretive studies, and the more pragmatic values (and methodological commitments) rooted in the instrumental/empirical sciences, might explain why discrepancies progressively emerged between the theoretical elaboration of a feminist science (mainly inspired by reflections about the social sciences) and the actual practical implementations carried out by feminist biologists. A comparison between those two levels of feminist science (i.e. discourse and practice) will enable us to test this hypothesis.

The third part of the thesis analyses interviews of mainstream biologists and two case studies of practising feminist biologists. First, it shows the points of convergence and rupture between the norms of practice in conventional biology and in feminist biology. Secondly, it highlights the originality of the actual contribution that feminists have made in the domain of biology both sociologically and scientifically (i.e. epistemologically, methodologically, conceptually). Thirdly, it discerns the gaps and continuity between feminist theory and feminist practice of science. It also suggests, however, that the resistance of mainstream biologists to the feminist critiques and concrete projects of biology in the past decade remain partly political (i.e. hostility to feminism) and normative (i.e. according to institutionally acceptable scientific rules). For, while the idea of a feminist biology derives fruitfully some original conceptual tools and designs of enquiry from the social sciences (especially in the areas human biology and clinically-oriented research), one can as yet recognize that the epistemological conditions and methodological norms of production biological knowledge nevertheless constitute the shared framework of both feminist and mainstream researchers in most areas of biology. Hence, the shift of recent feminist critics of science (such as Harding and Longino), from an epistemologically-oriented critique of scientific knowledge to a critique in terms of theory building and ideological assumptions, may appear as more fruitful in the institutional legitimation and advent of concrete projects of feminist biology.

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INTRODUCTION

Statement of the Problem and Research Goals

This research endeavours to evaluate the attempts of Anglo-Saxon feminists to enunciate a new scientific practice for the natural sciences. It focuses on biology, a discipline which extends beyond -- so it seems -- the realm of the social sciences. It is from biology that feminist critics have set out to develop a new approach to scientific research, comprising methodological and theoretical changes in scientific research conventions, and not least, a reformulation of scientific epistemology itself. Complementing, or perhaps preceding these changes, feminists also aim to reform the gendered division of scientific labour and the organization of scientific activity. Therefore, it must be stressed from the outset that any evaluation of both the feminist critiques of biology proposed in theory by social scientists and other critics, and the actual attempts of practising biologists to implement concrete projects of 'feminist biology' must appeal to several sociological arguments rather than to epistemological or scientific analyses alone[1].

As this thesis will demonstrate, there are diverse critical tendencies in the feminist challenge of science but all of them ultimately call into question the ideological and political structure of the whole social system from which science is derived and acquires its credibility. It must be borne in mind also that in the feminist critiques of scientific knowledge more generally, the target has usually been the macro-structure of science, such as its 'patriarchal ideology', its 'androcentric culture' and the 'rational ethos' of scientists, rather than the micro-aspects of decision-making in the laboratory as such.

Feminists have built their main critiques of biology on the basis of a sociology of scientific knowledge. The standpoint of a sociology of knowledge assumes that both the epistemological tenets of the scientific enterprise and the specific assumptions underlying scientific theories are socially embedded. For feminists therefore, epistemological canons and scientific theories are in fact the reflections of social norms (or ideologies) which are not universally true but rather bound to change.

For feminists, the canons and theories of the natural sciences mirror patriarchal interests and androcentric world-views. In biology especially, theoretical assumptions reflect the patterns of social relations characteristic of the patriarchal, male-centred system of social life. For feminists it is first and foremost -- if not exclusively -- biological theories about women that are male-centred (and biased). The dichotomy between culture and nature in the explanation of sexual dimorphism and the 'reduction' of women's behaviours to biological causes are paramount examples of concepts and social norms that impregnate biological theories with male biases. Once publicized more widely outside the scientific circles, these theories maintain, via their ideological authority, the oppression of women within the family, the health care system, the labour market and back again into the scientific organization itself. Moreover, even the norms of method according to which those theories are tested and believed to be true and objective reflect ideological and political biases. Hence, it is spurious to think that theory and methodology are universal and bias-free. Rather, they constitute 'context-bound' norms, derived from the social system dominated by men. In brief, the central argument of feminists is that patriarchal and androcentric beliefs and representations of nature loom large in both the production and legitimation of biological theories about women and about the differences between the sexes.

A crucial question feminist philosophers and critics of science have therefore started addressing is: why is it that, in spite of their questionable universality and their adverse impact on the ways society treats women, these theories still remain largely uncriticized? Also, on what grounds are we to justify feminist biological theories as 'better' or 'truer' alternatives to 'patriarchal' and 'male-biased' theories, if we assume that knowledge is 'ideological' and context-bound?

A great number of feminists have argued that should changes in social norms and ideology congenial to feminist movements arise, then the emancipation of women and their full participation in decision-making ought to occur. This would in turn favour, as part of wide-ranging societal transformations, a new kind of science based on feminist thought and women's social practice and values including the 'normalisation' of women's experiences as questions relevant to a new research agenda. In such a 'world', theories produced by feminist scientists would ultimately be accepted as legitimate and the hitherto 'unorthodox' methods underlying them would be deemed epistemologically acceptable.

A major problem with the standpoint of these feminists however, is that it treats current natural science as totally patriarchal and hence, that it must be totally eradicated. Another problem concerns the adequacy of 'feminine values' to replace scientific cum patriarchal values in the production of 'better' and 'truer' knowledge. Feminist authors treat 'feminine values' (or 'feminine practice') as a vague concept encompassing diverse forms of 'personal inclinations' and aspects of social practice. I would suggest that 'feminine values' need to be differentiated on sociological, political, and epistemological grounds if they are to be of any use in the analysis of scientific knowledge production and more importantly, in the implementation of concrete projects of feminist biology. A closer investigation of the socio-political and epistemological aspects of the production of biological knowledge is therefore needed in order to illuminate the strengths and limitations of feminist theories of science as a platform for the implementation of concrete and viable projects of feminist biology.

It is my first contention that a more 'detached' examination of epistemology and the process of theory-building in biological science would help to spell out more adequately which aspects of 'traditional' biology are worth retaining in the actual projects of feminist biology. This should be done co-jointly with an examination of the socio-political factors favouring/hindering the institutional legitimacy of projects being advanced by feminist biologists. Secondly I shall argue that, from the point of view of cognition, aspects relative to the political awareness of scientists, their 'common-sense' experiences, their social interests, their 'emotional inclinations' and lastly, their professional power of negotiation are relevant to scientific production but only in some areas of biological research and subjects of study and not necessarily in others. Nevertheless, many feminist theoreticians tend to apply their 'socio-political' analysis of science and the thesis of a 'feminine' practice of biology to a cognitive process that I consider far more complex and far less 'common-sensical' than they assume it to be. These are the conclusions that I shall draw from a review of the feminist literature on biology and science in Part I of the thesis. These conclusions will be buttressed in Part II by an epistemological analysis, and in Part III by our empirical fieldwork.

One should want to stress at this point that the collective role of women in the constitution of a feminist science remains empirically and theoretically problematic. Women do not form a homogeneous group in terms of social interests or political militancy in spite of sharing substantially the living experiences of a dominated group within patriarchy (Harding 1986a; Halberg 1989). Even though feminist theories of science generally consider women as key agents in the transformation of scientific practice, the empirical evidence mustered in this connection is tenuous. The 'feminine' values that could infiltrate science and might transform it are manifold, and the increasing number of women in science is, in the same vein, likely to converted in different ways. One of the ways the feminine values have actually been converted is contrary to feminist goals. Because women are usually 'clean' and meticulous manual workers, several have been assigned -- or have even preferred -- to do the 'technical' rather than the 'intellectual' work in the laboratory, instead of 'emancipating' themselves from the status quo in the gender segregation of laboratory work.

In this thesis we shall deal with the question of 'feminine values' and the impact of women's presence in biology as a problematic aspect of the notion of feminist biology. But the central issue this thesis will address concerns the problems that feminists have had in synthesizing their criticisms of science on the one hand, and the practical conditions of implementation of projects of 'feminist biology' on the other. Feminist theoreticians have hitherto devised rather unsatisfactory models of feminist science and have not reached a consensus on the aim and scope of their critiques (see Harding 1986b, 1989; Hawkesworth 1989; Schiebinger 1987). Feminist philosophers, social scientists and biologists who have contributed to a critique of biology as yet do not agree if their criticisms are merely political and organizational (as in the works of a majority of historians and science educationalists), or if they announce a new theoretical and methodological approach which might involve a re-evaluation of the epistemological foundations of natural science proper. The examination of two of the few existing cases of feminist biological practice in Part III of this thesis should help to shed light on this question and assess the 'potential for action' of theories of science advanced by feminist theoreticians.

Feminist approaches to scientific knowledge and the issue of 'feminine values'

There are various tendencies in feminist studies of science covering historiographies of women in science and education, political analyses of the scientific structure with respect to gender, and critiques of scientific epistemology.

Regarding epistemology, Sandra Harding distinguishes three avenues that feminists may consider as forming a feminist sociology of scientific knowledge: feminist empiricism, the feminist standpoint and feminist postmodernism (1983, 1986b). She claims that the first two constitute concrete research practices currently adopted by feminists. Both challenge the conventional nature of scientific 'objectivity' by voicing their feminist orientation, but retain the powerful insight of what may be called a 'critical realism grounded in social experiences' (1989, 1990). Postmodernism however, poses problems. While in the other two feminist epistemologies, the standards of objectivity and the realist stance on knowledge are preserved, in postmodernism, in contrast, relativism of bodies of knowledge is invoked, and the absence of any 'Archimedean' point of view precludes the possibility of validating any beliefs at the expense of others. The cognitive aim of postmodernism is critical and self-reflexive rather than instrumental. According to Harding, postmodernism is essential because it helps to deter the fetichism of formal categories of epistemology and methodology which are likely to obstruct the use of appropriate tools and relevant evidence for certain research problems as they did in the early struggles for the development of women's studies, and stifle a more comprehensive understanding of social reality (Harding 1987a). Also, postmodernism may be seen as a 'safety net' for feminist research, lest feminists themselves unwarrantedly reproduce classist, racist, and sexist theorizing (Harding 1986a, 1987a).

Harding's point of view was appropriated by Hawkesworth (1989) but she stressed that she did not approve of a total dismissal of rationality and critical realism. She argues that what she calls the 'feminist critical epistemology', must preserve at least some 'quasi-universal' standards of inquiry and validation, as these standards must be invoked if feminists "are to make a successful assault upon erroneous misogynist claims" (Hawkesworth 1990, 423).

Hawkesworth also maintains that psychological or 'functional' analyses (i.e. those which claim the potential of 'feminine values' to replace 'masculine' scientific norms) cannot be substituted for epistemological discussions in feminism. In discussing epistemology, one ought to focus on the 'known' rather than the 'knower', and on the nature of knowledge and validity claims rather than human (read: male versus female) motivations. Having said that, she does not dispute the utility of conducting investigations into 'motivations', for these may at least reveal some of the 'sources of errors' that affect our conceptions and perceptions of the world (1990). For this reason, Hawkesworth indicates that 'feminine values' can contribute to change theory-building in science but cannot be substituted for standards of objectivity, logical inference and rational argumentation.

Schiebinger (1987) and H. Rose (1983, 1986, 1987) among others, grapple with the question of how to articulate the feminine values (i.e. the attitudes towards people and nature and the predispositions ascribed to women as a social group) and feminist action (i.e. the militancy and concrete courses of action of feminists).

Upon a re-reading of the role of women in the history of science, Schiebinger sees the 'feminine' and the 'feminist' articulated in different ways throughout history. She is reluctant to associate the two in any rigid way. She prefers to see their interplay as grounded in practice and to investigate it by means of individual historical cases rather than invoke a general theory to explain it. On the other hand, Rose definitely claims the prime importance of feminist thought and action in the elaboration of a feminist, 'women-centred' biology. She does not dispute the import of feminine values in the development of a new practice for the natural sciences but she gives a crucial importance to the role of catalyst played by the feminist 'vanguard' of the women's movement.

Ultimately however, the articulation of feminine values and feminist programmes of action underlies theoretical flaws similar to those involved in the development of a Marxist praxis of the working class. Feminist theoreticians tend to respond dogmatically to 'incoherences' in the traditional practices of women scientists rather than recast their theory of scientific knowledge on different epistemological grounds. It is hoped that the analytical examination of Parts I and II of this thesis and the empirical fieldwork of Part III will shed some light on the problems incurred by feminist theory in relation to the issues exposed above.

Although there are very few feminists who pursue biological research proper (all of whom are women), it is fundamental to assess their actual scientific production at this stage of the development of feminist thought about science. Moreover, since feminist thought is by definition oriented towards action, it is important to evaluate how feminist theory and action relate to each other in terms of both political struggles and cognitive conditions of scientific knowledge in the natural sciences.

Plan and Method of Research

This thesis will argue that the feminist criticisms of biology concerning both the scientific and the organizational aspects of biological science deserve credit, however they fall short of mapping out a fully-fledged and viable project of feminist biological practice. Feminist theory underestimates the epistemological bases that current 'patriarchal' biology provides for the legitimacy of concrete projects of feminist biology.

It seems particularly worthwhile at this point to define what is meant by 'epistemology' in order to give an idea of the type of discussion we shall address in this thesis. Epistemology is defined as a general theory of knowledge which predicates the criteria of valid knowledge and the (methodological) procedures which help to distinguish why some scientific theories are 'truer' than others, or between valid and non-valid theories (Dancy 1985; Gellner 1959, 1964, 1974, 1982; Habermas 1974, 1976, 1979, 1984; Hesse 1974, 1980a, 1980b; Lakatos 1970; Popper 1959, 1969, 1972; Toulmin 1972). With respect to natural science, the epistemological issue of validity (and 'truth') is closely connected to the ontological issue of the character of nature (or 'reality') and the practical conditions enabling humans to acquire knowledge of it.

In modern epistemological debates, the main question is not so much to do with rationalism versus empiricism but more to do with the scope of, and interplay between, social influences and causal determinism of reality on our representations and interpretations of 'facts'. In this context the notion of epistemological canons refers to those 'hard core' tenets (or conditions of possibility of knowledge) that are not local or ascribed to any social agents in particular but represent 'quasi-universal' constraints on validity-claims in knowledge, the 'mega-level' of social and empirical constraints on human knowledge.

This thesis does not dispute the idea that epistemology is partly rooted in the history of human thought and socially entrenched thereof. The idea implied is that 'epistemological canons' are not always locally-specific norms of production and validation of knowledge. These 'canons' or 'mega-norms' seem to transcend any social ascription, and the historiography of modern natural science makes us argue for the existence of such quasi-universal 'norms of epistemology'. This view borrows from the arguments of Hesse about 'scientific logical inferences', the 'network model of theories', and the 'pragmatic criterion' (1974, 1980a), and those of Gellner about 'cognitive selectors', empiricism, and experiences of the senses that we shall discuss more thoroughly in chapter 3.

With respect to biology, epistemology indicates how 'theory-building' ought to be ideally conceptualized, and which methods and techniques of enquiry ideally ought to be used in the 'discovery' and 'validation' of knowledge about living organisms, human beings and 'lower forms' of life.

Therefore the first objective of this research is to highlight the limitations (and contradictions) of feminist theoretical and critical writings. Although some of the sociological arguments of feminists concerning the domination of men and patriarchs over biology are justified, a total neglect of issues more specific to the epistemology of the empirico-analytical sciences and of biology is misguided. The working out of a viable theory of scientific knowledge and defence of a 'feminist method' applicable to the natural sciences as a whole, and particularly to biology must confront these issues more systematically. For these reasons, the projects of feminist biology devised by feminist theorists do not appear very convincing and there remains serious doubts about the possibility of implementing them. Also, by being overly provocative and critical, feminist theoretical discourse has tended to arouse resistance on the part of mainstream biologists and as a consequence, has undercut the scientific credibility that certain feminist biological theories would have received otherwise.

However, this thesis does not imply that all the feminist biological theories and explanations about women's biology are not scientifically and epistemologically sound. Indeed several new theories, concepts and explanations developed by feminist biologists are grounded in the epistemological canons governing the justification of scientific results in the empirico-analytical sciences, such as the canons of critical realism and the criteria of empirical falsificationism. As such, the historiographical evidence reviewed and the empirical material mustered in this research show that some feminist biological theories may well be vindicated within the very confines of 'mainstream biology'. Indeed, the beliefs of practising feminist biologists in various degrees of critical realism, and their apparent adherence to the Popperian empiricist falsificationist theory of scientific knowledge constitute important anchors of legitimacy for their undertakings. Feminist biologists do not seem to avoid these 'ultimate rules' of scientific practice but rather stand by them, disregarding feminist critics and theoreticians.

I therefore believe that an empirical study of the practice and works of feminist biologists would help to clarify the scope and 'true nature' of feminists' contribution to biological science. Only in this way can we appraise satisfactorily the weight of political and organizational reasons (e.g. political hostility, institutional orthodoxy) on the one hand, and of scientific norms of practice on the other, ruling against the idea of a feminist biology. In brief we shall look for explanations of why feminist biology has so far had an extremely limited response, both politically and scientifically, from within the community of biologists. It is hoped that the field studies will also help to shed light on the issue of 'feminine values' and women's participation in the development of a feminist biology, by qualifying the weight they should be accorded within a sociological theory of scientific knowledge. This will be done by exploring whether the gender variable affects the discourse of biologists on matters related to feminism in biology.

As mentioned earlier, the present project will be carried out in three steps. First, there will be an analytical assessment of the strong and weak points of the feminist critiques and attempts to reformulate scientific epistemology as a basis for new, less male-biased norms of scientific practice in biology. Chapter 1 deals with the particular criticisms of biology which feminists have advanced and chapter 2 examines more generally their theories of the structure and aims of the scientific institution, the scientific method and its epistemological 'foundations', and their suggestions for the construction of a new 'feminist' science practice.

This critical review will be followed, in Part II of the thesis, by an exploration of the conditions of possibility of the project(s) of feminist biology as enunciated in the feminist literature surveyed in Part I. Chapter 3 includes a discussion of the differences in the epistemologies of the human (and social), and natural sciences emphasizing the specificity, methodological and theoretical, of biology. Chapter 4 is an overview of the scientific debates and social controversies which biological research has produced. It examines, as a case in point, the scientific project of Radical Scientists and the Dialectics of Biology Group. On the basis of this discussion, this thesis argues that feminists must first define more strictly the scope of their critiques. Secondly, they must make a stand on the legitimacy of the new body of knowledge they produced, in accordance with their aims of denouncing 'patriarchal biology' on the one hand, and of justifying 'feminist biology' as being a 'better', 'truer' form of knowledge on the other.

Part III of the thesis investigates the oral and written material documenting the points of convergence and divergence between feminist theory, mainstream discourses about biology and feminist practice of biology. In chapter 6, an analysis of interviews with practising British male and female biologists will be presented and contrasted with the critiques of feminists theoreticians. We shall survey mainly the following themes: opinions about organizational issues of research work, especially in light of the integration of women; the scientific relevance of the debate between holism and reductionism in biology and of the feminist critiques of reductionist theories of womens' biology, behaviour and 'disorders'; the potential role of women as a group in biological practice; and the impact of feminism on biological research. As noted before, the last two issues are problematic in a sociological theory of scientific knowledge, and we must therefore aim at documenting them in the present research.

Two case studies of feminist biologists were conducted in order to highlight further the clashes between the theory and the practice of feminist biology. The first case study, presented in chapter 7, is about the British zoologist Lynda Birke who is unique among British feminists in that she is actually engaged in biological research and makes a strong case for the idea of feminist biology. The second case study, detailed in chapter 8, is on the Canadian-based Groupe de Recherche-Action en Biologie du Travail or GRABIT (Group for Research-Action in Biology and Work). GRABIT represents, from an institutional perspective, the most developed form of feminist biological practice, which makes it a highly relevant case history. These interviews and case studies will aid in comparing the discourse and practice of mainstream biologists and feminist biologists. It is hoped that the results will provide elements for a much needed substantive appraisal of the contribution of feminism to biology.

Sociology of Knowledge, Epistemology, and Feminist Biology: Theoretical Preliminaries

One could rightly argue that the sociology of knowledge has, over the past thirty years, narrowed the boundaries separating the diverse areas of study it has traditionally dealt with (Barnes 1974; Gurvitch 1966; Merton 1959; Wolff 1957; Abercrombie 1980; Law 1986; Schutz 1962). These include the studies of religion, common sense and popular beliefs, political ideologies and scientific knowledge. On the other hand, one may contend that the sociology of scientific knowledge has not become an integrated area of study as such, for it may be divided into several branches of study depending on what sociologists focus their attention on.

For instance, one may decide to focus on the importance of the dominant ideology, culture of the time or scientific heritage on the acceptance of new scientific knowledge. This type of approach is typical of the field of the history of science from which sociologists borrow heavily and to which, in turn, they contribute theoretically (Mendelsohn 1977).

In contrast, one may decide to question the epistemological canons of science separately from the sociological factors. This type of investigation has more to do with the philosophy of science and what has come to be 'the internal approach' to scientific knowledge -- as opposed to the 'external approach' stressing the social embeddedness of all knowledge (Knorr-Cetina & Mulkay 1983; Hull 1988).

Since the early sixties, there has been a great deal of effort to try to link the epistemological questions with the concerns of the sociology of scientific knowledge. These attempts aimed to 'salvage' the ideals of rationality and 'true' knowledge. The works of Habermas emerge as an attempt to remove the cognitive scepticism inherited from the Frankfurt School (Habermas 1970, 1974, 1976). Those of Popper (Popper 1957, 1959, 1969, 1972) can be seen as an attempt to overturn the epistemological vacuum left by the failure of Logical Positivism to ground 'true' knowledge in universal, hard core foundations. Although these philosophers represent two different traditions, one more rationalist, the other more empiricist, both epitomize the refusal to give in to the scepticism voiced in the epistemological perspectives of cognitive relativism, Wittgenstein's philosophy and the phenomenologists, and postmodernism.

One must acknowledge that the difficulties of surmounting the problem of cognitive relativism has unsurprisingly given more clout to a 'strong programme' in the sociology of science (Barnes 1974; Barnes and Bloor 1982; Bloor 1976). The 'strong programme' has inspired several 'contextual' studies of scientific practice at a micro-level (Collins 1985; Knorr-Cetina and Mulkay 1983; Law 1986). In these studies, the construction of scientific facts guides the research designs, and the 'traditional' concerns of epistemology are considered irrelevant. These studies illustrate that the negotiations over the meanings and accuracy of experimental or observational evidence are in fact the core of scientific practice, and that what scientists believe to be the search for 'independent' empirical evidence of theories is simply not what they suppose it to be.

The 'strong programme' also seems to have spurred an increased number of ethnomethodology-inspired analyses of laboratory work (mainly oriented towards conversational analysis) (Latour & Woolgar 1979; Lynch 1982, 1988; Woolgar 1982). This new tradition in the sociology of science has produced many well-documented studies, very rich in empirical content, and dealing with the observable aspects of science at a micro-level. It posits that science ought to be observed via the ways scientists work, communicate and abide by the 'norms' of production in the scientific institution, and that sociologists should not take for granted what scientists perceive as being the rules they pretend to be complying with.

There has rarely been an attempt to relate these micro-features of scientific practice to the cultural and historical context in which specific scientific activities evolve and 'survive' (Harré 1983; Chalmers 1988; Knorr-Cetina & Cicourel 1981). In such studies the impact of political and institutional negotiations on the production of scientific knowledge seems to be overstated at the expense of an examination of the epistemological conditions which might also have contributed to the success and 'progress' of modern science as a form of knowledge (Hollis and Lukes 1982; Chalmers 1988; Manicas and Rosenberg 1988; Hesse 1980a, 1980b; Sayers 1985; Hull 1989), or a 'moral order' as Harré (1983, 1986) would advance.

Likewise, science has been studied as 'a system of organized knowledge' or 'Big Science', operating and legitimating itself in close relationship with other systems like industry, the welfare state, defence and warfare, medicalization (Bernal 1939; Habermas 1970, 1978; Rose and Rose 1969, 1976a; Sklair 1973). I believe that an understanding of the reasons why it has gained so much stature as a form of knowledge might gain from analyses focusing on other levels of the scientific practice. Some 'hard-core', 'mega-level' criteria relating to epistemology, or even to a 'moral order of rational conventions' seem to have played a role in the production and legitimation of scientific knowledge which strictly local, socially specific and sociologically identifiable variables do not explain satisfactorily (Habermas 1974, 1976, 1984; Hesse 1974, 1980a; Gellner 1974, 1982; Harré 1983, 1986; Hawkesworth 1989).

Over the past thirty years, the sociology of scientific knowledge has been largely influenced by the Kuhnian outlook and the notion that scientific statements and inferences are empirically underdetermined. That is to say, the evidence provided to prove a scientific hypothesis never really covers all the possible evidence, for the main reason that theoretical choices always limit the pool of observable facts considered relevant for the explanation of the problems at hand. According to Kuhn, science always builds on a system of techniques, concepts and instruments, within which a finite series of experiments is accounted for, thus providing an artificially constructed terrain of empirical, observable and testable evidence (Kuhn 1970a, 1970b). But there is a caveat. Firstly, Kuhn's fails to get rid of the idea of 'progress' within a paradigm, and second, to theorize the actual translations -- rather than incommensurability -- between paradigms.

Decades later, it is not unreasonable to say that the works in the sociology of scientific knowledge still reflect the difficulties in coming to terms with the intellectual heritage of contemporary, post-positivism epistemology. But I would contend that, in spite of the ineluctability of the social and historical embeddedness of all forms of knowledge, one does not necessarily have to succumb to the ideas of Kuhn's 'incommensurability' (see Hesse 1974; 1980a; Bernstein 1983), cognitive relativism (see Hollis and Lukes 1982; S. Sayers 1985), or Feyerabend's anarchistic perspective (Lakatos 1970; Harré 1983). This research partakes to such a tradition.

On the basis of a sociology of scientific knowledge inspired by Kuhn, the Marxist theory of ideology (itself elaborated by the Frankfurt School), the relativist or context-bound analyses of knowledge, and postmodernism, feminist critics of science have endeavoured to eradicate the androcentric and patriarchal assumptions and inferences entering the scientific process of theory-building. Feminists have assumed that certain inferences developed about the causes of womens' biology, behaviour and social achievements are considered valid only because they stem from facts which are value-laden in favour of males (i.e. androcentric values) and patriarchy (i.e. the values supporting the systematic domination of males over females in society) (Fisher 1980; Hubbard 1979; Merchant 1982; Harding and Hintikka 1983; Fee 1983; Keller 1985; Rose 1983, 1985). They argue that the validity of these inferences underwrites the constraints of a patriarchal system where empirical counter-evidence can hardly exist and, consequently, alternative theories cannot readily be supported. Several feminists (Fee 1983, 1985; Birke 1986; Brighton Women and Science Group 1980) have even advanced that a full recognition of the feminist criticisms might be impossible in the present patriarchal context.

It is my contention that the majority of feminists have been mistaken in rejecting more traditional canons of objectivity and rationality in the design of a new science. This problem was foreseen by many feminist theoreticians (Keller 1982, 1987b; Longino and Doell 1983; Fee 1985; Rosser 1985) and is now central in their discussions about epistemology and scientific knowledge (Longino 1987, 1989; Harding 1986a, 1987a, 1989; Saarinen 1988; Alcoff 1987; Hawkesworth 1989). Certainly, the adoption of a postmodern, 'deconstructionist' outlook on science, inspired by the archaeology of knowledge of Foucault (1970, 1972, 1980) whereby systems of knowledge are conceived of as 'regimes of truth' or political discourses, was favourable to the goal of unveiling dominant, patriarchal ideologies pervading scientific theories about women. However, deconstructionism hardly provides a conception of validity coherent with the avowed claims of feminists who regard patriarchal biology as flawed from an 'Archimedean' point of view. It is no surprise that several feminists are now struggling with the problems of validity and truth-claims in science and the justification of their critiques of biological theories and projects of feminist biology (Longino 1987, 1989; Rosser 1987, 1988b; Harding 1987a, 1987b, 1989; Mura 1989; Birke 1986; Birke and Vines 1987).

In brief, feminists may have dismissed analyses of rationality too hastily, for the problem of relativism now lies bare in their hands and this undercuts their claim that 'feminist biology' is 'better', 'truer' and 'closer to reality' than traditional biology.

One of the arguments of this thesis points to the relative absence of the works of Habermas and Hesse in the writings of feminist theories of scientific knowledge. These two authors have developed insights into relativism and rationality which represent an improvement on the positions of the Frankfurt School, Kuhn, Foucault, and the 'Strong Programme'. They have made seminal clarifications between the epistemologies of the natural and the social sciences and given more clout to the notion of rationality and continuity in the history of modern science, while not disputing the social and historical embeddedness of human knowledge. They also have indirectly helped to illuminate the specific problems of biological knowledge which feminist theoreticians have started addressing but as yet, seem to have left largely unresolved. In this last analysis, it seems unfortunate from a theoretical point of view, although not very surprising from a political or 'dogmatic' perspective, that Anglo-Saxon feminists have systematically overlooked such 'established', yet prescient works.

For example, feminist theorists rarely distinguish among diverse scientific subject matters and between the epistemological conditions lending themselves to the various methods and models of explanation specific of the natural sciences, and those of the human (and social) sciences. An examination of the feminist critiques of biological knowledge shows that hermeneutic questions (dealing with human subjects and patterns of behaviour) and the pragmatic or instrumental criterion of the empirico-analytical sciences (dealing with non-intentional objects and organisms such as levels of hormones or physiological make-ups) always are at the heart of these critical reflections. In this light, it is not surprising that the main target of feminist critics of the natural sciences has been biology, for it lies at the juncture of the living and the non-living, the human and the non-human, the social and the physical sciences. I should suggest that the critiques of feminists with regard to natural science in general, and biology in particular, only partly justify their attempts to build a 'new' feminist biology on grounds provided by a discussion of a sociology of the social sciences. One must locate the biological questions addressed by feminists within a larger epistemological debate in order to appreciate the real scope of projects of feminist biology. Only then should one contemplate the originality of feminists' contribution to the development of biological science, and assess the weight of the 'extra scientific' opposition that feminists have activated from 'mainstream biologists'.

The feminist critiques of science and projects of biology may well be taken as examples for a sociology of scientific systems, thereby shedding some light on the political and institutional reasons why feminists have had a rather limited impact on the biological sciences compared with a more significant one in the social sciences. But I contend that important epistemological problems also prevail in relation to the feminist positions on biology and empirical science more generally. Hence, there may well be important 'scientific' reasons why biologists do not accept the feminist critiques as valid, or the feminist projects of biology as possible. This is why this thesis examines the types of scientific problems addressed by both mainstream and feminist biologists. By comparing them we shall identify the areas where feminist approaches to biological practice both diverge from and converge with mainstream biology, and the areas where there seems to be predominantly political and institutional constraints hindering a dialogue and fruitful exchange.

In short, it is necessary that the strengths and weaknesses of discourses about 'feminist biology' be evaluated from a sociological viewpoint <u>and</u> upon an epistemological investigation of the current practical problems and controversies involved in the practice of empirical science and hermeneutic studies, for there lies the core of the problematic question of valid knowledge in human biology -- and feminist biology more particularly. I contend that the strength of 'feminist biology' should be sought in the actual empirical research of feminist biologists rather than in the theoretical works of feminist critics. As the results of the empirical studies conducted in this research will suggest, the strength of feminist biology is rooted in the defence of specific women-centred research interests (and anti-patriarchal ideology) in the areas of bio-behavioural and health studies. It does not reside in the creation of a new epistemology, but in the constitution of new conceptual and, to a lesser extent, methodological tenets for biology. Having said that, feminists biologists have in their actual practice struggled courageously to defend the use of the interview techniques and interpretive methods in biological and clinical research. They also have integrated conceptual categories relating specifically to female biology and the social conditions of women into models of explanation of women's health and women's biology. Inspired by feminist sociological theories and employing methods of investigation developed in the social sciences, feminist biologists have presented new biological findings applicable not only to women but also to men. This will be made clearer in the case studies of the British biologist Lynda Birke and of the Canadian-based 'Research-Action Group in Biology and Work'.

To summarize, this thesis argues that the feminist criticisms of biology have been central in the constitution of new research designs and a specifically feminist research agenda in biology. Feminist biologists have come to ask original research questions, by focusing on certain techniques of investigation and analytical concepts which have been omitted in the development of biology as a 'hard science'. As a result they have provided new types of data on, and biological explanations of, human behaviours, 'disorders' and ailments. They have not totally restated the criteria of validation of biological knowledge, but only stressed the genuine role of human subjectivities (of women's in particular) as part of the evidence relevant to the process of enquiry in human biology. Hence they only have had a minor or residual role in the implantation of a non-positivist, socially reflexive, epistemology for the natural sciences.

Endnotes

1) I deliberately excluded from my study the issues of bio-medical technologies for two main reasons. First, I believed that the problems of 'in vitro' fertilization and new reproductive technologies are linked primarily to the medical profession and its social power and less so to biology. Second, although the political aspects in NTR are very relevant from the point of view of sociology, this should not lead us to neglect the 'cognitive' aspects of biology from the point of view of a sociology of knowledge. Hence, I preferred to concentrate on biological issues where politics is not immediately problematic in accordance to my aim of testing the feminist theories of knowledge in the natural sciences.

CHAPTER 1

THE BIOLOGICAL THEORIES ADDRESSED BY FEMINISTS: ON THE BIOLOGICAL ROOTS OF HUMAN ACTION

The common thread amongst feminist criticisms of biology is the attempt to show how biology has mis-represented women and to correct these representations in favour of a more accurate understanding of woman's biology, social behaviour and social opportunities with a view to women's emancipation from patriarchal domination.

The evolution of sex differentiation has always been a question of importance in the life sciences. As a consequence, the biological theories concerned with the problem of human action and evolution have been the main targets of feminists. More importantly, since science is extremely influential as an ideology, feminists have decided to address biological theories more particularly because these theories are fertile grounds for a scientific sanctioning of the idea of woman as the 'second sex' or as the 'feeble sex', and therefore for reinforcing the patriarchal order.

The feminist criticisms of biological theories can be traced back to the turn of the century in the fields of social medicine and psychology (Rosenberg 1982; Sayers 1982; Harrison 1981)[1]. In the nineteen-thirties and nineteen-forties, studies in anthropology gave the feminist criticisms a second wind by challenging patriarchal ideas about the 'female nature' which had dominated that field. The empirical works of Mead and Benedict (in Rosenberg 1982) and those more theoretical works of de Beauvoir (Beauvoir 1949) are paramount in this respect. The feminist criticisms of science were given their impetus via feminist anthropology in the 1970s. At that time feminist scholars systematically engaged in debunking the 'male dominance' and other 'male-centred' theories of the social organization of primates and hominids, and those about the evolution of the human race (Fisher 1980; Haraway 1978; Hrdy 1981; Hubbard 1979; Leibowitz 1975; Reiter 1975;

Slocum 1975; Tanner and Zihlman, 1976; Zihlman 1978).

In the same vein, feminists vehemently rebutted natural selection theories of male aggressiveness and female passivity propounded in E.O. Wilson's sociobiology. In the 1970s, Wilson, a renowned ethologist from Harvard, produced a grand theory, a synthesis of biological and environmental causes explaining the present social structure in human societies through sex-typed patterns of behaviour among animals, which he called 'the modern synthesis' of sociobiology (1975). Feminists and a great many biologists rejected the spurious theorizing and the sexist overtones of Wilson's human sociobiology. His sociobiology (to be differentiated from mainstream animal sociobiology) soon became popularized by the media. For feminists and critics of science, it was important to expose the scientific flaws in Wilson's thesis and to reject it from its inception, lest it should sanction, on 'pseudo-scientific' grounds, the patriarchal power structure and acts like rape, social violence, and discrimination against women.

During the late 1970s, feminists more directly addressed the flaws of biological explanations of sex differences. Their critique has since covered theories of fetal development, hormones-linked behaviour, physiology, and brain functions. By this time, feminists had realized that not only "bad science" needed to be revised on feminist premises, but also "science as usual" -- its metaphors, its epistemological basis, and its institutional functioning.

Thus, primatology, sociobiology and theories of sexual differentiation are the main areas addressed by feminist critics of biological knowledge. Let us examine in detail their arguments and assess them from two perspectives, from that of ideology and that of scientific epistemology.

Primatology, Evolution Theory and Anthropology: Unveiling Androcentric and Patriarchal Representations in Biology

In 1975, a collection of critical essays entitled Towards an Anthropology of Women (Reiter 1975) examined the roots and development of inequality between the sexes. Aware of the "potential for a double-male bias in anthropological accounts" influencing both social facts and scientific thought, these essays provided, among other things, new evidence and interpretations in primatology and evolutionary theory. As feminists, as primatologists and as anthropologists, the authors had three goals: they needed to revise "male" explanations such as those based on the notions of male dominance and "Man the Hunter"; they needed to increase the numbers of studies on female specimens; and they needed to cultivate a scientific consciousness congenial to an anthropology of the whole of "human kind", including both males and females as full-fledged subjects. Recognition of the diversity among species and cultures was also important.

Feminist primatologists and anthropologists have given rational re-interpretations (i.e. following the rules of logical inference) of old or more recent anthropological and paleontological evidence whose content does not trivialize, undermine or deprecate the roles of females. These have become part of current anthropological and primatological theorizing, in the continuation of the work of several scientists of both sexes (Fisher 1980; Hrdy 1981; Slocum 1975; Leibowitz 1975). By comparison, although he himself acknowledged and pointed out different instances where male-dominated social organizations of primates do not exist, Wilson did not feel compelled to revise the universality of male dominance altogether.

Therefore, the unearthing of male biases in the research questions being asked and in the assumptions guiding the interpretation of data, in addition to an acknowledgement of the relative absence, or trivialisation, of data on female subjects formed a critical framework on the basis of which feminists endeavoured to shed new light on the flaws plaguing primate and evolutionary theories relating to the sexes (Fisher 1980; Hubbard 1979; Haraway 1978; Reiter 1975; Slocum 1975). I shall give some examples.

Leibowitz (1975) mustered data on diverse species of primates in order to disprove the theory relating sexual dimorphism[2] to sex roles, a theory which traditionally supports male-dominance interpretations of mating behaviour. In her essay, she shows that Orangutans exhibit a marked degree of sexual dimorphism (e.g., males can weigh twice as much as females); yet males are rarely involved in aggressive interactions, while the females which are uninterested in mating can reject males without difficulty. These observations contrast with plains-living baboons, a species that also is highly dimorphic, but presents strong evidence for male aggressiveness. Leibowitz does not disagree with the study of De Vore's and Washburn, a common reference on the subject, indicating that the male-dominance model seems to apply appropriately to baboons. Among plains-living baboons, males are usually leaders and protectors of the group, while females are mainly nurturers and sexually receptive to the males competing successfully for mating. She stresses, however, that the forest-living baboons display sex-role patterns which differ from plains-living baboons. Within the former groups, old females can lead the group. Female baboons can initiate intercourse with various males, and are not necessarily sexually passive. Males may change from one group to the other, and are also often seen as the first to escape if danger is imminent, leaving females by themselves and encumbered with infants. Leibowitz concludes that traditional explanations of primates' sexual roles are flawed on two grounds: on the choice of baboons to represent a universal model of sexual roles, and on the neglect of the influence of environmental conditions on patterns of sexual behaviour developed among primates, which may vary within the species.

Fisher (1980) goes even further in her criticism. She gives various instances where "patriarchal" interpretations have crept into evolution theorizing. She questions, for example, the use of terms like "possession of females", "dominance in intercourse", or "undersexed males". Why, she asks, have primatologists used such wording to describe facts which do not necessarily lend themselves to this kind of interpretation? Discussing the notion "mounting", for instance, she says

> 'Mounting' is the male-oriented but not always accurate term of biology. The male chimpanzee sometimes stands behind the female, clasping her round the waist; othertimes he squats, with his buttocks barely clearing the ground. Gorillas have been observed to sit with the female before them, a position which George Schaller describes as seated in his lap, though to a woman observer the female gorilla might well appear to be on top, with her partner underneath. (1980, 13)

This kind of male bias, Fisher holds, has led to a systematic undervaluation of the role of females in human evolution and in the development of cultural artifacts. She emphasizes that male-centredness and modern patriarchal interpretations do not have their place in studies of primates and hominids. She gives plenty of evidence on which she can base her counter-interpretation of male dominance. On this account, she agrees with Slocum (1975) who severely criticizes the concept of "Man the Hunter", developed again by Washburn in the 1960s. It seems legitimate, Slocum writes, to start from the premise that male hominids were mainly protectors, while females were nurturers. Indeed, as she notes, the female-child bond is probably the only truly universal pattern in primates and hominids' social organization. However, it is preposterous to infer that male hunters were solely responsible for the evolution of communicative signs and codes, and for the development of artifacts such as tools and small weapons.

Like Fisher, and also Tanner & Zihlman (1976), Slocum proposes that women gatherers and caretakers were more likely than male hunters to have developed skills, customs, and tools reflecting a genuine evolution of the human species. The reason is that females were the individuals mainly responsible for passing these skills, customs and tools to their offspring. Secondly, these authors indicate that gathering and hunting small animals, activities usually performed by the females, have been known to have provided approximately eighty percent of the food supply of the community. Why, then, pay so much attention to "big game hunting" as the motor of hominid evolution? Only a systematic male bias could have forced the anthropological evidence to fit a theory such as "man the hunter"; in general, evidence indeed appears more congenial to a theory emphasizing the role of women gatherers and nurturers in the early evolution of the human species, its language and culture.

More generally, Slocum contends that anthropological evidence neither guides the formulation of evolutionary theories, nor validates them. She contends that it is how the questions are being asked which determines and limits the array of possible answers. For her, it seems, evolution theory of hominids is a short-hand for a self-fulfilling androcentric and patriarchal prophecy.

For her part, Hrdy (1981) argues that feminist theories emphasizing the positive role of females in evolution do not go beyond the conventional assumptions of a division of labour. Hrdy aims at providing evidence for the idea that females too have evolved by being competitive, socially involved and sexually assertive individuals like the males. But her rationale is not shared by all feminists, who would instead try to deter any new attempts to revive the ideas of dominance and aggressiveness in evolution theory[3].

The impact of the feminist criticisms of primatology, evolution theory and anthropology has transgressed the simple unearthing of male-biased metaphors. Feminists were also led to address the controversial questions of how scientific changes occur; and on which kind of epistemological foundations might a new, 'better' science be built.

Tanner & Zihlman (Tanner and Zihlman 1976; Zihlman 1978) suggested that a reconceptualization of anthropological evidence on the basis of feminist views ought to be juxtaposed with any strict biological determinism which emphasizes men's achievements in evolution at the expense of women's roles. Their criticisms have thus led to a complete revision of the conventional biological approach towards cultural and sexual evolution.

Slocum went even further. She highlighted that the inheritance of male-centred knowledge relating to the evolution of hominids was partly due to the "underdetermination of evidence" in scientific theorizing, implying that the procedures of validation of scientific theories themselves are imperfect. But by the same token, she raised a major problem for 'feminist' scientists: on which basis would they justify their own scientific theories if empirical evidence is not sufficient to prove them right?

Donna Haraway (1978, 1981) is representative of those feminist authors who have developed these two types of critical reflection to their logical extreme. She is widely quoted in feminist literature about primatology and biological science in general. Her views thus deserve special attention in order to assess the sociological, methodological, and epistemological problems associated with the elaboration of a feminist biology.

In a series of articles on animal sociology and primate behaviour (1978), Haraway examines the work of C.R. Carpenter (anthropologist of the 1930s) showing that his thesis of sexual roles reinforces the notions of "dominance pattern" and "patriarchal authority" in biology. She contends that animal sociology has built on the assumption of "the union of the political and the physiological", yet without questioning such a premise nor claiming any clear evidence to support its empirical accuracy. According to Haraway, the union of the political and the physiological epitomizes the vindication of a male body politic within the science of animal sociology. She propounds the view that, as its challenger, a female body politic could only be vindicated by means of a critical insight (like Marxism or that of the Frankfurt School) into primatology, animal sociology, and sociobiology, leading to a complete rethinking of the basic patriarchal categories relating to sex-economies.

Haraway goes even further in her reflection on the reconstruction of biological theories, sometimes extending her conclusions to the whole area of scientific epistemology. In her article, "In the Beginning Was the Word: The Genesis of Biological Theory" (1981), she contends that the power of naming and speaking of well-known scientists has become paramount in the production and legitimation of social beliefs and scientific knowledge. She argues that biology is a body of "tales about origins, about genesis, and about nature"; that it consists of "rhetorical strategies". Therefore, she claims that "the contest to set the terms of speech" must become the impetus for "feminist struggles in natural science" (ibid., 471), and the platform on which feminist biology will build its legitimacy. This is why Haraway urges feminists to articulate their position to construct a feminist world, despite the obvious difficulties in doing so from within a science dominated by males.

According to Haraway, it is no surprise that feminists have been so preoccupied with language. Hitherto, they have had to resort to a scientific language permeated with a patriarchal voice which has very successfully subordinated the reality to its own discourse and terminology. The dichotomization between the concepts of nature and culture, for instance, or other "totalitarian objects", such as "the gene", were all elaborated within a patriarchal science. These have left the patriarchal order untouched with no room for more flexible explanations of sex and gender constitution involving culture, whereby the concept of 'female biology' would not necessarily equate 'women's social destiny'. As a result, Haraway strongly urges feminists to find a new voice and fight the patriarchal world, its language and its interpretations.

Yet Haraway warns against the anarchy occasioned by different voices. She therefore explores the sort of epistemological foundations which a feminist science would require in order to be seen as a science of truths rather than just as another voice, whose claims to authority would be discursive or simply political. She therefore urges feminists to construct a theory of representations warding off the epistemological vacuum[4] induced by relativist and language-focused approaches to knowledge.

By doing so, however, Haraway seems, in the end, to undermine all she has been saying against patriarchal biology. In other words, she seems to say that her feminist discourse is not more justified than old patriarchal theories in primatology, hominid evolution, and animal sociobiology. Also, I should like to suggest here that the kind of strong phenomenological and constructionist theory of knowledge which a feminist like Haraway propounds can only be sustainable within a context allowing for the co-existence of diverse bodies of knowledge. On the other hand, if these bodies of knowledge become conflictual, and when a time comes when they <u>must</u> reject co-existence, then phenomenology fails to justify changes and replacements of old knowledge by new knowledge on the basis that the former was flawed with 'errors' and the latter is 'true', for everything is assumed to be 'true' within its own context. If feminist critics of biology aim at rejecting patriarchal knowledge because it is flawed or simply 'untrue', then a phenomenological theory of knowledge is epistemologically inadequate to justify that.

I should also like to point out that the 'biological' questions addressed by feminists, evolution theories and studies in animal behaviour as it were, have a lot in common with the interrogations characteristic of the social sciences. These 'biological' questions concern themselves with human beings (or human-like beings) whose ontology is indeed rather remote from that of the inanimate objects or micro-organisms central to the physical sciences and modern biology. Unlike the social sciences however, biology is not always concerned with culture and history: in fact, it primarily deals with the empirical evidence of non-intentional organic matter. Hence, the clarification one can make at this point: feminist primatology and anthropology are not necessarily biology but more specifically sub-sets of bio-behavioural studies.

Let us now look at the feminist critiques of sociobiology and test whether the above argument holds. Sociobiology uniquely tries to merge a biology of micro-organisms (genes) with the ontology of human beings, endeavouring to use modern evolutionary theory to an understanding of human behaviours. Thus, the feminist critiques in this case might announce a slightly different standpoint on scientific theory, methodology, and epistemology.

Sociobiology Criticized

The criticisms and defences of sociobiology can be classified into four types of disputes. First, disputes focus on the propaganda overtones of sociobiology, leaving behind its heuristic values. In fact, the kind of sociobiology developed by Wilson (1975, 1978), Tiger and Fox (1978), and Barash (in Haraway 1981) bears a distant resemblance to the careful reflections of the King's College Sociobiology Group (1982). Albury (1980) is one of many critics who insists that Wilsonian sociobiology hypotheses are formulated in a way that they can easily be used to support the beliefs in the aggressiveness, sexual promiscuity, and dominance of the males over the females, especially among the lay public. Sociobiologists and other apologists of sociobiology (in Caplan 1978), or philosophers such as A. Rosenberg (1980), and Ruse (1978, 1981, 1987, 1988), however, defend the Wilsonian hypotheses on the grounds that they are legitimate scientific conjectures and hence refutable as such.

The second dispute concerns the importance allocated to environmental factors and to genetic factors in the explanation of an organism's behaviours. For instance, Albury (1980) and Guille-Escuret (1985) refute the allegations of Wilson, Ruse and Dawkins (1976, 1986): while the latter stress that sociobiology accounts for environmental factors in its explanation of behaviour, the former argue that the whole undertaking of sociobiologists would indeed become meaningless if it were not for its total commitment to the idea of a genetic determinism of behaviour.

The validity of Wilson's or Dawkins's sociobiology as an explanation of human behaviour is severely attacked by radical scientists, such as Rose, Kamin and Lewontin (1984), but also the feminists Birke (Birke 1984c; Birke and Best 1980a: Birke and Silvertown 1984), Bleier (1984), Lowe (1978), Hubbard (Hubbard 1979, 1982; Lowe and Hubbard 1983), and the members of the Brighton Women and Science Group (1980). They reject the idea that DNA is the <u>sole</u> and <u>direct</u> source of human phenotypes and behaviours. For these authors, who are all trained biologists, genetic determinism is wrong in general: it is a sheer simplification of the complex processes involved between the genetic release of biochemical substances, and the cellular, physiological, anatomical, and neurological development of the whole organism in its environment, all of which are yet to be understood[5].

The third and fourth types of dispute fostered by Wilsonian sociobiology are precisely about the analytical and methodological flaws in its theory-building. As Bateson

(1986), Guille-Escuret (1985) and Hampe and Morgan (1988) note, sociobiological theory places causal models of sub-cellular events and intentional models of human action on an equal footing. This logic of analysis is highly simplistic and not even excusable on grounds of parsimony.

On the methodological level, the truth-claim of scientific theories like sociobiology remains highly uncertain in light of the empirical under-determination of primatological theorizing about sexual behaviour. As shown previously, several feminists have challenged the interpretations of white male primatologists in relation to primates and hominids' behaviour. Feminist critics of sociobiology have, in turn, questioned if it is sound to infer human action from models of animal behaviour, and to draw quasi-definite conclusions on the basis of very scanty (and highly contradictory) primatological and anthropological evidence (Bleier 1984; Lowe 1978; Lowe and Hubbard 1983).

Let us thus turn to the works of Ruth Bleier who has devoted a large part of her feminist reflection to an analysis of the flaws of Wilsonian sociobiology. Her works are largely quoted in feminist literature, and they provide a sound basis for a discussion of the specific contribution of feminists to this area of biological thought. More specifically, such an examination will highlight the new problems arising from the feminist attempt to elaborate a new research programme in human biology, a strong environmentalist approach, replacing that of biological determinism.

In her book <u>Science and Gender. A Critique of Biology and its Theories on Women</u>, Bleier says that <u>animal sociobiology</u>, as a field of study, has provided useful insights for an understanding of the social behaviours of animals. As for the <u>human sociobiology</u> developed by Wilson however, she argues that it is "deeply flawed <u>as a science</u> conceptually, methodologically, and logically" (1984, 16) and invokes several reasons for this. First, she contends that the basic premise of Wilsonian sociobiology, that <u>human behaviour</u> has evolved through adaptation based on Darwinian natural selection of adaptive behaviours now encoded in our genes, is totally spurious. For, in general, sociobiologists, like a majority of geneticists, agree that it is not possible to link any specific behaviour to any specific genetic configuration, and that what the presence of a gene indicates is only a 'biological potential "for" human behaviour' and nothing more precise than that! For Bleier, therefore, Wilsonian human sociobiology is just another "elaborate mythology of women's biological inferiority [introduced] as an elaborate explanation of their subordinate position in the culture of Western civilizations" (1984, vii).

Bleier contends that even though, in principle, sociobiologists like Barash and Dawkins believe that culture and learning play a role in the shaping of behaviour, in practice they do not acknowledge any extra-genetic influence on behaviour. She writes:

> But what is really at issue in Sociobiological theory is not the physical capacity for behaviour that biology provides but rather the genetic encoding of the entire range of complex human behaviours and characteristics that are expressed in a nearly infinite variety of ways by different individuals and cultures and often not expressed at all, such as altruism, loyalty, dominance, competitiveness, aggressivity. (1984, 17)

This logic gives rise to unwarranted generalizations about the presumed innate female and male differences in reproductive strategies and human roles. For instance, according to Wilsonian sociobiology, the maximization of selected male genes operates quite differently from that of female genes, and this explains why males and females have different reproductive and mating strategies: the males are "naturally" promiscuous because they aim at frequent inseminations, while the females are more choosy because they must look for mates that will be willing to protect their offspring. Bleier opposes this logic which, she contends, rests on gross psychology and on a spurious theory of genetic determinism of human behaviour. Bleier also points out that the characters selected as empirical evidence are simply those of the upper/middle class white Anglo-Saxon scientists who consider male aggressivity, competitiveness, and selfishness to be typical of human societies. By and large, she argues that the logic of human sociobiology is circular: once traits are selected, a "gene for" this trait is sought, and then used to feed-back the logic of genetic determinism[6].

Human sociobiology, Bleier contends, does not relate to any sound empirical evidence; there are no specific behavioural traits in hominids which leave fossil records; hence, no specific proof of genetic encoding of these is possible. Sociobiologists must resort to animal observation in order to make for the missing pieces of evidence of the biological evolution of human behaviour. Bleier therefore claims that:

> Sociobiologists attempt to reconstruct evolutionary theory by inventing plausible stories that attempt to show how a particular behavior or social interaction in humans or other species could have or would have been adaptive and therefore favoured by natural selection and genetically carried through subsequent generations. Basically, the aim is to establish the biological 'innateness' and inevitability of present-day human behaviours and forms of social organization. (1984, 22)

Bleier finally underlines theoretically flawed deterministic stances on the development of the embryo embodied in Wilsonian sociobiology. Drawing from the works of Ruth Hubbard, Bleier stresses that protein synthesis in the embryo "is not determined by the gene alone but is also a consequence of the environment in which the molecule finds itself" (ibid., 43). She continues by stressing that in human behaviour the environment in question is manifold: from the physiological milieu, essential in the development of the foetus and its brain, to the learning (cultural) environment. Bleier concludes her demonstration by reiterating that, notwithstanding all the other scientific flaws, human sociobiology remains invalid first and foremost because human behaviour is the result of the manifold interaction between biology and the environment. She writes:

> Behaviors are the products of the brain's functioning in interaction with the external world, and the innumerable patterns of social behaviours, relationships, and organization that characterize human societies have evolved through cultural transmission within specific historical contexts. (1984, 46)

Thus, the feminist critiques of human sociobiology reveal an element which was not stressed as much in primatology and anthropology. By rejecting crude biological determinism, they raise the importance of the environment, organic and social, in the study of biology in general -- and human biology in particular. They do not wish to dispense with biological factors altogether, however; they maintain that both the biological and the social environments must be taken into account in feminist biology. But this is not an easy undertaking, for dealing with the concepts of biology and culture simultaneously involves the articulation of two different levels of analysis, causal and historical, and as a result, two different epistemological traditions. The next section will highlight some of these difficulties.

Before we close this section however, let us present briefly the points feminists have raised against the Wilsonian sociobiology, and see the evolution of feminist thinking about biology since the first studies in primatology.

First, feminists have claimed that human sociobiology is preposterous in inferring human behaviour from animal models. The sources and motivations of men and women's patterns of behaviour, temperament and mating are more complex than sociobiologists want to show (Lowe 1978; Lowe and Hubbard 1983).

Secondly, variations <u>within</u> the sexes, races, species are much wider than variations <u>between</u> those same groups (Lowe 1978). So, feminists contend, why invest so much time studying the genetic variation of <u>some</u> sex-related differences instead of other types of variations? This bias in the agenda of biological research can only be explained by the vested interests of dominant groups within the scientific institution (Bleier 1984, 1985b, 1988a; Brighton Women and Science Group 1980; Rose and Rose 1976c, 1976d).

Thirdly, as far as behaviour and women's social achievements are concerned, recent historical studies carried out by feminist historians show that culture has constrained women much more than their biology may have done (Hubbard 1979; Sayers 1982; Merchant 1982; Jordanova 1980).

Fourthly, feminists oppose the idea of changes to that of biological destiny which human sociobiology induces. They suggest that human biological science should build upon the humanist ideas of a full realization of human potential and a possible transformation of the social order (Bleier 1978; Brighton Women and Science Group 1980; Hubbard 1979; Lowe, 1978: Sayers 1982).

Finally, I should like to draw attention to two aspects in the feminist critiques of sociobiology which will bear heavily on their strategy to elaborate a new approach to biology.

First, feminists do not agree on, or discuss thoroughly, the role of language in the production of knowledge. As shown earlier, Haraway for example, saw science as mere story-telling. But Bleier does not, for her part, and as the next section will show more clearly, think that language has such power over scientific knowledge (1984, 1988a, 1988b). She assumes that reality is constructed by language only to a certain extent. She believes in the existence of some hard-core facts independent of our power to name them. But she also warns against those sociobiologists "who play loose with language", and infer from evolutionary genetics and scanty evidence that, for example, women have an innate inclination to child care.

Secondly, several feminists who maintain that environment influences biology, go so far as to say that it is impossible to distinguish the impact of biology from that of the environment on behaviour (Lowe 1978, 1983; Bleier 1984). But this argument gives rise to an epistemological 'catch-22'. Feminists obviously try to justify their own discourse about biology and environment on the ground that they could effectively discern the effects of environmental factors from the effects of genes in behavioural development. Bleier indeed argues that:

> With increasing sophistication of conceptualization and the equipment available to make scientific observations ... increasing knowledge is gained about the influence of prenatal and early post-natal environment and learning in the determination even of the kinds of behaviours in birds and mammals that previously were called instincts and thought to be entirely genetically programmed and not learned from others. (Bleier 1984, 44)

My point here is not to suggest that it is easy to differentiate the impact of the environment from that of biology -- particularly in studies of behaviour. I am just highlighting a contradiction in the discourse of feminists who seem to dispense too hastily with the possibility of analyzing these factors separately, in favour of a rhetoric of anti-biological determinism.

The next section will highlight the practical problems and theoretical contradictions which the argument of the inseparability of biological from environmental factors must confront. It will suggest that feminist biologists cannot logically (or epistemologically) support this central argument of the feminist critiques of biological science, for it is incompatible with the rules of justification of knowledge in the empirico-analytical sciences, those by which they would otherwise continue to abide in the construction of feminist biology per se.

Feminist Approaches to Biology: the Difficult Integration of Environment and History in the Genesis of Sex/gender Patterns of Behaviour

A brief review of feminist criticisms of biological explanations of sex differences in behaviour will help to underline the importance which an holistic approach has had in the elaboration of a feminist approach replacing that of current biological determinism, or reductionism.

During the nineteenth century and right into the twentieth, scientific explanations of sex variations in social roles have been based on biological determinism. According to feminists, this type of research reinforced the ideology of male superiority, without questioning the impact of culture and the rigid social stratification of gender roles on women's behaviour (Merchant 1982; Rosenberg 1982; Sayers 1982).

Shields (1982) for instance, points to the variability hypothesis which stated that males are more likely than females to vary from the norm in both physical and mental traits, thus safeguarding the ideology of the superior intellect of the male. In the early 1900s however, a number of scholars showed that the environmental factors might have impeded the full realization of women's biological and intellectual potential. Nevertheless, as Shields points out, history has shown that patriarchal theories do not lose their credibility easily. For, even when 'stronger' rational arguments were raised against them, which could explain both old and new evidence, patriarchal theories were safeguarded by adducing ad hoc propositions[7].

At the turn of the twentieth century, several women scholars involved in research on sex differences in medicine, public health, and psychology began questioning bio-determinist theories. Rosenberg (1982) shows that theories paying more attention to social psychology and the influence of life styles and social pressure in the genesis of sex-typed behaviours and biological ailments stirred much interest from both within and without academia. She also suggests that the women scholars involved in that research were probably doubly motivated since it was of utmost importance, for them as women, to demonstrate that they could fulfil academic standards in addition to introducing a new scientific approach to human biology.

Over the past twenty years, a great many feminists applied themselves to removing the spectre of dimorphism, or rigid sex dichotomies. Sayers (1982) for instance, a psychologist, urged feminists to look at "woman's destiny" not in terms of biological predispositions alone, but also from the standpoint of history and human agency.

For feminist biologists, this meant a denunciation of biological reductionism instead and a systematic increase in emphasis on the study of environmental causes. In the late 1970s, several feminist biologists disengaged radically from any kind of research resting on a separation of biology and environment; but there have been some disagreements among these authors. Some supported a complete withdrawal from research on the <u>causes</u> of sex differences for both political and scientific reasons, like Lowe (1978). Other authors simply warned against making scientific short-cuts in explanations of sex-related differences, like Lambert (1978) or Baker (1980). Let us examine the strengths and weaknesses of these arguments with a view to the practical implementation of an original feminist approach to biology.

Lambert, a researcher in biology, defended the view that "especially in the case of higher mental functions, a precise separation of the biological bases into those which are intrinsic in origin and those which are not <u>may</u> be an <u>unrealistic</u> goal" (Lambert 1978, 105; my emphasis). She also contended that at present, when a strong biological reductionist view dominates the whole discipline, the definition of "the biological" definitely undercuts the feminist movement. She therefore urges feminists to develop a framework of biology in terms of a relationship between the organism and its environment. In brief, Lambert did not argue that biology should be dismissed altogether in the explanation of sex-gender variations in behaviour. She simply suggested that researchers avoid a biological reductionist stance on human behaviour.

Baker (1980), another researcher, however illustrated the practical difficulties and theoretical controversies relating to an approach combining biology and environment. She propounded the idea of a bio-environmental approach through her research experience in clinical paediatrics. In her view, the influence of hormones on "gender roles and behaviour" bears on "patterns of behaviour" identified in children exposed to abnormal pre-natal hormonal environments during pregnancy (Baker 1980)[8]. But, she continues, the hormonal influence does not bear on "gender identity", that is on whether a child perceives itself as a boy or a girl in congruence with its sex. Hence childrens' gender identity is linked to gender-rearing (in accordance with their biological sex) which may frequently clash with their "patterns of behaviour". As a result, Baker believes that biologists ought to collaborate with anthropologists, and the two disciplines ought to discuss their findings in order to create a deeper understanding of human behaviour.

On the other hand, feminists could argue that Baker's findings rest on a questionable "gender-typed" classification of comportments, which would not necessarily gain from anthropological studies unless these are effected from a feminist point of view (Messing 1983a). Also, if Baker's approach manifests the beginning of an environmentalist approach to biology and behaviour, it seems to do so without really instauring a framework combining biology and environment factors. In the last analysis, therefore, Baker does not reject the idea that the role of biological and environmental factors can be distinguished in an explanation of human behaviour.

A major criticism levelled in recent feminist works argues that a truly non-reductionist approach to biology should do more than merely adduce sociological or anthropological findings to biological explanations: it should totally integrate them (Lowe 1983; Bleier 1988a; Birke 1986; Birke and Vines 1987; Rose 1982a, 1982b). Let us first examine the position of Lowe who propounded the idea of a holistic approach to sex-gender differences in behaviour, and then discuss the strengths and shortcomings of her suggestion for the elaboration of a viable feminist biology.

The works of Lowe, an American chemist strongly committed to feminism, display a strong holistic approach in which environmental and biological factors of sex-gender differences should not be severed but rather, should coalesce. While she writes that these factors cannot, or should not, be separated (Lowe 1978); at other times, she examines the role which external conditions have effected on the development of women's biological characteristics (Lowe 1983). She therefore seems to contradict herself, implying that she has made a separation, at least analytically, between two levels of factors, biological and environmental. Let me demonstrate the point briefly.

In her article of 1978, in which she dealt with genetics and its influence on behaviour, Lowe suggested that to "distinguish" genetic and environmental factors in human behaviour is "insurmountable". Surprisingly enough, she also underlined, in the same article, the existence of experimental evidence showing that the impact of genetic factors varies with the environment, thus drawing the line between these two levels of factors. Lowe later gave extensive evidence demonstrating that "a great many aspects of biological function, including body size and strength, hormone levels and possibly brain development can be altered considerably by changes in the environment" (ibid., 41). She also contended that even though biological propensities exist and can be identified as such to some extent, conversely one "cannot separate" the contributions of biology and culture in behavioural differences. Lowe discussed findings on the existence and development of sex differences in strength and physical performance, height, intellectual abilities, hormones and typical behaviour, and offered the theory that women's biology can develop in a variety of ways.

She said that "much of this difference in strength is the result of society's encouraging the average man to be more active than the average woman" (ibid., 44) Thus, she concluded, as women become less confined to sex-typed activities and begin to do the same jobs and tasks as men, the idea that women's biology is their destiny will progressively wither.

There is a series of problems here. First, the demonstration of Lowe hinges on a definition of the environment which is quite extensive. This brings about a confusion as to <u>what precisely</u> constitutes the environment. Lowe does not specify whether it is social (outside of the body) or biological (inside of the body). Even though this may be congruent with her claim never to separate biology from environment, the result is that her concept either lacks utility as an analytical tool, or conceals her anti-reductionist rhetoric.

In my opinion Lowe posits a <u>conceptual</u> separation between biology and environment although she does not explicitly acknowledge it. She says,

> Hormonal contributions to behaviour depend in part on the level of hormones at a particular moment, which are themselves determined by a person's past interactions with the social environment and in part on the details of the current social environment. It is not possible to abstract behaviour from its social context. (ibid., 54)

She also clearly speaks in terms of the social constraints as separable, both empirically and analytically, from biological potential, such as women's musculature, anatomy, and intellect.

Interestingly enough, Lowe often speaks cautiously when it comes to criticizing scientific results. She will, for instance, speak in terms of a notion of "it remains to be established" (referring to sex-gender dimorphism in height), or of "what is being measured is not at all clear" (IQ tests) or "highly uncertain" (probes and tests on brain lateralization). Consequently, it is not clear whether she means that it is only a question of time before we find more definite results, or if she <u>is implying</u> that the absence of any conclusive evidence proves that it is logically impossible to arrive at answers on sex-typed behaviours. What is clear is that <u>she states</u> that it is impossible to separate biological facts from environmental factors: "Biology", she writes, "does affect human behaviour, but the examples I have discussed make it clear that <u>there is no way to separate</u> the contributions biology and culture make to behavioural differences" (ibid., 54). If Lowe implies that there is no scientific procedures (logical, experimental) which could help distinguish biology from culture, then her view is logically self-deceptive. But if she implies that there should be a guiding principle to help us get out of rigid bio-determinism, then she might have given feminists a sound orientation for a new research agenda in biology.

Finally, the contradictions in Lowe's critique seem to be totally defused in the concluding part of her essay of 1983, where she urges feminists to:

> Examine the question of the possible existence of biologically based sex differences ... but only in response to the claims of biological determinists who say that ... knowledge of these provides a guide to social policy and to the limits of possible social change. (ibid., 56)

In my view, this makes it clearer that her critique is primarily oriented against the political abuse of reductionist biology, and is much less devastating vis-à-vis the analytical potential of experimental research for biology.

The kind of clarification that has just been made in relation to the theoretical contradictions implied by the arguments for a holistic approach also stand for the recent works of Bleier (1988a) and Birke (Birke 1986; Birke and Vines 1987). It is my view that such an accumulation of methodological and theoretical incoherences might have seriously undermined the construction of a feminist biology. But I do not want to pass in silence the important fact that, even though feminists use the same methodological categories and canons of scientific justification as conventional biologists, in contrast to the latter, they indeed use them more critically when it comes to testing biological explanations of human behaviour. I should refer to Part III of this thesis for a substantive comparative study of the scientific discourses of mainstream biologists and of the feminist zoologist Lynda Birke in this connection.

In one of her latest criticisms[9], Bleier goes over the numerous scientific flaws in recent studies about "the biological basis in the brain of presumed gender differences in cognitive abilities or in hemispheric lateralization of cognitive processes" (Bleier 1988a, 93). Recent brain lateralization theory assumes that males process information predominantly with the right hemisphere, while females use both hemispheres more symmetrically. But as Bleier points out, several studies have shown that brain lateralization theory of sex differences is based on poor statistical significance of results, conflicting results or failures of replication, poor experimental design, lack of sufficient controls for variables, and lack of consensus on the definition of spatial ability[10]. Her own replication of these results shows no evidence of a sex-typed function of the splenium in the corpus callosum. She argues that even "if there were gender-associated differences in hemispheric lateralization of visuospatial function, [conversely] there is no evidence of a correlation between hemispheric lateralization and visuospatial ability" (ibid., 94).

From what has been shown, one may conclude that, from the point of view of epistemology, Bleier does not reject empirical evidence as a source of scientific proof. Indeed, as she writes, "usually quite a few different assumptions can be made, all of which can be justified though only some may turn out to be correct" (ibid., 101).

I would suggest that no matter how the real interplay between biology and the environment impedes on distinguishing their distinctive contribution to the development of human behaviour, this does not deny the right to understand them more clearly: it is indeed the mandate of all scientific endeavours, including that of feminist biologists.

My understanding of Bleier's work on brain lateralization theory of sex differences is that she endeavours to justify her claim on both counter-experimental results, to which she must obviously give some credibility, and a feminist critical stance on biological reductionist theory, analysis, and interpretation. I would contend that while she argues for a rejection of crude biological reductionism as a theory (especially as a model of explanation of intellectual abilities and human behaviours), she does not convincingly discard experimental reductionism as a methodological tool for the testing of explanations of (at least some) human behaviours. Methodological reductionism is precisely the kind of scientific commitment which feminist biologists have retained in order to elaborate a viable, theoretically original 'feminist biology'.

We conclude this chapter with a discussion of the arguments of some feminist biologists and philosophers who have proposed "milder" solutions to the eradication of sexist biology, that is to say, solutions which lie within the current epistemological framework of biology and yet reveal original theoretical thinking about human biology. We will give some evidence that the radical critiques of feminists did not have to enter the dire straits of theoretical incoherences, methodological impracticalities, or epistemological vacuousness in order to lay the foundations for a biology more congenial to the fair treatment of women in science and in society.

Feminist Criticisms of the Norms of Research Practice in Biology

Several feminist biologists have criticized biological theories about women from a less radical standpoint on scientific epistemology. In the main these feminists argue that the scientific enterprise as a whole is not to be dismissed. Yet these authors hold that a reform of science must arise and produce changes within the institution as such, that is, the organization and culture of scientific work in order to integrate women as fully-fledged participants. These reforms should help to alter the authoritarian attitudes of men towards the work of women colleagues and to eradicate the prejudices of scientists in relation to "woman" as a subject of study.

The American embryologist Fausto-Sterling (1985, 1987) maintains (like the majority of feminists) that a great many scientific theories do not so much fit the results of experiments but fit instead the cultural schemas of what nature "is supposed to be". As a case in point she reviews a study of 1981 by the renowned doctor Bruce Carlson[11] on the subject of the development of embryos; she points out that the categories used to interpret results are loaded with androcentrism[12]. Fausto-Sterling suggests that a feminist, woman-centred, approach would have produced "a narrative that treats female sexual differentiation as requiring as much investigation and explanation as male sexual differentiation" (1987, 66).

Referring to her own research experience in the area of embryo development, Fausto-Sterling demonstrates that according to mainstream "androcentric" accounts, testosterone (the hormone associated with males) is of the utmost importance in the development of both male and female embryos. These accounts state that male ducts "develop" while female ducts "regress", or as Carlson himself puts it, "develop along female lines in the absence of other modifying influences" (Fausto-Sterling 1987, 65). Fausto-Sterling remarks that the androcentric ideas of a presence-or-absence-of-maleness (i.e., that something is added to an embryo to make it male and that the female represents some natural ground state), are widespread in developmental biology.

Fausto-Sterling shows that some recent studies have built on the idea of a positive role for oestrogen (the hormone associated with females) in embryo development[13]. These studies indicate that a female-oriented perspective has begun to infiltrate thinking on biology, even though there is still a great lack of understanding of oestrogen's role in female development.

Hence, while she acknowledges the power of science to objectify a non-universal (i.e., male-oriented) understanding of gender and nature, Fausto-Sterling reiterates that feminist biology does not have to dispense with the conventional scientific procedures of experimentation. She simply warns against using uncritically male-loaded categories and control probes. In brief, she believes that feminist biology, as a woman-centred research agenda and an approach critical of male biases, can take place without renouncing the conventional canons of empirical validation in the experimental sciences. She writes,

> The activities of scientists are self-deluding and self-correcting; they are at once potentially progressive and retrogressive. What we must do in writing about them is to shuttle back and forth along the strands of meaning in order to gain more complex and accurate understandings of the processes involved (1987, 74-75).

As she also stated in her well-known book <u>The Myths of Gender</u> (1985), science cannot be <u>completely</u> divorced from the values of the society in which it is produced. Once biases in experimental designs and interpretation of results are removed however, in most cases sex differences are obliterated[14].

The views of Fausto-Sterling lead us to those of Messing, a Canadian geneticist, whose "research-action" work in occupational health has led her, and her research group, to develop an original approach to woman's biology and work. In Part III of this thesis, a case study of Messing's group, GRABIT, will discuss the difficulties and successes it has encountered in introducing such a new approach. Let us briefly indicate how Messing's views on feminist biology challenge both the political organization of research and the conventions in theory-building, while they remain relatively 'conventional' as far as canons of scientific epistemology are concerned.

Messing is especially aware of the institutionalization of a certain type of culture among biologists. She indicates that it has borne heavily on the maintenance of sexist prejudices in both the division of labour (in science and in society in general), and in the scientific approach itself, in biology and occupational health (Messing 1983a). According to her own research experience, topics like occupational risks affecting menstrual cramps or pregnancy are less likely to be carried out compared to research more akin to the interests of the (male) sponsors and employers. She also reports that information about human health and behaviour is, in general, based solely on male samples or with inadequate control groups. She points out that the rationale of selecting all-male samples for reason of 'uniformity' has the effect of excluding research on females altogether[15]. Finally, as Messing contends, feminists have justly criticized studies in which women are used as subjects but are treated very casually, this occurring frequently (especially in medical research and in tests of reproductive technologies)[16].

Messing also maintains that patriarchal biases still pervade biological research, because it allows a systematic dismissal of useful information on the grounds that the points of view of scientists are the only guarantees of objective knowledge. A feminist approach, she argues, would use methods of investigation congenial to "listening" to what patients and workers have to say, an aspect fundamental for research on women workers' health (Messing 1990a).

In short, Messing, is very critical of how science uses its influence, social prestige, and self-perceived superiority in order to support flawed results in biological research about sex variations, class, and race. Yet in spite of her strong critical perspective of the concepts and methodology used in biological research, she does not dispute the scientific value of the experimental methods and hypothetico-deductive thinking.

This section will close with a discussion of the views of the American philosopher Longino and biologist Doell. Like Fausto-Sterling and Messing, Longino and Doell support the idea that feminists "do not have to choose between correcting bad science or rejecting the entire scientific enterprise" (Longino and Doell 1983, 208). Rather, feminists must immediately embark on a criticism of the institutional setting of scientific work and its impact on the research agenda of biology. They claim that the operations of bias in scientific thought are complex and need to be taken as they are, and as they are alone, not as a facile excuse for condemning science as a whole. What is most interesting in their work is that they distinguish, among diverse areas of biology, where the feminist critics could contribute most, and where their arguments have, relatively-speaking, failed.

Longino and Doell published an article, widely cited in the feminist literature about science, on analysis and reasoning in two areas of biology: evolutionary studies and hormonal studies (1983). In this article, they criticize science for being permeated with androcentric biases in its representations of women's body and behaviour. Like other feminist biologists, they focus on the selection of questions, data, and hypotheses. But they concentrate more specifically on the "distance" between evidence and hypotheses, on "the logical notion of being more or less directly consequential" (ibid., 210).

Longino and Doell do not overlook the fact that scientists make up for missing links between evidence and theory by enforcing their explanatory ideals. But they also believe that certain scientific procedures ought nevertheless to be secured. They claim these are among our best guarantees against error. Having said that however, they endeavour to show that the distance between evidence and hypotheses differs depending on the nature of the biological question at hand. This implies that conventional procedures of scientific proof loose credibility depending on the types of biological subject being studied. They take the example of hormonal studies to prove their point.

As they explain, there are different types of hormonal studies of sexual differentiation which do not encompass the same range or "distance" between evidence and theory. They single out three types of studies: anatomy and physiology, temperament and behaviour, and effects on cognition; they then compare these types of studies. Across these three areas of study, the data available is not consistent, ranging from simple measurements of hormonal levels in the body, to animal experiments where typical sexual behaviours (mounting, mating posture, female receptivity) are induced by injection of male or female hormones, to behaviour of humans with hormonal imbalances or defects. In anatomy for instance, studies of hormonally caused genitalia differences in humans are more straightforward than the other two types. When male fetuses are castrated they clearly develop a female appearance. In physiology, however, the physiological effects of this kind of intervention are not clearly identified, and Longino and Doell note the general agreement among biologists that further analysis is needed to see how they unfold. Finally, the link between levels of hormones and differential cognitive potentials is not clear at all, and controversies on the subject continue, as will be shown below.

In the main, Longino and Doell aim to demonstrate that the interpretation of the empirical link observed in the first type of study seems quite conclusive, while it remains blurred in the other two types of studies. Thus, in the last two types of studies, theorizing is likely to build on logical inferences which are not totally warranted. The relation between data and hypotheses becomes more complex and blurred in studies attempting to link hormonal levels with behaviour patterns. Despite these difficulties, Longino and Doell have noted the wide diffusion of studies on the subject, the findings of which being highly controversial, and the research design, not entirely rigorous[17]. As Longino and Doell demonstrate, in these studies the identification of typical behaviour may have been influenced by the observer's expectations, the classificatory judgments of which may be biased in one way or another. In addition, the early environmental factors which may have shaped gender role behaviours of children are not taken into account.

Longino and Doell also contend, like many feminists, that to resort to research on non-human mammalian species in order to understand human behaviour is contentious; it rests on the controversial premise of a continuity of behavioural phenomena throughout the species. But more specifically, they argue that since human situations are highly interactive, human dispositions cannot be exclusively associated with prenatal or neo-natal hormonal levels. They maintain that inference of causation in these cases is presumptuous, worst of all if drawn from animal modelling. Longino and Doell thus conclude that: "the considerable distance between evidence and hypotheses regarding the hormonal determination of behavioral sex differences contrasts sharply with the close fit between the two in the case of anatomical sexual differentiation" (ibid., 222). This leaves a great gap in explanations of human behaviour, a gap to be filled in ultimately by "the preconceived ideas and values of the researcher" (ibid.).

But, ask Longino and Doell, does this imply that the entire physiological project itself is intrinsically sexist? To their mind, the answer to this question should be qualified. On the one hand, such a project actually is, in current circumstances, a sexist project, displayed first and foremost through its descriptions of gender-dimorphic behaviours. This type of problems can be corrected by choosing less value-laden terms which still emphasize gender dichotomies (e.g., the term "tomboy" to describe the behaviour of girls); and by developing "cross-cultural study and a more sophisticated vocabulary for the description and classification of behavior [which] might help to avoid the barbarisms of ethnocentrism" (ibid., 224-25)[18]. Therefore Longino and Doell believe that theoretically it is possible to minimize the biases and augment the description of genuine bio-behavioural differences.

They even suggest that it could be possible to find physiological correlates to these theoretical differences. As a result, and if their argument holds, it allows for a feminist practice of biology to improve and refine the agenda of physiological enquiry using the conventional canons of scientific epistemology.

Longino and Doell conclude their article by attempting to define what an appropriate feminist response to masculine bias in science might be. First, they suggest that androcentric assumptions must be recognised, and then replaced by more cogent, less sexist, premises and interpretations. In their view, a sound feminist response to androcentric science would have to search for additional determinants of behaviour. Secondly, they encourage feminists to look for and eradicate the sexist motivations of some research programmes. Finally, they close their discussion by urging feminists to commit themselves to a rational debate:

> It is not necessary for us to turn our backs on science as a whole or to condemn it as an enterprise. In a number of ways, the logical structure of science itself provides opportunities for the expression of the creative and self-conscious sensibility that has characterized recent feminist attempts to transform the sciences. (ibid., 227)

In brief, for Longino and Doell, and also for Fausto-Sterling and Messing, feminist responses to current biology must be tactical, articulating their criticisms, but also avoiding the hostility (or total indifference) of the scientific milieu. Thus, in the end, neither could they nor should they totally avoid abiding by the established canons of epistemology in the empirico-analytical sciences.

Summary: What Have the Feminists Critiques of Biology Shown?

To sum up the findings of this first chapter, let us start with the fact that feminists have been interested in the science of biology for two main reasons. First, biology is at the heart of a definition of nature in contrast to culture, and socio-historically it has rested at the heart of a genesis of the gender structure of society. Secondly, biology has tended to define human behaviour and its development from within the confines of fixed categories of sex, race and class: hence it has become a powerful ally of the status quo.

It should be clear at this point that the bulk of the feminist criticisms of biology revolve around the themes of evolution, sociobiology and biological theories of sex/gender differences in health and behaviour. It is thus a small area of biology which is substantively at stake in the feminist literature. It is also noteworthy that these questions are those which, from the perspective of a sociology of knowledge, have always been most controversial in the life sciences[19].

Feminists have singled out the presence of male biases in hypotheses, otherwise left unnoticed in biological research. They claim that male-centred and male-dominance biases pervade biology for the same reasons they pervade science and society in general: first, because of the dearth of evidence, but ultimately because of the epistemological principle of the empirical underdetermination of scientific inferences; and, secondly, because of the power of the male-dominated scientific establishment to filter out "woman-centred" and "feminist" questions and hypotheses.

Male-biased assumptions have also survived for more trivial methodological reasons, that is to say, 'bad' science. Feminists often stress the inadequacy of the samples being used in biological research. As they emphasize, male-only samples should not be deemed as universal, or as the sole reference of normality. In addition, animal models should not be used to represent accurately patterns of human actions and behaviour.

Male biases have, however, been maintained on far less obvious caveats. Eminent biologists, as well as radical scientists and feminists, have criticized the theoretical limitations of biological (and genetic) determinism and of its meta-theoretical premise, biological reductionism (i.e., reducing higher-level phenomena like biological functions and human behaviour to explanations in terms of lower-level elements such as genes, molecules, hormones). Biological forms, they argue, are evolutive: they are not fixed, they change and interact with the conditions in the environment. Biological reductionism provides useful models of explanation of certain phenomena, but it does not reach a complete and true understanding of living objects.

Feminists have finally urged a change in the authoritarian discourse of current biology; they urge that it be more open to counter-evidence, contrasting points of view, and social criticism. In this light, several feminist biologists and epistemologists emphasize the "method of listening" as a way of evaluating their own observation and interpretive frameworks. Yet in this regard, feminists may have reached an epistemological paradox. For the majority of their counter-arguments still resort to the established canons of justification to prove themselves right.

The next chapter will examine the suggestions of feminist biologists, sociologists and philosophers of science in order to integrate the feminist criticisms of biology into a general framework of feminist science. It will be shown, as this chapter has suggested, that feminist critics of science have reached an epistemological impasse which could, however, be avoided. For their critiques do not invite logically to an 'epistemological revolution', but rather to the renewal of biological research upon feminist <u>theoretical</u> advances and, to a certain extent also, upon a greater attention to the 'biological' evidence gained from observation of the life and work of women.

<u>Endnotes</u>

1) Several early non-scholarly scientific contributions by women are cited in Alic (1986), Gosztonyi-Ainley (1986), and Schiebinger (1987) for instance.

2) Sexual dimorphism is defined as the constant differences between males and females beyond the basic functions of sexual organs. See Winchester (1969).

3) See, for instance, Bleier (1978, 1984); and Birke (1984c). Hubbard (1979), for her part, contested the whole sociobiological endeavour. Interview of Ruth Hubbard, by the author, Cambridge, Massachussets, 8 May 1989.

4) Indeed, Haraway does not agree with feminists who hold a quasi-Marxist view of knowledge. She does not believe that women could develop a knowledge, a better science, encompassing both the view of a dominant and the dominated discourse on the

sole ground of their objective location as dominated minorities. Her reason, however, for not subscribing to their arguments seems purely intuitive. She writes:

> I find this feminist approach promising but not fully convincing. That argument must wait. What becomes clear, however, is that feminists have now entered the debates on the nature and power of scientific knowledge with authority: we do have something to say. The only remaining problem is what, and here we are speaking in many voices. (1981, 481)

5) Moreover, as several sociobiologists cautiously have contended, the "gene-centred" logic in behaviour studies should not be confounded with gene-deterministic hypotheses (Bateson 1982; 1986; Hampe & Morgan 1988). For if, in evolutionary genetics, one is justified in saying that the organism and its behaviour ("the vehicle") are there to protect the DNA ("the replicator") transmitted to offspring; on the other hand, it is not the DNA which is selected and has a universal function of survival, it is the characters (and behaviours) of the organism which are selected.

6) This is also the argument put forward by Gould and Lowentin (1979).

7) The same rationale applies to the discipline of craniometry, which supported the superiority of the male intellect on the basis that men's skulls (read: brains) are on average bigger than those of women. Unfortunately, it was soon discovered that the average skull of blacks was bigger that than of whites, hence shattering the white male-dominance logic of craniometry. See Katz (1988), Sayers (1982), and Gould (1985).

8) Baker studies the following discordant populations of children: CAH girls (congenital adrenal hyperplasia), TF subjects (testicular feminization), Reifinstein's syndrome subjects, Turner's syndrome subjects, and subjects whose mothers underwent hormonal treatment (surplus of progesterone or surplus of oestrogen) during pregnancy.

9) Ruth Bleier, considered by many feminists to be a pioneer and leader among feminist scientists, died on January 4, 1988. See J. Walzer and L. Gordon, 1988. "A Decade of feminist critiques in the natural sciences: an address by Ruth Bleier", Signs, 14, 1 (Autumn): 182-85.

10) Bleier quotes the research by N. Geschwind and P. Behan ("Left-handedness: association with immune disease, migraine, and developmental learning disorder. In Proceedings of National Academy of Sciences 79, 1982) about testosterone restricting the development of the left hemisphere of the brain of males "in utero". This study is based on trials on 507 fetal brains, and concludes that there is actually no statistical difference between the sexes. Bleier also criticizes a study of C. de Lacoste-Utamsing and R. Holloway on corpus callosum: "Sexual dimorphism in the human corpus callosum", Science 216 (1982), which examined a substance which connects the brain hemispheres, and suggested that the female brain is less well lateralized than the male brain for visuospatial functions.

11) Bruce M. Carlson, Pattern's Foundations of Embryology (New York: McGraw-Hill, 1981).

12) In his study Carlson measures the effect of "male" hormones alone, in a fashion which makes the reader think that "female" hormones are not important in embryo development. Indeed, feminist biologists have often opposed the usage of the term "male" and "female" hormones because this fosters the idea that testosterone is exclusively found in males, while oestrogen and progesterone are exclusively found in females. Such dichotomy of male/female nature fosters the idea of hormonally induced sex and gender differences. See, for instance, the critiques in Sapiro (1985).

13) These studies have shown that the presence of oestrogen in the embryo milieu changes males into females (in fish), that males may be immersed in oestrogen and progesterone in placenta (in mammalians), and that brain cells can convert testosterone into oestrogen (in male rodents).Fausto-Sterling quotes Ursula Mittwoch, Genetics of Sex Differentiation (New York: Academic Press, 1973); J.D. Wilson et al., "The Hormonal control of sexual development", Science 211 (1981); R.W. Goy and B.S. McEwen, eds., Sexual Differentiation of the Brain (Cambridge, MA: MIT Press, 1980).

14) It is recognized however, that Fausto-Sterling does not readily undertake an examination of the reasons why science has, for so long, been such a male ghetto and promoted a male-centred research agenda. See, for instance, S. Rosser, "Book Review", Signs 12 (Winter 1987): 402-5.

15) Messing also cites examples of samples used in cancer research in industry, in research into heart disease, and in studies of mating behaviour.

16) A prime example of this would be research conducted with placebo pill: the result effected was pregnancy for many poor women participating in the study. See also the works of R. Duelli-Klein (with R. Arditti and S. Minden) Test- Tube Women: What Future for Motherhood? (London: Pandora, 1984); and "What's 'new' about the 'new' reproductive technologies?" In Man-Made Women. How Reproductive Technologies Affect Women, G. Corea et al. (London: Hutchinson, 1985).

17) The study of Anke Erhrardt and Heino Meyer-Bahlburg, "Effects of Pre-Natal Sex Hormones on Gender-related Behavior", Science 211 (1981) is cited as a case in point.

18) They indeed maintain that language alone does not create the object; it might, however, misdescribe it, even to the point of occasionally obscuring reality. Their position is thereby less radical than that of Haraway (presented in this chapter), for example.

19) See chapter 4 for more detail about these controversies.

CHAPTER 2

FEMINIST EPISTEMOLOGY AND THE ELABORATION OF A NEW SCIENTIFIC PRACTICE IN BIOLOGY

Feminist criticisms of biology have spurred the elaboration of a feminist alternative research programme not only in the social sciences, but for science as a whole.

A question which looms large in that connection is: What needs to be changed in scientific practice for it to be more congenial to non-sexist, 'truer' biological theories? Do we merely need organizational adjustments in schools and higher education? Or is it necessary (in order to introduce these adjustments) to raise women's consciousness of patriarchal biases in society and knowledge at large? And what if more profound changes in the rules of scientific method were the prime conditions for the development of a feminist biology?

Mainly inspired by schools of thought arising from a post-positivism sociology of - knowledge, feminist theoreticians and epistemologists started asking the far-reaching question: Do we need a feminist epistemology to vindicate a feminist, non-sexist, and non-patriarchal science? Do we need a feminist society? (Fee 1983, 1985; Flax 1983, 1987; Harding 1986b; Harding and Hintikka 1983; Keller 1982, 1985; H. Rose 1983, 1985)[1]

Hitherto in this thesis, we have seen the reasons feminists isolate as a rationale for changing certain theoretical categories, hypotheses, and research designs in the practice of biology. These reasons seemed to be justified and legitimate. But I have also highlighted some contradictions and practical problems with which feminists have been confronted in the elaboration of their new research programmes. First, their critique of a realist epistemology for science was not congruent with how they viewed the validation of their own biological theories. Secondly, feminists seemed to maintain that in studies of biology and behaviour, (patriarchal) biological determinism could be rejected on empirical grounds;

but they could only contend that their own holistic approach should be taken for granted -- for no better reason than that there is no conclusive evidence for biological reductionism! This last argument indeed sounds more argumentative than strictly empirically-grounded; it resembles a kind of "negative endorser" theory.

This chapter will thus try to shed some light on the gaps between the aims of feminist critics of biology and their suggestions for the implantation of new norms of scientific practice, including a rethinking of its epistemological premises. It will argue that these theoretical suggestions do not render total justice to feminist biology and its potential as a 'better' science. Rather, the epistemological canons of the empirico-analytical sciences as formulated in critical realism seem to serve more satisfactorily the purposes of concrete projects of feminist biology.

The first two sections will deal with the intellectual legacy of phenomenology, discussing, first, the Marxist feminist view on epistemology and, secondly, the feminist psycho-sociological insights into the scientific system. The last section will examine, and endeavour to explain (in light of the aims and scope of feminist projects of science) the unfolding of recent debates within feminism in relation to the epistemological categories of objectivity, subjectivity, and values.

Within these three tendencies of feminist theories of scientific knowledge, the problematic role of women in the elaboration of a feminist biology is being discussed. We shall note, for instance, that the role of women and of 'feminine values' is always being articulated in combination with the 'vanguard' programme of action of feminists and political consciousness-raising. Also, the articulation of the 'feminine' and the 'feminist' is emphasized more or less strongly by the feminist authors working within these diverse traditions.

The notion of 'feminine values', however, recovers diverse sociological components of social action that need to be distinguished. These components may reflect, for instance, the objective social interests of women (in relation to the health system or the household for example). Or they may correspond to the 'caring' and 'loving inclinations' of women as a social group. Or else they may mirror the professional interests women are likely to defend within the scientific institution. More importantly, the point to make with respect to 'feminine' and 'feminist' values is whether the social 'motives' they represent can satisfactorily replace epistemological 'guidelines' in the construction of a science less sexist and more conducive to a complete understanding of women's biology and health.

I would suggest that feminist attempts to build a "better" science incorrectly claim new epistemological credentials. However, "feminist biology" might rightly appropriate the status of original and anti-conventional discourse about human biology. As shown in chapter 1, the specificity of feminist biology seems to rest on the following tenets: a distinctive research agenda for human biology; a set of alternative theories about sex-gender differentiation and behaviours; and the usage of certain techniques of data collection (borrowed from the human and social sciences) complementing, though not replacing, conventional methods of investigation and validation in biology.

The Legacy of Marxist Phenomenology in Feminist Critiques of Science

Several feminists have argued that if the canons of scientific epistemology had been 'thought out' via the social practice of women, that is, on the basis of feminine 'emotional inclinations' towards, and 'empathy' for, people and their personal experiences, these canons would have secured the grounds for a "better", less sexist, racist or classist, science. In order to become a reality today, the constitution of such a 'feminist epistemology' would, however, necessitate a certain degree of political awareness and militancy on the part of women scientists, a process involving the assistance of a 'feminist vanguard' of women in science. This view on scientific changes seems to have been profoundly influenced by the Marxist theory of knowledge, whereby the location of actors within the social structure gives them a different "worldview" of social life (Gramsci 1985; Larrain

1983; Mannheim 1936, 1952).

According to early Marxist feminist writings about science (Arditti 1980; Hubbard 1979; Lowe and Hubbard 1983; H. Rose 1983, 1985), a "feminist" science based on "feminine" values would be <u>better</u>, because historically, women's social role and practice have not been conducive to the domination of nature and social control, but rather to a respectful and caring attitude towards the natural environment and human beings. A feminist science would also be <u>truer</u> because, as a dominated group, women might attain a more comprehensive understanding of the world (Hartsock 1983; Harding 1983).

The Marxist-inspired project of a feminist science is fully described in the works of the social scientist Hilary Rose (H. Rose 1983, 1985, 1986; Rose and Hanmer 1976; Rose and Rose 1976a, 1976b, 1976c, 1976d). Rose looks at science from the perspective of a social practice taking shape as part of a social totality. In this view, science is seen as an ideological weapon for the maintenance of the social order of Capital, patriarchy and the white race. In contrast, feminist science would rest on the ideology of the socially dominated practice of women.

As Rose argues, women occupy a dominated position in society, both ideologically and materially. Ideologically, reproductive labour is undervalued; productive labour is more highly regarded. Feminist Marxists stress that reproductive labour generates a totally distinct type of social relations, which is, nevertheless, most important for the maintenance of society. Reproductive labour involves caring for the people, nurturing children, unselfish patterns of behaviour and love. Productive labour, in contrast, fosters control over human and material resources, a control enforced by coercive organizations of social relations. The social relations generated by productive labour are, in their current form, diametrically opposed to the values of love and emancipation (H. Rose 1983, 1985).

Relative to science, more specifically, Rose suggests that the history of women's social practice under capitalism reveals why the act of any woman appropriating scientific practice has been regarded as a contradiction. She holds that, under capitalism, any woman would find that "her involvement with the abstraction of scientific practice as it has developed under capitalism and patriarchy, on one hand, is in painful contradiction with her caring labor on the other" (1983, 87).

Rose therefore proposes a new feminist epistemology for science, integrating manual and intellectual labour, by means of "caring for" and "feeling for" the objects being studied. She also posits that a feminist methodology would seek to bring together the subjective and the objective. Feminist epistemology, she claims, involves a reintegration of the "hand, brain, and heart" for the natural sciences (H. Rose 1983). Such an epistemology, she maintains, has already provided science with the existing feminist critiques of biology and medicine, which have in turn brought about "a more complete materialism, a truer knowledge" (ibid., 72). But the entrenchment of 'feminine values' within scientific norms of practice would most surely need the active participation of a feminist vanguard. For only militant action could sustain the difficult challenge of implanting 'values' and a 'style of practice' traditionally alien to science. Finally, this double process of activating 'feminine values' and 'feminist politics' would generate the 'feminist standpoint' on knowledge, whose product, far from being biased, would be a 'more complete' understanding of human life.

Thus, according to Marxist feminism[2], feminine practice is more likely than male practice to reach true knowledge. By reason of their location within the social structure of patriarchy, women as a group might have a more encompassing vision of the world, and, as reproductive labourers, hold a more caring attitude towards social relations and relations of knowledge between knower and the objects and people to be known.

This point of view is more fully developed by Arditti, a feminist Marxist biologist. Arditti defends a project of feminist science in which she accords women a major role (1980). She contends that the potential of feminism to develop a "truly humane science" rests on the fact that "women are more generally in touch with their feelings" (ibid., 364). Therefore, a feminist perspective would give "the prevalent mode of science" human concerns. Feminist science would provide a "special form of knowing" avoiding the traditional division between intellect and emotions. It could "re-legitimize the intuitive approach" which is being consistently undervalued in current science (ibid., 364-66). For Arditti however, while both men and women scientists may be committed to the conversion of science from prevalent norms to a liberating and healthy activity, yet, it is the women who are more likely to be at the forefront of this project because they are more in touch with the values of love and respect.

The idea of a better science embracing humanist goals was, in early feminist writings about science, taken on to justify the union of feminine subjectivity and scientific objectivity as the epistemological foundation of feminist science. The works of some feminist epistemologists and historians of science (Keller 1983, 1985; Merchant 1982; Oakley 1981) illustrate this tendency. If the idea of subjectivity has a legitimate place in the philosophy and the sociology of science; for both epistemological and ethical reasons, however, its importance within the scientific process as a whole is highly questionable. Even within feminist epistemology and the history of science, many authors are reticent to accord a central role to subjectivity, let alone "emotions", in the development of a non-patriarchal and non-sexist science. This is true even though they may acknowledge the utility of interactive methods for certain research questions (in human studies for instance) (Kirkup 1986; Koblitz 1987; Jaggar 1989; Hawkesworth 1989; Mura 1989; Stacey 1988; Tronto 1989)[3].

Since the roles of feminine subjectivities and emotions in the development of a "better" scientific process is far from settled, the role of women in the elaboration of a feminist science is also problematic. The idea that women as a group can foster a new scientific approach is counteracted by the evidence that feminists (scientists and non-scientists), more than women scientists, have been at the forefront of reforms in the research programmes of biology in the interests of women. It is, in fact, noteworthy that several women biologists do not actually identify with all or even some aspects of the so-called "feminine practice", but rather seem to endorse "masculine" attitudes of intellectual authority and competition.

The Marxist feminist biochemist Hubbard, for example, does not agree totally with the idea that women's main contribution to science is their "feminine" attitudes towards nature and people. Her point of view suggests, rather, that women, as a group, because they have been the victims of sexist science and medical malpractice, have an objective interest in a feminist reform of biology (Lowe and Hubbard 1983). Also, she does not necessarily give women a prominent role in the social and ethical reforms of scientific practice. She seems to argue that it is mainly social critics, feminists included, who will, and ought to, participate in the process of changing science for the benefit of all. More substantively, Hubbard subscribes to a Marxist view on biology which reinstates a less rigid view of explanations about "nature", a total (yet self-reflecting) dedication to human welfare, and opens up scientific thinking to a full re-examination of its assumptions and procedures of research.

In her introduction to H. Rose and S. Rose's collection of essays in <u>Ideology in/of the Natural Sciences</u> (Hubbard 1980), Hubbard develops her argument for a new science on three pivotal issues relating to traditional science. First, scientific language endows knowledge with depersonalization, and reifies the idea that science and scientists are free from political biases. Secondly, science treats reality as decontextualized facts rather than as something taking its full identity within a context. Such a view implies that current "scientific methodology" only permits the capture of natural phenomena which are repeatab-le and measurable. It cannot deal with unique occurrences, or with interactive systems (see also Hubbard 1982). Thirdly, in the present social climate, science "is flawed with arrogance": scientists try to enforce <u>uncritically</u> their framework onto objects and phenomena even though it is improper to describe the reality under study in all its complexity (Hubbard 1980).

According to Hubbard, science has, for centuries, built on these three pivotal norms.

Conversely, in a scientific practice reformed on Marxist feminist bases, scientists would be led to conceive of "nature" as a complex system of interactive parts, a conception which constitutes the most accurate picture of life from which models of explanation can be developed (ibid.). (Such a conception also implies that 'women's biology is not their destiny'.)

Therefore, Hubbard continues, in order to solve the problems of science, we need more than simply to ward off the negative results of science from within; we need also to criticize it from outside; we urgently need to "redefine what science is about and how to do it less wastefully, and more healthily and humanely" (ibid., xxv); and, as far as scientists are concerned, to "redefine science and its methodologies out of a full awareness of the ideological components that are implicit in it" (ibid., xxvi).

In her discussion of biology and women, more particularly (Hubbard 1979; Lowe and Hubbard 1983), Hubbard opposes the naturalistic fallacy of biological determinations, in favour of a more holistic approach. She says, "there is no such thing as human biology in the pure ... what we think of women's biology is a political construct, not a scientific one" (Lowe and Hubbard 1983, 6). Secondly, she posits that if women themselves were to ask biological questions, they would likely obtain more reliable information about women's biology than that which conventional biology has thus far produced. She believes that, in spite of the economic and political constraints imposed on women and feminists by the patriarchal system, it is possible for <u>women</u> to ask the <u>questions</u> that are <u>of interest to them</u>.

Hubbard thus clearly gives a prominent role to women in the re-evaluation of traditional explanations of women's biology and behaviour. She does so however, assuming women's personal interests in, and immediate knowledge of, their own bodies and malaises, and without giving too much weight to the idea of intrinsic 'feminine values'. For these reasons women would reinstate a more 'woman-friendly', and less sexist biological programme of research about sex and behaviour. But she also accords, with regard to the implementation of these profound reforms, a role of leadership to feminist and other socially-involved groups. In the elaboration of a new type of biology, one must confront, she contends, political and economic interest groups. So, she does not conceive of feminist biology as purely rooted in women's subjectivities; she would rather argue, in the main, for a Marxist feminist biology committed to tackling the political and rational debates lying ahead -- regarding research approaches, ethical problems, and social interests in science.

Hubbard's substantive critique of scientific activity in a capitalist and patriarchal society appears to have been taken on by several feminist authors who have studied the relationship between science as a proto-universal epistemology and science as a power system vested with men's interests. The writings of Fee (1983, 1985) are illuminating in that respect for they illustrate the scope of the feminist projects of science, and display clearly the amplitude of their theoretical shortcomings. Let us look briefly at Fee's arguments in order to highlight the problems which current feminist theoreticians are trying to resolve (or to avoid) in the elaboration of a feminist biology.

Like Rose and Arditti, Fee believes that "there is something unfeminine about science" (Fee 1983). For her, masculinity is an incomplete and distorted form of humanity. She claims that the issue for feminists is not making women more scientific but rather making science less masculine and thus more completely human. Science must indeed be transformed to permit the acceptance of women within it, but more importantly to "conceptualize new kinds of relationships between human beings and the natural world, by overcoming an alienation between culture and nature" (ibid., 15).

The core of Fee's thesis lies in her discussion of the notion of objectivity. She concurs with Hubbard's analysis, holding that the notion of scientific objectivity is sufficiently vague to hide its real political purpose and to keep its legitimacy under the guise of epistemological virtues. She contends that science is permeated by social values, more precisely, masculine values.

Fee concentrates, in the main, on the negative political effects of pseudo-objectivity and she does not, therefore, examine further the types of reforms suggested in Hubbard's earlier works and which required development. By the same token, Fee avoids substantive analysis of the question which, in my view, is crucial for feminist biology: Is it, epistemologically, absolutely necessary to change the scientific rules of proof or any other methodological categories in order to have a feminist science?

For example, part of Fee's discussion on objectivity concerns the relationship between thinking and feeling, and between values and objectivity. First, Fee says that the absence of feeling and emotion in science is the direct result of the domination and political authority of men. This is, in my view, at best sketchy, at worst presumptuous.

Secondly, she analyses "emotions" and "social values" as if they were co-extensive epistemological categories. We must underline that "emotions" are simply "felt". If they can be raised as evidence to support observation and cognitive judgements, they are not debatable as such: they simply "are" or "are not". In contrast, "values" are cognitive categories assuming the roles of either premises or conclusions in the process of knowledge; thus, they can be rationally discussed as such[4]. Fee distinguishes neither between values which predicate cognitive judgments, nor between values which predicate judgments of social ethics. While she urges scientists to recognize the existence of values in science, she does not even commence examining the basic practicalities of how the social resolution of ethical and social issues, and the procedures of scientific research, could be organized for the sake of a democratization and an amelioration of decision-making process in science. From a sociological point of view, values entering model-building seem far less conflictual than values related to ethics, even from the perspective of feminist critiques of science. In fact, feminists do not seem so much to challenge the <u>idea</u> of a pragmatic, instrumental type of knowledge (referring primarily to instrumentally-oriented cognitive judgments), as the <u>abuse</u> of instrumental knowledge (referring primarily to matters of politics and social ethics) in areas like human biology, pharmacology, psychology, and industrial "progress". (See chapters 3 and 4.)

Indeed, in spite of all her criticisms, Fee maintains that science should not be rejected wholesale; she states that rationality, critical evaluation, and empirical testing must be preserved. She does not explain, however, why she suddenly takes this point of view. For her, there is only one way to preserve the promise of scientific progress: it is to base science on individual creativity, stimulated by, but also ultimately subjected to, the constraints of community consensus through a set of recognized procedures (Fee 1983). In my opinion, this statement demonstrates at least two key things: first that Fee cannot really provide any new basis from which to develop a feminist epistemology; and, secondly, that she starts from a shaky theoretical basis to assess workable solutions to the problem of democratizing science[5].

Scientific research is oriented by dominant social groups, and a majority of scientists would acknowledge this. But this does not prohibit them from thinking that the method itself will prevent errors supplanting truths. (See chapter 6). In her critique of scientific objectivity, Fee is right in pointing out this fact. It may be argued, however, that she overstates her views, and criticizes the abuse of scientific rationality in other spheres of social activity, rather than the epistemological utility of rationality. In other words, she focuses on the 'abuse' of science rather than on its 'use'. She also coalesces political and epistemological questions into that of "values", an inexcusable adumbration at the expense of a sound analysis of science as a rational process. She does so, risking the fact that her critical argument will not even be taken seriously by self-critical biologists.

It is my contention that, like other Marxist feminists, Fee does not investigate if some useful components of the scientific method should be kept. She also does not explore either how a social consensus on scientific validity could be reached. Fee's view of a feminist science remains merely a "negative-endorser" project of science[6]. In other words, once Fee has pointed out the basic sexist flaws in scientific practice, especially its current use of objectivity for ideological (patriarchal) purposes, she does not try to discuss either substantively or more comprehensively the epistemological and practical implications of her approach to science.

True, Fee claims that what she has developed appears more as a feminist critique than as a feminist project of science. She indicates that "there is no way of imagining in advance, a fully articulated scientific theory" (1983, 22). Yet she holds that "we are, however, free to play with ideas and to consider the criteria that a feminist science should fulfil" (ibid.)[7].

> At this point, while it is necessary to argue the case for the entrance of women into the scientific professions as presently constituted, it is also important to push the epistemological critique of science to the point where we can begin to construct a clear vision of alternate ways of creating knowledge. ... Overcoming the dualisms that feminists have identified as being associated with sexual dichotomies, such as the subject/ object relation, may offer the prospect of a radically transformed science, one that is as yet only faintly visible as a possibility for the future. (ibid., 24-25)

Let us summarize the foregoing arguments in this section. The Marxist view of the 'socio-epistemological' problematic of relations of values and facts has enabled feminists to produce a powerful critique of the patriarchal ideology prevailing in biology. Accordingly, feminist science would neither hide, nor renounce its political aims on behalf of women, but on the contrary, would work with a full awareness of its political biases. By overstating the issue of social values, more especially in terms of masculine versus feminine values, in science however, feminists were bound to face serious problems in building a new scientific method. By seeing the role of scientific values strictly in terms of ideological weapons for patriarchy or, in contrast, of moral and ethical prerogatives of women and 'feminine inclinations', they too hastily dismissed the valuable role some values also play in the development of procedures and methods of validation in the empirico-analytical sciences, for the benefit of both feminist and conventional biologies.

Finally, as will be argued in chapter 4, an examination of biological epistemology tends to show that, as biology concerns itself with life in general, the values entering its model-building have been, as in the social sciences, more directly subjected to social debates than the values entering model-building in the physical sciences[8]. In contrast, biology has remained closer to the physical sciences, both experimentally and with respect to the logic of scientific proof entrenched within critical realism. As I shall develop these two points in a further chapter, new light may be shed on the reasons why feminists, borrowing their views on biology mainly from critical social theory, have had troubles justifying themselves within the confines of experimental biology.

History of Science, Psycho-sociology of Science, and the Impact of Institutional Changes

Several feminist historians, sociologists, and practising scientists have suggested that it is the social setting and the historical weight of androcentric representations of scientists which are the deep-seated reasons for the relative absence, poorer performance and lower status achievement of women in science. They have tried to demonstrate that in school science (Bentley and Watts 1986; Kahle, 1985; Kelly 1985; Smail, Whyte and Kelly 1982), in higher education (Brighton Women and Science Group 1980; Kahle 1985; Rossiter 1982; Rosser 1985), and in professional circles (Koblitz 1987; Reskin 1978; Outram 1987; Rosenberg 1982; Simeone 1987; Widnall 1988), the conditions of the learning, and the performance, of science were unfavourable to females[9]. These authors seem, therefore, to adhere to a theory of knowledge that Harding has rightly called 'feminist empiricism'. For they do not assume that the sexist flaws of current science lie in its epistemological 'norms' or methodological tools. Rather they consider as their main subject of investigation the prejudices pervading theory-building and research agendas, whose infiltration, they hypothesize, is primarily due to an institutional imbalance of power between men and women scientists at both micro and macro- levels of decision-making.

The bulk of recent feminist reflections on science rests on the concept of science as an educational and professional system inseparable from a more general division of labour, itself maintained through representations of gendered roles. According to a great many feminist historians of science, the scientific culture and infrastructure have greatly contributed to maintain the image of scientist as being male, and scientific work as being more suited to the aptitudes of men. These are proposed to explain the proletarianization of women in science, their exclusion as full-fledged scientists, and their relative absence from science[10].

Some feminists, however, also explain that these historical processes underwrite the idea that feminine practice (whether conscious or not, repressed or not) is totally different from masculine practice. This implies that womens' approach to nature, people and knowledge, is intrinsically distinct from that of men. The association of males with scientific activity can be interpreted as either a conscious political process excluding females, or on the basis that, more implicitly, the scientific method and model-building take their deeper roots in the male psycho-cognitive pattern (Flax 1983; J. Harding 1986; Keller 1982, 1985; Sayers 1987). These two processes combined might explain more fully why, historically, women have achieved less impressively in science than men.

Thus, this perspective on the problem of women in science addresses head on the deep roots cum social origins of scientific epistemology, a contrast with the avenues explored by the authors referred to previously in this section. In fact, this perspective sets itself the task of testing the theoretical underpinnings of the 'feminist standpoint' on scientific knowledge against the micro-structure of scientific practice, an enterprise which contrasts, this time, with that of the exploration of the macro-levels of science characteristic of Marxist-inspired feminist studies.

According to the feminist authors subscribing to this view, the values entrenched in scientific knowledge are not universal. They are, in fact, co-extensive with male cognitive psychology. Scientific endeavour is shaped in accordance with men's goals, desires and representations of nature and the social world. These are, in the main, to dominate, predict, and control nature (and women); to conceive of people and nature as objects; and to elaborate models of explanation as mechanistic metaphors. Such "cognitive" values and interests are indeed closer to the typical psychological development of boys than of girls, the former detaching themselves from their parents earlier and with less emotional turmoil. As adults, males are more inclined to sever emotion and reason, to distance their own identity from that of other people, and to become more aggressive. Females, on the other hand, have more difficulty separating their own identity from that of others, and also to separate emotions from reason. The female pattern of psychological development is said to induce the contradictions women must experience when they attend science classes, undertake experiments, try to learn abstract concepts, and strive to advance in science. These contradictions are exacerbated by social upbringing, role expectations, and the settings of science education and scientific work.

Thus, according to this argument, the 'exclusion' of women from science has less to do with women's nature than with the phenomenological genesis of scientific knowledge. Given the historical backgrounds of our societies, the genesis of scientific knowledge has partaken of the socio-psychological dichotomies male/ female, objectivity/ subjectivity, rationality/ emotivity.

Phenomenology as the study of "forms of life" has, indeed, been very influential for feminist literature about knowledge[11]. Phenomenology states the 'dependence' of our mental representations of nature and reality on the 'relative' systems of beliefs in which they take place (Berger and Luckmann 1967; Schutz 1962). Since phenomenology posits a relativist stance on belief systems, it justifies, as an epistemology, the existence of alternative "worldviews": all beliefs are true within their own contexts. The works of Glennon (1979), Spender (1980, 1983), and Stanley and Wise (1983), for instance, were strongly committed to phenomenology. However, as other feminists have argued, phenomenology somehow undercuts the political rationale of feminist critics of science: for it undermines the real, objective, existence of a social structure which oppresses women (Currie and Kazi 1987).

The feminist arguments in favour of a phenomenological insight into female psycho-sociology and female experiences have been criticized in recent works of feminist educators, historians of science and epistemologists. The issue raised by these authors was whether feminist scientists and critics of science, in their attempts to alter scientific knowledge in favour of a more equitable treatment of women, should be mainly concerned with substantive theory-building in political and sociological studies of school science education and the scientific professions, or if they should proceed by examining epistemological matters. Let us look at some of these studies in order to highlight the directions they have taken, and why.

An important part of the literature about women in science has paid particular attention to the evolution of women's consciousness of covert sexist biases in science[12]. Several of these studies assign a major role to teachers and professors of science in the transformation of the science curriculum, the image of scientists, and the consciousness-raising of students (Andersen 1987; Bentley and Watts 1986; Kahle 1985; Kelly 1985; Schuster and Van Dyne 1984; Rosser 1985). Others endeavour to document how, historically, the scientific institution has systematically undermined the role of women in science (Ginzberg 1987; Hearn 1982; Koblitz 1987; Merchant 1982; Outram 1987; Rosenberg 1982; Rossiter 1982).

The strategy described in Schuster & Van Dyne (1984), for example, primarily concerns the liberal arts, but it is widely quoted in feminist literature about science. They describe the introduction of feminist theory-building in the human sciences in a six-step strategy of consciousness-raising of women students. In their view, the emergence of women's studies programmes in the curriculum should spur the inclusion of women's experiences not only as problems on their own, but as a relevant basis for a new understanding of social affairs. Schuster and Van Dyne acknowledge several sources of resistance to such transformation. These are the weight of invisible paradigms on the belief that science is neutral and universal; the students themselves, who might not have experienced the adverse effects of sexism, or who might believe that an equal opportunities movement would suffice to balance the previous absence of women from prestigious positions in social life; the multicultural realities which could impede a project embracing women's experiences as a whole; and the loss of old certainties in favour of a plurality of accounts of history and society.

Although Schuster and Van Dyne's account may appear legitimate for the social sciences, one may also wish to ask if it could be applied directly to biological science.

Andersen (1987) remarks that feminist transformations in science have already borne fruit in disciplines where interpretative methods are used. In these areas, she argues, feminist critiques will be fruitful inasmuch as they make a breach in the hegemony of science, and replace it with a pluralistic view, inclusive of women's subjective experiences. In the case of science and technology, however, she says that the main contribution of "a feminist view of science" is still to come. She suggests that, mainly, feminism will continue to repudiate the cultural dualisms associated with masculinity and feminity which have permeated scientific thought and discourse. Andersen, thus, believes that "it is impossible... to move directly from the male-centred curriculum to [a] 'transformation' of that curriculum in favour of a co-educational one -- without passing through some form of women's studies" (ibid., 226)[13]. Her position lends itself to the tenets of a project of feminist biology presented earlier, via the works of biologists, such as Fausto-Sterling, Messing and Hubbard. That is to say, she is very critical of prejudices governing the selection of scientific data and the formulation of research problems. Still, she seems to lean towards a realist epistemology which would give us at least some minimal guarantee that 'feminist biology' provides 'better' and 'truer' explanations than 'patriarchal science'.

Rosser (1985) takes a much more radical view of a feminist science curriculum. Adapting Schuster and Van Dyne's framework, she is fully aware that women scientists are, in general, oblivious to the implicit sexist biases in science, and argues that unless women scientist's <u>personal lives</u> are directly concerned with feminist issues, they would be unlikely to generate changes in teaching science or in research[14]. She maintains however, that real changes in the science curriculum might well involve more than that. These changes would imply a recognition by women, of the rigidity and inaccuracies of knowledge about women, and of the male biases concealed in the notions of objectivity, rationality and dominance; they would also focus on the importance of introducing feminine attributes expressed via their subjectivities, feelings, and caring attitudes into science; and, finally, the possibility for more than one way of practising science to co-exist with that presently normative.

Similarly, Bentley and Watts (1986) have argued that the nature of normal science is masculine because it reflects male values such as rationality, logic, objectification, and quantification. For them, female values are holistic, co-operative, amenable to diverse forms of knowledge, qualitative, and as such, they adumbrate the unification of the intellect with emotions. Bentley and Watts also believe that a feminine practice of science would not only produce a better, more humane and more self-conscious, scientific practice, but they also believe it would generate a superior method and a more comprehensive epistemology for scientific knowledge.

In contrast, Jaeger (1987), for example, severely criticizes authors such as Bentley and Watts. First, she argues that if feminists think women can create a new science based on feminine values <u>cum</u> 'emotional inclinations' towards nature and human beings, they must initially thoroughly investigate the relation between emotion and cognition in the thinking subject. But they fail to do so. For that reason, Jaeger contends feminist epistemology can be rejected as an alternative theory of knowledge.

Jaeger also highlights the confusion between the social and the natural sciences created by feminist theories of knowledge (ibid.). She suggests that 'feminine values' <u>cum</u> 'life experiences' might be relevant in understanding the subject matters of the social sciences (i.e., human and gendered experiences). She also argues, however, that these values are rather useless in understanding the object matters of the natural sciences (i.e., inanimate objects). Finally, she contends that "traditional science" already subscribes to some of the principles advanced by feminists, such as, uncertainty about our explanations of nature, inability to completely control natural events experimentally, recognition of biases in observation, and the holistic view of nature and reality[15].

The only aspect of science on which Jaeger's critique converges with that of feminists is in the realm of school education. She maintains that school science must be altered in order to be more congenial to girls, that is to say, to sustain their curiosity, to develop their skills, and to meet their expectations[16]. But in the last analysis, Jaeger does not believe that the root of the problem of women's low participation in science lies in the fact that the "nature" of science is intrinsically "masculine".

Like some feminist historians (Koblitz 1987; Outram 1987) and educators (Kirkup 1986), Jaeger is more inclined to withdraw from the traditional view of a feminist science based on feminine values. Likewise, Outram (1987) suggests to feminists that they concentrate on the history of the professionalization of science rather than on philosophy. She argues that the institution, more than the deep-rooted assumptions of scientific epistemology, has precluded the full participation of women in science. Historical accounts indeed show that in the past, numbers of women used to be involved in scientific work, but only as assistants to their relatives (Alic 1986; Gostzinyi-Ainley 1986; Rosenberg 1982; Schiebinger 1987); these accounts alternatively show that a great many, as far back as a century ago, proceeded to higher education, although mainly in women's colleges (Rossiter 1982; Cott 1986; Delamont 1989). Thus these accounts indicate that the professionalization of science was far more favourable to men; the best jobs became a male preserve, and tenured positions were almost inobtainable for women. The sole career niches available to women were the assistantships, temporary fellowships, and research positions in less prestigious research areas such as home economics and botany.

Finally, Koblitz (1987) also opposes the feminist thesis positing that women were prevented from working as scientists mainly because the scientific method is inherently male[17]. She maintains that "the feminist question in science" should be "concerned less with any abstract concept of gender than with overt and covert sexual discrimination, social expectations and the socio-political atmosphere" (Koblitz 1987, 403). She judges the idea of a feminist epistemology based on feminine values as shortsighted, even caricatural. She also indicates that feminists have been wrong in thinking that the "feminine" insight, as it might be used in some areas of biology, could be generalized to the whole of science. She finally maintains that recourse to intuition and "quasi mystical" scientific insights is not a female preserve, but relatively common in stories about renowned scientists, both male and female[18].

As previously discussed, the standpoint on feminine values and scientific objectivity held by some feminists is at best controversial, at worst shaky. Recent debates among feminists indeed show that they have reoriented their reflections and works from the perspective of an original contribution to scientific epistemology, to a more pragmatic reform, yet original in its theory-building, of biological knowledge about women. The last section of this chapter will show that recent feminist reflections on science have indeed shifted from the question of a new epistemology to the question of new research agendas. At the turn of the 1980s, feminine practice was posited as able to engender new methodological and epistemological rules. Ten years later however, realizing that this argument was empirically and theoretically unsustainable, several feminists are now undertaking to redefine the parameters of a feminist project for the natural sciences.

'The Science Question in Feminism': A Question of Method or a New Research Agenda?

Feminist reflection about science has made a major shift over the last years: instead of focusing on methodology or epistemology, it has concentrated its efforts in exploring science as a gender-system (or culture) which excludes women scientists and questions of interest for women. The works of Keller and Harding, two authors frequently quoted in feminist critiques about science, are two prime examples. It is suggested that this recent shift in feminism may increase the credibility of feminist attempts to change biology from within, and avoid the epistemological pitfalls which have somehow undermined the credibility of legitimate sociological concerns defended in projects of feminist biology.

Let us first give an example of the kind of arguments which has left feminist critics with unresolved epistemological questions relating to the validity of feminist biological theories. The essays of Rosser, a zoologist, illustrate well the predicament with which feminists have been confronted.

In a recent article (1988b), Rosser addresses the legitimacy problem involved in the full realization of feminist science. She questions whether "good science" could ever be value-free, least of all gender-free. She contends that traditional biology has been justly criticized for its reductionist and patriarchal approach; and she subscribes to a holistic approach in which biology and environment will posit the inclusion of cultural factors in explanations of women's behaviour and aptitudes. With all these arguments, we would concur. She also claims, with other feminist biologists, that "we can never know whether or not there are real biological differences between males and females because we can never separate the biological from the environmental" (ibid., 16). This argument, as discussed earlier, is questionable. Finally, Rosser holds that science is, ultimately, value-laden; therefore, she is forced to admit that even a potential feminist science is value-laden (ibid.). But this does not necessarily mean, as Rosser may incorrectly conclude, that it is impossible to legitimize feminist science on the ground that it is truer than any other kind of science.

In our view Rosser's argument is symptomatic of the relativist stance on knowledge underlying feminist theories of knowledge inherited from the phenomenological tradition. According to these theories, the inescapable role of values in the construction of knowledge implies that any type of knowledge, science included, is context-bound, rather than universal, and that it can only be judged according to the cultural norms of the society which nurtured it (Barnes 1974; Barnes and Bloor 1982; Bloor 1976; Mulkay 1979). The flaw in some feminist theories of knowledge, is to feel compelled to reject scientific validity altogether (on the ground that science is ultimately value-laden), instead of exploring social norms and epistemological rules which may justify it within our present scientific context.

Keller's view of science is not primarily inimical to scientific endeavour and the epistemological features of objectivity and reason. Her arguments focus mainly on these features as they have been culturally and institutionally associated with men and dissociated from women (Keller 1985, 1987b). This would explain why women have found it more difficult to obtain scientific jobs than men, and why they seemed to have performed less well in mathematics and science (1982). In the feminist literature, Keller seems to adumbrate the progressive disentanglement of the "feminine" and the "feminist" arguments in feminist theories of science (Keller 1983, 1987a)[19]. She distinguishes these two arguments, but maintains that both criss-cross the whole gender system of science.

The gender system of science encompasses the following social processes: education and psychological development, the workplace, decision-making about research agendas, and finally the socio-historical construction of epistemological assumptions and the rules of scientific method (1987b).

In her article "Feminism and Science" (1982), Keller contended that feminists should pay particular attention to the historical unfolding of objective knowledge, male psychological development, and the ideologies of power and domination. She suggested that knowledge in general, and science in particular, serve two gods: on the one hand, power and transcendence over nature, on the other hand, a certain view of domination, namely the male dominating the female. According to Keller, the view that knowledge could arise from a "conversing" between scientific thought and nature has always been undermined by this two-fold process (see also Merchant 1982). Drawing from the object-relation theory of Winnicott and Klein (see also Flax 1983; J. Sayers 1985) Keller tried to buttress the idea that women might have developed a different attitude towards objectivity, the relations between nature, society, and science, because they prefer the understanding side of science rather than its impulse to control. In this connection, Keller wrote: "In the historical effort feminists can bring a whole new range of sensitivities, leading to an equally new consciousness of the potentialities lying latent in the scientific project" (1982, 602). She concluded that feminist critiques must extend to the foundations of scientific thought and distinguish "the objective effort from the objective illusion" which, in the current state of science, are features that "belie its claim to universality" (ibid., 594). She summarized her view as follows:

> In short, rather than abandon the quintessentially human effort to understand the world in rational terms, we need to refine that effort. To do this, we need to add to the familiar methods of rational and empirical inquiry the additional process of critical self-reflection. (ibid.)

In 1983, Keller wrote Life and Work of Barbara McClintock, about the eminent biologist who won a Nobel Prize in the 1980s. It is undoubtedly this book which spurred controversies and confrontation among feminists about the "feminine" and the "feminist", in which one of Keller's contentions was that "love and feeling" were typical of McClintock's practice and of her scientific reputation. As a result, many feminists believed she was giving empirical evidence for a feminist epistemology based on feminine practice.

Keller has clarified her point of view in recent works (1987a, 1987b). She stressed that feminist science does not aim to replace science completely, but only to render explicit the masculine ideology shaping the scientific understanding of nature and objects in general. Shying away from the idea of a "feminine science", Keller started arguing more emphatically for a "feminist science" as the standpoint from which a critique of scientific scrutiny -- qua androcentric -- might be conducted. She stressed that the aim of feminist science is to uncover the masculine biases which still remain in science, through the "terminology" describing nature and women, and through the patriarchal interests it implicitly defends[20].

In "The Gender Science System" (1987b) Keller maintained that the story of McClintock is a prima facie case of how the science system treats women. McClintock was seen as a marginal woman and an eccentric scientist: her scientific work was received reluctantly. The way in which McClintock's work was "re-habilitated" by the science-system also illustrates the way feminism is treated by scientists. Keller pointed out that feminists were among the first to view McClintock's work and approach to biology as sound; but it was only in retrospect, after McClintock's "jumping-gene theory" was finally considered of prime importance by traditional biologists, that her "style" of practising science was finally recognized as valid by the scientific establishment.

In short, Keller does not pretend that the feminine ethos is epistemologically qualified as an alternative scientific approach. She merely argues that the feminine relation to objects and women's marginal position within the science-system might both have helped to produce certain cognitive patterns which could capture more adequately certain experiences in the natural world -- as in the case of McClintock. She considers feminist science as a critical theory rather than as a comprehensive alternative to current norms of scientific practice. Feminist science is a systematic appeal to the fusion of beliefs and cognition, facts and values, evidence and subjectivity, all of which should, ultimately, help to dissolve the dichotomy feminine practice/ science practice which has historically shaped the practice of science and excluded women.

> The question at issue, finally, has to do with the meaning of science. Although we may now see that science does not simply 'mirror' nature, to say instead that it mirrors culture (or 'interests') is to make a mockery of the commitment to the pursuit of the reliable knowledge that lies at the core of scientists' work. It is also to deny the manifest (at times even life-threatening) successes of science. Until we can articulate an adequate response to the question of how nature interacts with culture in the production of scientific knowledge, working scientists will continue to find their more traditional mind-sets more comfortable, more adequate. (1987a, 90)

I should like to close this chapter by going over the works of the philosopher Harding, a central figure in feminist epistemology. Harding also adumbrates a shift within "the feminist question about science", however, in her case, she proceeds from claims about a feminist methodology and epistemology to claims about a feminist research agenda. As she stressed in her most recent writings (1989, 1990), both 'feminist empiricism' and the 'feminist standpoint' emphasize the need for empirical enquiry and theoretical reflexivity in the process of knowledge. These two avenues have now paved the way for the enunciation of a 'feminist science': a process whereby the adequacy of analytical tools and relevancy of research questions would have to be constantly questioned and revised. As such, therefore, Harding is far from renouncing the usefulness of canons of validation provided by critical realism. Let us provide here some details.

In 1983, Harding (in Harding and Hintikka) discussed the project of feminist epistemology on the basis of a criticism of the three traditions of empiricism, functionalism or relativism, and Marxism. In her view the main lacuna in empiricism and relativism is their inability to correct their own errors: empiricism does not acknowledge the social values involved in thought and representation; relativism does not distinguish "true" from "false", nor "bad" from "good" beliefs. As for Marxism, she indicated that it had overlooked the sex/gender system in its account of objective contradictions within society. At that stage in her reflection about science, however, Harding was still exploring the barriers to the emergence of a feminist science; she was not addressing the substantive problems related to the viability of a possible feminist epistemology.

In 1986, Harding became more specific. She acknowledged the valuable contributions of feminist empiricism and feminist Marxism ("the feminist standpoint"); she gave credit, for example, to the works of Doell and Longino as representing feminist empiricism. In contrast to feminist empiricists however, she suggested that current science is not merely "bad science", but rather "science as usual" and must, as such, be subjected to more profound changes. She did not believe that science could be reformed by means of the same methodological rules as traditional science. Harding instead proposed to lay the foundations of feminist science in "unstable categories" of analysis such as "gender", "anti racism", "anti sexism" (1986a).

In other writings (1986b), she held that "feminist epistemology" must challenge male-dominated science by replacing traditional canons of truth by a constant questioning of its old certainties. Harding further posited that feminist epistemology must reject the three following premises: masculinity-affirming division of labour, assignment of gender identities to human infants, and asymmetric meanings of masculinity and feminity in gender symbols (or "gender totemism"). In brief, Harding rejected any fixed totalizing theory in favour of a multiple system theory. She advanced the idea of a feminist perspective based on "unstable categories". As science is facing unique dilemmas, Harding held, the framework of "unstable categories" ought to be given a chance as a replacement of traditional science.

One might wish, at this point, to question whether Harding was really trying to construct a new epistemology, amenable to a workable feminist science, or if, like other feminists, she only set out to criticize some theories?[21] The answer to this question emerges in her last writings.

In more recent essays, Harding (1987a, 1987b) goes so far as to refute the idea of a new feminist methodology. Instead, she accords credit to the more traditional contributions of feminists to science, namely the social sciences. As she notes, these contributions arose mainly from qualitative studies and from conceptual innovations in theory-building: the qualitative methods have permitted us to centre our attention on women's personal experiences, which have, in feedback, generated new data and research questions. The distinctive contribution of feminist science is therefore the "gathering of evidence in different ways" (1987a, 25). The originality of feminist science does not lie, initially, in the elaboration of a new methodology or a new epistemology; its originality is mainly expressed under the form a "distinctive research agenda". Harding writes: "Feminist research is distinctive in its focus on gender as a variable and an analytic category, and its critical stance toward gender" (ibid., 29).

Several clarifications could be made in relation to Harding's reflection about epistemology. First, Harding did not, in the final analysis, reject feminist empiricism, and, in this sense, her position is closer to that of Longino and Doell than she herself claims. As a result, she might well have dismissed prematurely the Popperian epistemology as applied to biology and the natural sciences; Popper himself proposed to make a distinction between the "worlds" of the critical attitude and argumentative language, and that of empirical statements[22]. For these reasons we contend that feminist science, as described by Harding in particular and feminists in general, is not a new scientific epistemology (for it still relies on the Popperian critical realist epistemology), but, rather, that it represents a distinctive research agenda.

In a similar vein, Longino has tried to define feminist science as "process oriented" rather than "content oriented" (Longino 1987). She believes that feminist science is basically a process of criticism rather than a theory-building framework; it is "practice rather than content". She thus makes an appeal not for a "feminist science" in the first place, but rather for "doing science as a feminist" (ibid., 53. See also Longino 1989). Taking criticisms of biology as an example, she holds that one of her aims as a feminist critic of science has been to draw attention to the influence of culture on human behaviour. This would suggest that feminist science can successfully alter scientific knowledge when it is basically concerned with the reconceptualization of, for instance, human behaviour and gender. However, while I concur that feminist science essentially is a critical outlook on gender-related theories, I do believe that it may also be content-oriented, as a review of certain feminist contributions to primatology and evolution has shown.

I should like, finally, to highlight a flaw in the feminist reflection about science stemming from a generalization of feminist criticisms of biology directed toward the whole of scientific epistemology. I suggest, in this connection, that as long as feminists will not distinguish between the various teleological genres which constitute the various types of biological explanations; or will not distinguish between the "world" of rational explanation and the "world" of sense-datum[23], subjective experience and emotions, the feminist challenge to biology will not be taken seriously by practising biologists or epistemologists.

Conclusion

I have endeavoured to show that if it might prove intellectually sound to try to translate the norms of feminine practice as norms for scientific research[24], the former cannot replace the latter altogether, least of all as the canons of a 'new' scientific epistemology. It may be reasonable to contend that some norms and skills can be adapted directly from the family context, for instance, to the functioning of a research team; but it does not seem reasonable to maintain that all specific norms of science could be so translated. Therein lies the limitation to an application of the "caring approach" to scientific research. I believe, along with philosophers as diverse as Popper, Habermas, and Hesse, that scientific knowledge is demarcated from common sense, intuition, empathy, emotion; science rests on a rational method of logical inferences by which explanatory statements can be assessed or challenged upon empirical evidence.

I have propounded the idea that feminist projects of biology have, until recently, been devised on the basis of unfortunate confusions between 'feminine inclinations' (moral and emotional) and epistemological canons of justification. I would now suggest that these confusions might have taken place partly because of the dual status of biological explanations (which Longino & Doell have tried to expound in their article of 1983). That is, biology may sometimes features aspects resembling those of behavioural inquests and the hermeneutic method, and at other times, aspects more similar to those of physics, chemistry, and the empirico-analytical sciences.

I should however stress that feminist theoreticians were right in two respects: first, that the "empirical underdetermination of scientific theories" is the condition of possibility for their critiques of male-biased biology; secondly, that feminine subjectivity (as rooted in women's shared experiences of life as a social group) may be conceived of as a platform for conceptually original theory-building in human biology, including the formulation of research questions, the elaboration of observational designs, and the interpretation of results.

The reason for this, I would suggest, is two-fold. First, the validation of scientific explanations in biology does not respond to only one set of canons, but to a two-tiered set of epistemological values which do not equally fit the diverse types of phenomena explored in behavioural studies and in the physical sciences. There are models of explanation which seem specific to biology, in which the background knowledge and underlying values that enter theory-building (relating to lower organisms but more particularly to higher organisms) are more directly influenced by ideologies about the social order than it is the case in physics or chemistry by comparison. Secondly, there are rational norms of explanation (such as coherence and empirical inferences) which scientists cannot dispense with in science, primarily because they are the guidelines which precisely distinguish error from falsehood and inadequate from acceptable assumptions in theory-building. In observation-based disciplines however, such as human and social studies and parts of biology, these guidelines are difficult to comply with, primarily because of the ontological character of the subject matters in these fields of studies. This constitutes, I would suggest, the epistemological and sociological context that has nurtured the clash between patriarchal and feminist theories in some areas of biology, and might also give rise to a fully-fledged feminist biology within the confines of mainstream human biology and critical realism. We shall refer to Habermas and Hesse to illustrate this point in the next chapter.

Biology involves itself with both the epistemological controversies of the social sciences and the technical power endowed to the physical sciences. This dual nature of biology is a common feature in its history, as we shall see in chapters 3 and 4. I have suggested that the type of epistemological and methodological issues feminists have addressed is not very remote from those that one may find in the history of biology. Causal models of explanation and structural determinism are epistemological categories that even the 'softer' disciplines of biology have relied on, only perhaps with more caution and sophistication. We shall refer to Gellner to argue this point further.

In short, the epistemological problematic created by the peculiar status of biological explanations seems to substantially contribute to the clash between establishment biology and feminist critiques of biology. On the other hand, this also renders possible the vindication of projects of 'feminist biology' from within the confines of this science and without effecting drastic changes in the basic canons of scientific justification. Chapters 3 and 4 will explore the epistemological and historical features linked to the practice of biology, bearing in mind the central question of this thesis: what are the epistemological and sociological conditions of possibility for constructing a 'feminist biology' as such.

Endnotes

1) For a critical review of these epistemological issues in feminist theory, see Hawkesworth (1989).

2) Marxist feminism is not a homogeneous body of theory. The relationship between class and gender is not analyzed in the same way by radical or culturalist Marxists, for instance. (See Oakley 1982; Mitchell and Oakley 1986; Moi 1987; Walby 1990.) It is reasonable to think however, that as critics of science, Marxist feminists share roughly the same views in their analysis of the patriarchal system of science.

3) Kirkup (1986) and Stacey (1988) highlighted the limitations of a feminist epistemology based on "feminine" values for the social sciences, arguing that this perspective posits the use of methods of observation and interaction which would, in fact, serve inadequately research on certain types of problems. For instance, a feminist approach oriented towards the cooperation and sharing of experiences between the observer and the observed appears rather inappropriate if the observer has to accommodate with subjects who are either unwilling to cooperate, or who might conceivably be dishonest in their responses.

4) I am borrowing the argument from the analyses of Habermas, Hesse and Gellner which are discussed in chapter 3.

5) For instance, Fee points out that the production of knowledge, instead of being dissociated from its social uses, should be construed as an act of prime social responsibility. A solution in terms of more social responsibility of scientists presupposes a complicated process, the principles of which Fee does not even theoretically discuss. This issue about modern science has been debated for a protracted period of time (See Barnes 1985; Medawar 1985; Passmore 1978; Ravetz 1971).

6) This notion is borrowed from Gellner (1974).

7) In my view, this is, at best, an inconsistent, at worst, an indefensible argument.

It is inconsistent, because Fee has just argued that one should replace the spurious objectivity of science by social values such as "non-domination", "non-exploitation", "respect of nature", and "social responsibility" (in Fee 1983). It is indefensible, because it is odd for a feminist committed to the social responsibility of scientists to suggest the consideration of "criteria of science" by "playing" with ideas. It seems, indeed, an inappropriate method of solving a serious problem, especially for someone who so strenuously criticized the very casualness with which scientists currently handle their social responsibility.

8) This is not to say that the physical sciences have never faced internal debates in relation to theory-building. In fact, several biologists interviewed for this research suggested that theoretical physics, for example, is heavily speculative, the epitome of scientific imagination, while parts of biology, such as molecular biology, do not so much rely on interpretive power. See chapter 6. I would suggest that the values predicating theories in physics have been far less frequently directly subjected to social and political issues compared to those of biology and human studies. See the historical accounts in Capra (1983), and Mendelsohn (1977), and the epistemological essays of Hesse (1980a) and Gould (1985).

9) Renowned scientists such as Lonsdale (1970) have also deplored the traditional upbringing of girls as not conducive to science and technology-oriented activities (See also Haber 1979; Richter 1982; J. Harding 1986).

10) See, for the field of biology in particular, Abir-Am (1982a, 1982b), Murphy (1980), and Rossiter (1982).

11) The importance of both consciousness-raising, and the role of subjective experiences as research insights, are certainly among the most typical features of a phenomenological feminist theory of knowledge. See British Sociological Association 1987; Glennon 1979; Jaggar and Bordo 1989; Oakley 1981; Sabrosky 1979; Spender 1980, 1983; Stanley and Wise 1983).

12) The slogan "the personal is political" and the rise of "consciousness-raising" groups for women are important features of this concern of feminists.

13) Unsurprisingly, Andersen's examples in this case are drawn from the life sciences, namely the subjects related to sexual differences in behaviour, evolution, reproductive technologies, and women's health (Andersen 1987).

14) In this connection, Simeone's small-scale study with academic women (twenty respondents) (1987), for instance, shows that only a few women disengage totally with feminism in a rather hostile way, while equal numbers respond either positively, without any qualification, or else with some reservations as to whether they would call themselves feminists. Overall, the majority of women was described as seeking the reconstitution of the career model of academia according to a full integration to women's culture and social position. These women are typically "doing research on women-related topics, forming alliances with female colleagues and students, becoming involved with women's concerns on campus, and playing a nontraditional role with respect to marriage and family" (ibid.,

97). They will also, more characteristically than other women, identify overtly with feminism. Given the sample of Simeone however, this tends to more accurately reflect the situation of social science scholars rather than natural scientists. See the results of the present study in chapter 6 for a comparison with Simeone's conclusions.

15) Although Jaeger appears more knowledgeable as to the complexities of the scientific processes of discovery and justification, she seems, in my view, to neglect the existence of gaps between the principles and the actual practice of science, gaps which feminists, for their part, have tended to overstate.

16) The Nobel Prize winner, Lonsdale (Lonsdale 1970) also spoke along these lines as early as the initial years of the 1970s.

17) In this regard, the views of Fausto-Sterling and of Messing, for example, are symptomatic of a malaise among feminist biologists and critics of science. They contend that it is unsound (if not even dangerous) to think that "feminine" social practice is co-extensive with "feminist" science. They persist, however, in attributing a central role to women in the genesis of a feminist science, and also to denouncing the hierarchical structure of science in favour of a more cooperative, "woman-friendly" working milieu. Interviews with A. Fausto-Sterling (Montreal, May 23, 1989); and of K. Messing (Montreal, May 31, 1989) by the author. See also the essays of the neurophysiologist Donna Mergler "Les différentes attitudes développées par les femmes de science dans leur travail" in Cahiers de l'ACFAS, no. 22 (Montreal: ACFAS/ Presses de l'Université du Québec, 1983), and "La science au masculin: réflexions d'une scientifique sur 'A l'école des sciences'" in Resources for Feminist Research 15, 3 (November 1983).

18) See, for example, the positions of some eminent biologists. In Chargaff 1978; Luria 1985; Watson 1968; Olby 1974.

19) This is reflected in the debates surrounding her book on Barbara McClintock (Keller 1983) at the Joint Conference of the British Society for the History of Science and the History of Science Society (BSHS/HSS) held in July 1988 in Manchester, where Keller was asked to defend her point of view on feminist science. See BSHS/HSS Proceedings (1988).

20) In "Women Scientists and Feminist Critics of Science" (1987a), Keller expresses how she experienced a shift from mind-set as a woman scientist to mind set as a feminist critic. There are three major steps in that shift: they are, an identification with the women's movement; recourse to psychoanalysis, that is, to the ideas that "the personal is political" and that beliefs have an influence on other forms of cognition; and finally, an integration of one's experience of motherhood and scientific work. Keller indicates that only subsequent to such a shift of mind-sets "did it become possible to raise the questions in what I came to think of as their proper form -- as questions not about the remaking of science from the perspective of 'women's vision and creativity' but about the simultaneous remaking of our conceptions of men, women, and science" (ibid., 89).

21) Walby (1990) also questions Harding's views on the same lines.

22) Popper (1972) distinguishes "three worlds" in epistemology: the world of sense-datum and experiences, the world of theory and ideas, and the 'social' world in which ideas about reality are transmitted, discussed and challenged.

23) Longino has adopted a more pragmatic position (as noted in chapter 1). She recently reiterated her perspective on feminist science and epistemology (Longino 1987, 1989), stating that, in the first instance, one always needs to choose some descriptive point of view about objects: this is a necessary condition for cognitive statements. Critical discussion, as necessary as it might be in the "progress of knowledge", does not provide any operational framework or structure which describes or explains objects. The critical standpoint only helps to choose (among cognitive goals or descriptions of events) which are to be accepted as conforming to certain aims (rationally selected and discussed) and the experiences of the sense. Other feminists (Alcoff 1987; Fee 1985; Flax 1987; Hawkesworth 1989; Saarinen 1988) have recently argued that, as science makes theory-choices, feminism must also make theory choices in the development of feminist science. It is suggested, however, that such choices would include rethinking definitions of truth and the legitimacy of scientific knowledge.

24) Similar attempts have been made to transfer the norms of domestic work to those of bureaucratic management (Millman and Moss Kanter, 1975). But these are now regarded as misguided (Ferguson 1984).

CHAPTER 3

DIFFERENTIATING MODELS OF EXPLANATION, VALUE-ASSERTIONS IN THEORY-BUILDING, AND CANONS OF VALIDATION IN HUMAN AND NATURAL SCIENCES

The critical undertaking of feminists in relation to biological science owes a lot to a post-positivist sociology of knowledge. This sociological framework constitutes the background through which a critique of the natural sciences, methods and theories is made possible.

During the first third of this century the natural sciences were still seen as the sole form of scientific knowledge capable of reaching a true, objective, and context-free understanding of the world. But they became subjected to a sociological critique, as greater numbers of philosophers finally agreed, in the aftermath of unsuccessful attempts by logical positivists to establish foundations of a universally true knowledge (as totally based on an observational verification independent of the prejudices or representations of the world of the knowing subject), that any scientific theory was "empirically underdetermined" (Chalmers 1982; Habermas 1974, 1976; Halfpenny 1982; Harding 1976; Hesse 1980a, 1980b; Hodson 1982).

It was believed that sociological factors, pertaining either to the micro-system of scientific institutions, or to the macro-system of social structures, could partly explain not only why a given theory or hypothesis would come into existence, but also how 'a rationale' for the establishment of norms of justification of scientific theories was settled.

In this chapter we will be concerned in large part with questions relating to the legitimation of knowledge and the justification of theories, but also to the logic of discovery (or origins of scientific hypotheses). We will refer to the latter in chapter 4 as we discuss biology proper and suggest that some epistemological features of human biology might invite more readily to the idea of a 'feminist biology' based on 'women's shared life experiences'. With respect to feminist critiques of biology, this chapter will address the following issues: first, is it reasonable to assume that among societal factors affecting the production of scientific knowledge, all are patriarchy-laden, that is, reflecting the values and norms of a patriarchal power structure? Alternately, can some of these values and conditions also be applicable to a feminist biology? In brief, are there any scientific norms and epistemological canons which could predicate the production and validation of both "patriarchal" and "feminist" biological theories?

Secondly, is it sound to apply indiscriminately a socio-critical blueprint to all disciplines, be they part of the social, physical or life sciences? Rather, should one assume that the different types of objects found in these diverse disciplines (e.g. animate/inanimate; living/non-living, human/non-human) lend themselves to different models of explanation which do not bear directly on social and political ideologies and conflicts? Is it not more legitimate and sound to seek to distinguish these models on the ground that each involves more or less immediately controversial social values and evaluative judgements relating to sex and gender roles, for example? Is it not so that the objects of study in diverse disciplines might require different methods and conceptual approaches in order to be properly explained?

All these questions do not seem to be properly examined or even addressed in the feminist literature about scientific knowledge. I shall suggest, however, that some useful insights into the analysis of the "sociological dynamics of change" in the natural sciences, as reflected in the emergence of a feminist biology, are likely to derive from the works of philosophers with an interest for a sociology of scientific knowledge such as Habermas, Hesse, and Gellner. In their attempts to undercut the strongly discursive and intersubjective perspectives on science of numbers of feminists, these authors might have reinstated the sociological and epistemological bases needed to explain (on grounds other than merely political) why certain feminist biological theories have gained (and might gain more) credit in the views of "mainstream biologists".

Sociologies of Scientific Knowledge

Early sociologies of knowledge, like those of, for instance, Comte, Marx, Dilthey, Durkheim, Gramsci and Mannheim, seemed to be committed to the idea of an objective method for the social sciences. They were all seeking an universal point of view on the social world (in Bauman 1978; Bernstein 1983; Bleicher 1982; Giddens 1976; Hamilton 1974; Hekman 1986; Larrain 1983; Mannheim 1952; Outhwaite 1975) capable of freeing itself from social biases, and emulating methods of the natural sciences. For several, the method produced in the natural sciences eschewed the influence of social ideologies, generating universally true knowledge, and could thus provide objective foundations for a study of society. Marxist historical and dialectical materialism looms large in that connection. They had much trouble however, in reconciling a framework of causal determination with the principle of historical changes (see also Jay 1986; Popper 1957). For this reason, they seemed to have turned to teleological models in their explanations of social changes.

On the other hand, hermeneuticians endeavoured to develop a distinctive method for human and social studies. They suggested that, as humans were animate creatures, endowed with volition, motivation and symbolic expressions, they presented a peculiar problem for knowledge, in comparison to objects in the natural sciences. Human beings cannot be "explained", as hermeneuticians would say. They can only be "understood" with reference to the cultural context in which they live and communicate; their meaningful expressions can only be "interpreted" in their context. Having made a point for the specificity of human and cultural objects however, the idea of establishing an objective method for the study of context-bound action and intersubjective communication was to remain the source of heated debate among phenomenologists and hermeneuticians.

The sociologies of knowledge developed more recently have addressed the questions of how knowledge is constituted not only in the social sciences, but also in the observational and experimental-based disciplines of the natural sciences. The aftermath of the failed project of logical positivists was to provide epistemological conditions for what was to be called in the 1970s the "strong programme in the sociology of knowledge" (Barnes 1974; Bloor 1976), setting out analyses of the substantive content of "true" knowledge in the natural sciences. Yet there is great diversity in the frameworks of a "social construction" of scientific knowledge being developed on this basis.

Sociologists of science, therefore, began to criticize the natural sciences directly[1]; but several philosophers of science, like Popper (1959, 1969, 1970, 1972), Lakatos (1970) and Hempel (1966) (to name only the most renowned) were still committed to the idea of a progress of scientific knowledge. They subscribed to a form of empiricism as the epistemological conditions of true knowledge, which, although weaker than in logical positivism, remained crucial as part of the conditions of possibility for the validation of theories. They were, however, challenged by both philosophers and sociologists of science like Kuhn (1970a, 1970b), Habermas (1970, 1974) and Feyerabend (1975). (See also in Lakatos and Musgrave 1970.)

Among the sociologists conceiving of science as primarily discursive, there are those inspired directly by Wittgenstein (in Winch 1958; Wilson 1970) such as the social constructionist Gergen (1982); but like Gergen's views, these seem, however, to apply more directly to behavioural studies. Other sociologists, like Schutz (1962) and Berger and Luckmann (1967), were influenced more specifically by the phenomenology of Scheler (1980) (see also Gurvitch 1966). Schutz for instance, developed a theory of types of knowledge ("cognitive styles") where people construct their representations of reality intersubjectively, and where the influence of the context at hand and the psychological pressures of peers ("inner group attitudes") are of prime importance in the production and legitimation of knowledge.

This perspective, in turn, seems to have influenced social constructionists interested in the analysis of how the social context shapes the production of knowledge in natural sciences as such. Among the sociologists representative of this approach, are Barnes (1974, 1985), Collins (1985), Mulkay (1979), Knorr-Cetina (1981), and Latour & Woolgar (1979). This approach was however criticized for not accommodating in its logic, a significant role for the empirical rules of justification (Hesse 1986; Harré 1983; Lukes 1982). Also inspired by phenomenology, ethnomethodologists were far more radical, adopting a linguistically-oriented approach to scientific practice (Garfinkel 1967; Lynch 1982, 1988). All these perspectives seem to vindicate a "strong programme in the sociology of knowledge", that is to say a programme of sociological analysis of knowledge produced in the natural sciences which states that sociology does not have to be confined to the investigation of scientific errors alone, but can also analyze how scientific truths and criteria of justification are established (see Barnes 1974; Barnes and Bloor 1982; Bloor 1976).

The inspiration of three other traditions in the sociology of knowledge seem to emerge more strongly in the works of feminist critics of science. First, one should notice the intellectual inheritance of Marxism and the Frankfurt School. One of the main ideas propounded by the School is that facts are never separated from values (hence from ideologies or social beliefs) epistemologically (Habermas 1974; Malherbe 1976; Ray 1979). Such an idea is reflected clearly not only in the feminist criticisms of "patriarchal biology", but also in the appeal for a feminist science as "more humane", "aware of its own biases" and "publicly committed" to feminist goals.

Secondly, there is a marked influence of Foucault's deconstructionism. The stance in which feminists have undertaken to challenge the authority of "patriarchal and androcentric science" reflects this. Foucault defended a view (although more especially for social and clinical sciences) which posits knowledge as the end-product of social practices oriented towards power and control (Foucault 1972, 1980)[2].

Finally, the Kuhnian outlook on scientific paradigms looms large in the works of early feminist critics of science. This outlook, less connected to the social system as a whole than to the scientific system as such (thus, much less radical than Feyerabend's views, for example), posited that concepts, instruments and knowledge are all theory-laden (Kuhn 1970a, 1970b). As a result, one ought to get out of a "paradigm" (like the "andro-centric" paradigm of biology) in order to introduce new scientific questions and to vindicate new theories.

Inspired by these intellectual traditions, feminists embarked on their critique of science on the principle that neither science or epistemology are secured against sociological investigation. As we have shown earlier, this means that from a feminist view, it was possible to scrutinize the basis of validity of biological theories on the premise that these had been developed within a patriarchal society and an androcentric science. Feminists believed that biological theories were sexist and flawed with respect to women in particular, and with respect to human beings and living forms in general. But they did not limit their critique to the unveiling of patriarchal and androcentric biases; they also endeavoured to eradicate these biases from the process of validation of theories, thus renewing biology as a whole -- its approach and its canons of scientific proof. In other words, a renewal of the research agenda, the conceptual framework, the method, and the epistemology, were all necessary.

As useful as they may be, sociologies of knowledge have often been criticized for creating more problems than solutions. Studies conducted in terms of a relativism (or "relationism", to use Mannheim's terminology (1936)) between forms of knowledge, for instance, were seen as rich in empirical content (see in Wilson 1970; and Hollis and Lukes 1982). It was also argued, however, that relativism generated more problems (for a legitimate analysis of the social aspects of knowledge) than solutions with regard to the inescapable social embeddedness of human knowledge, as Gellner insisted (Gellner 1974, 1982). Moreover, relativism was seen as dismissing too hastily the utility of certain "quasi-universal" notions (like principles of logic such as identity and contradiction), notions which made it possible to distinguish, rationally, valid from invalid inferences (Lukes 1970, 1982; Hollis and Lukes 1982).

Discourse analysis and Foucauldian deconstructionism, were considered useful for highlighting the presence of covert ideologies or power struggles in the sanction of metaphors, assumptions, models, hypotheses, and conclusive evidence. They also led, however, to an overstating of the power of language on our representations of reality (Mortensen 1986; Hawkesworth 1989).

Finally, phenomenology, as useful as it may be for identifying the agents and social pressures involved in the construction of knowledge, tended to blur the demarcation between common-sense, metaphysical knowledge and science -- or other types of knowledge produced by means of rational argumentation and validation.

Feminists (as we noticed in Part I) have used the critical categories of analysis provided by the three foregoing schools of thought; but these traditions did not seem to serve them well, as far as empirical science was concerned. Not only did feminists think their own argument for a "better" and "truer" biology was somewhat self-defeating; it did even not render justice to the view that women's oppression was real, and feminist theories of women's biology, a "truer" understanding of social reality. While I agree, to an extent, with the powerful insight secured by "feminine inter-subjectivities", the caring approach, or "empathic listening" as methods of investigation in the social sciences, on the other hand, I am reluctant to subscribe to the notion of a new epistemology based on the "subjective" and the "caring" for both the social and the biological sciences.

It, thus, seems worthwhile to have a look at the works of authors like Habermas, Hesse, and Gellner, in order to isolate the analytical flaws and contradictions, and also the promising avenues of action, in the feminist critiques of biology and natural science. For Habermas, Hesse and Gellner have all been interested in understanding how the social context shapes the representations which individuals or larger groups have of the world they inhabit, and also in the investigation of the social norms sustaining diverse types of 'cognitive interests', and rational procedures of validation of knowledge.

We shall address the following questions which are problematic in the feminist attempts to build a new biology. First, was it necessary to aim at renewing epistemology altogether in order to vindicate feminist biological theories? Or was it enough to use the Popperian model of the critical attitude[3]? Here Habermas will help us qualify our answer. Secondly, is it appropriate to use discourse analysis for both the critique of "androcentric" and "patriarchal" knowledge and the construction of new knowledge? Similarly, is it appropriate to use discourse analysis to the same extent for both social and natural sciences? Here there seem to be two qualifications, with which Hesse provides us. On the one hand, social values do not enter theory-building as directly in the social and the natural sciences, and these values should be criticized individually rather than indiscriminately. On the other hand, but in the same regard, biology seems to lie astride the social and the physical sciences; sometimes it raises social controversies similar to those in the human and social sciences, but at other times, similar to those in the physical sciences. Thirdly, is it necessary to reject determinist or structural models of explanation in human biology, in favour of historical or teleological models? Is it necessary to abandon the reductionist method in favour of hermeneutics or a socio-critique? On this point, Hesse and Gellner shed light on the epistemological status of biological theories enabling us to draw a comparison with the status of feminist biological theories.

We shall argue in the rest of chapter 3 that the influence of sociological factors on the production and validation of scientific theories does not extend uniformly to all the physical and human sciences. In chapter 4, we will pay special attention to biology. We will try to illustrate its specificity among the sciences, through a brief examination of its epistemological, historical and sociological dimensions. This should help us put forward the idea that biology is a corpus of theories which borrows from both the natural (empirico-analytical) sciences and the human (hermeneutic and discursive) studies methodological and conceptual tools which lend themselves to internal disputes relating to methods of testing hypotheses and theoretical models of explanations. As a consequence one should be cautious of the way one cites examples from some biological disciplines and then generalizes those examples to a critique of biology as a whole. The sociological and epistemological grounds for the critical study of feminist biology will thereby be laid out; thus we shall be ready to examine the empirical studies in Part III of this thesis.

Some Elements which Differentiate Models of Explanation and Canons of Validation in the Human and Physical sciences

Exercising a "critical attitude" in science may suggest different courses of action: a discussion of scientific discourse and research agendas in terms of their moral facets, their technical value, or even of their aesthetic content, or could revolve around the question of the logical "fitness" of structural, teleological, or interpretative explanations vis-à-vis empirical observations. In turn, possibly one should be able to delimit the potency of these 'critical judgements' by assessing if they describe the objects and are more or less relevant to the issue of which type of understanding one wishes to attain about these objects. In short, it is necessary, for purpose of a rational and critical argumentation about science to discern which types of "value goals" (of the social and the natural sciences), to use Hesse's terminology, are discussed. In this regard, Habermas and Hesse will suggest that there are certain substantial differences in the approaches to theory-building of the human and the natural sciences, even though the two are predicated by social values and 'cognitive interests' in the first instance.

The natural sciences deal with inert objects. It is reasonable to construe inert objects as if they were stripped from intentions underlying a structure which determines the process by which "they work". This stance on knowledge is suited to the purposes of discerning the factors responsible for the repetitive pattern displayed by certain phenomena and objects in nature. In this sense, objective knowledge is attained by means of a consensus about the type of cause-effect structures identified through observation and control, and ultimately through repeatable occurrences of the same pattern in given conditions. Criteria of empirical validation can thus be used pragmatically according to technical success. Yet the use of technologies implemented by physical science, for example, remains a normative issue which cannot be validated by the criteria of technical or experimental success (Habermas 1970, 1974).

In the social sciences no such distinction between technical success and social -- or moral -- norms seems appropriate[4]. Unless human beings really become totally coerced by social norms, automatons, or, worse, completely alienated[5], understanding of other human beings is believed to lie in a different dynamic altogether. Human beings are posited to be "conditioned" rather than determined by structural factors. Human behaviours are occurrences which seem more singular than repeatable, under "normal conditions". "Being" and "becoming" are two features of human phenomena which should never allow science to objectify "human" subject matters in terms of biological determinism or mechanistic metaphors of behaviour. If so, explanations may not only lose in richness but also in accuracy.

Both types of subject matters in the natural and the social sciences underline different sorts of representations about the patterns observed, which fit (more or less) that with which we endow them. Yet, this implies that observation constitutes the arbiter between true and false representations of the world; and this is where Gellner's reflection is most helpful. It distinguishes between two "planes", or stages, in scientific thought: selecting information and modelling explanation. It also implies that certain norms or values (like "experiences" and sensations) are very fruitful as selectors of information but empty as explanations, while others convey a limited view of the world and at the same time, manageable new explanatory insights (like the machine metaphor).

Habermas's Framework of Three Epistemologies

The contribution of Habermas to epistemology and to the sociology of knowledge is considerable. Habermas has, since the early 1960s, been developing a critique of the conditions of possibility of rational argumentation in the construction of human welfare. His endeavour is to establish links between cognitive, linguistic, social, and moral conditions of knowledge and human interaction in order to lay the grounds for universal pragmatics (Habermas 1976, 1978; also in Bernstein 1983, 1986; Giddens 1976; Mortensen 1986; Thompson 1984).

Modern Society, Rationality, and Instrumental Knowledge

Habermas is concerned with the evolution of social trends and their relation to knowledge and science. His main interest is to understand how and why contemporary social life has been unfolding towards the rationalization of values, as Weber (1968; also in Bauman 1978) had foreseen. He is also concerned with the dehumanization and alienation of humanity, which seems to have lost a genuine understanding of itself and of its destiny.

Habermas suggests that it is not so much the implementation of natural sciences and technologies which are endangering the quality of modern life. What is far more dangerous is the collapse of critical judgment about social decisions and norms, a degeneration to criteria of technical success[6]. In modern industrial societies, for example, technical progress is believed to conduce to social welfare without relying on partisan policies or social values. Rationality is construed as instrumental knowledge which serves the purposes of decision making (Habermas 1970, 1978). Paradoxically however, as political bureaucracies and economic systems develop, they must also allocate more resources to restrain the release of artistic, creative, and communicative forces of social movements which are developing within the cultural system. This produces a crisis of legitimacy and rationality, which shows that instrumentality remains dominant but does not altogether stifle social criticism. In other words, scientific instrumentality and technologies uniquely foster critical movements within their own technocratic culture and language.

Human Interests and Forms of Knowledge

It is in Knowledge and Human Interests (1976) that Habermas developed his framework of three epistemologies. These correspond to three types of human interests: instrumental, hermeneutic, and critical (or emancipatory). The three epistemologies refer to three usages of language: descriptive (of facts), postulatory (of rules of procedure), and critical (justifying decisions) (Habermas 1974, 216). The necessary conditions of social life cannot escape any of the following: technical production, a social project, and rationality by means of free communication ("universal pragmatics").

Habermas first defines the epistemology of empirical-analytical research: it is based on systematic observations, and underwrites the possibility of a correspondence between theory and experience. Cognitively, this epistemology is based on a technical interest in 'feedback-controlled activity': it is instrumental rather than functions as an hermeneutic. From this perspective, causal hypotheses are developed in anticipation of law-like regularity. Anticipation or decisions made in order to accept or reject basic empirical statements are not based on inductive logic alone however, but also on institutional rules focused on what is considered to be a reasonable explanation and an acceptable inductive inference. This type of epistemology applies to the natural and physical sciences, but it applies also to a large part of the human sciences and psychology.

Hermeneutics forms the second epistemology in Habermas's framework. The guiding cognitive principle is the practical import of understanding fellow human beings, the meanings and motives of their action and speech. In hermeneutics, objectivity presupposes that the subjects under study are speaking honestly and self-consciously (Habermas 1976, 1984). Consensus on the terms of speech and communication is the sole guarantee of a successful understanding between the hermeneutician and his/her subjects. This type of epistemology applies generally to the human sciences and psychoanalysis. It cannot purport to replace the model of cognition used in empirico-analytical sciences, partly because it does not have the same goals. Habermas argues that the success of technical applications has falsely created the illusion that pragmatic success is "truth", and that it is the values of science, as only possibly interpreted by hermeneutics, which can highlight this fact.

Finally, a third epistemology is oriented towards criticism and emancipation. It forms the argumentative basis for the selection of norms acting as "validation" criteria. It thus differs from the epistemologies of empirico-analytical research and of hermeneutically-oriented sciences inasmuch as it is "the dimension of comprehensive rationality which, although incapable of ultimate substantiation, develops in a circle of reflective self-justification" (Habermas 1974, 212). Criticism overcomes the dualism of facts and values by translating logical constraints into empirical constraints, and vice-versa. It provides the cognitive continuum between technical decisions and logical deductions. Habermas summarizes as follows,

> Critical argumentation differentiates itself from deductive argumentation in progressing beyond the dimensions of the logical connection of statements and includes a moment which transcends language-outlooks. A relationship of implication between outlooks and statements is impossible: outlooks cannot be deduced from statements nor, vice-versa, statements from outlooks. Nevertheless agreement upon a mode of procedure and the acceptance of a rule can be supported or weakened with arguments; at any rate, it can be rationally considered and judged. This is the task of critique with reference to both practical and metatheoretical decisions. (1974, 209-10)

Habermas has prepared the ground for an analytical distinction between technical rules, hermeneutic rules, and socio-criticism in order to show that these three sets of rules are entrenched in social values, but, also, ultimately, amenable to a rational discussion from the perspective of universal pragmatics. It reinstates the ultimate importance of communication and speech (interaction), and technical success (work) -- the two poles of social life and knowledge. We cannot favour technical interests at the expense of hermeneutic understanding, for human life is about work and interaction.

Habermas's framework sheds some light on the positive role of values in knowledge, and on the adequacy of diverse testing procedures in science. Explanations in the natural sciences are largely tested against their technical success, juxtaposed with explanations in the social sciences. Habermas also warns against a dangerous collapse of the goals of hermeneutics and socio-critique in favour of technological success. Knowledge remains subjected ultimately to critical evaluation; a total separation of facts and values is therefore epistemologically spurious.

Habermas has been criticized primarily for his failure to refer to substantive aspects of power (Giddens 1976; Larrain 1983; Bernstein 1983, 1986; Mortensen 1986), irrespective of gender or racial divisions in society. Basically he investigates the levels and conditions of possibility of valid knowledge but does not ultimately confront the actual clashes of values (Bernstein 1983, 1986). In this sense, his framework becomes too cultural and not sufficiently political or economic, in contrast to Foucault and the feminists.

How and when are we, in practice, to discount technical success in favour of hermeneutical or critical goals much as feminists have tended to do emphatically in their justification of a feminist biology? This is what Hesse and Gellner will help us clarify.

Value-goals in Science: the Special Status of the Pragmatic Criterion in the Sciences

In her book Revolutions and Reconstructions in the Philosophy of Science, Mary Hesse expounds the historical role of "pragmatic criterion" in the "modification of the traditional empirical criteria of confirmation and falsifiability" in an empiricist philosophy of science (Hesse 1980a, 190).

A Historical View on Scientific Epistemology and Empirical Knowledge: the 'Success Story' of the Pragmatic Criterion

Hesse acknowledges that natural science has accumulated empirical knowledge. This should not be seen in terms of conjectures and refutations of single theories, such as the Popperian outlook, but rather in terms of the empirical and theoretical coherence of whole research programmes, such as that of Lakatos. Hesse argues that the pragmatic criterion of predictive success is essential to an understanding of the historical development of natural science. She claims three reasons for this: first, the pragmatic criterion escapes the now refuted traditional criteria of empirical confirmation and falsifiability; secondly, it explains the preference of one theory over another without resorting to any local reasons (such as social customs, or psychological preference); finally, it accounts for revolutionary objections to theories without rejecting cumulative and progressive knowledge altogether, but by retaining a contact with the empirical world "by means of long-term testing of theory complexes taken as wholes" (Hesse 1980a, 190; see also Hesse 1980b).

She qualifies the Popperian idea of the critical attitude of science by expanding the sources of scientific critiques. Criticism, in Hesse's view, should be seen as both empirically and hermeneutically grounded. She does not, on the other hand, deny the existence of an hierarchy of scientific rules and conventions, themselves justified by some rational concepts such as space, time, identity and contradiction, causation. Rather, she argues that none of these rules and concepts belong to some transcendent reality (Hesse 1980a, 1980b).

From her historical standpoint on epistemology Hesse investigates the similarities and differences between natural and social sciences. What is right and wrong with the pragmatic criterion? Does it apply equally to both the natural and social sciences? How has it shaped the way we conceive of science, the way we practice it, and the way we have criticized it?

First, Hesse suggests that there is a continuity rather than a dichotomy between natural and social sciences. Here she diverges from Habermas's framework but only subsequent to appropriating his idea of technical and hermeneutic interests. She makes the point that these two types of interests are equally shared by both sciences, even though the natural sciences are more technically-oriented and the human sciences more hermeneutically-oriented.

> I suggest that the crucial distinction between the social and natural sciences is not so much the presence or absence of evaluative ideologies, but rather the success or otherwise of the pragmatic criterion. In the natural sciences, this criterion is overriding, and enables ideologies to be filtered out in the historical development of a science, leaving a deposit of pragmatic or instrumental truth. There is no a priori guarantee, however, that the pragmatic criterion will be as successful in the social sciences, in other words there is no guarantee that these sciences will, can, or even should attain comprehensive and progressive theories like those of physics or biology. This fact, together with the admittedly evaluative character of adoption of the pragmatic criterion in the first place, suggests that the social sciences may properly adopt goals other than that of successful prediction and control of their domain. (Hesse 1980a, xxi)

Central in natural science is the model of a "learning machine". Hesse identifies three conditions for such a model to be workable and to attain the goal of successful learning: there must be the possibility of a detailed test to reinforce correct learning; the environment must be sufficiently stable for self-correcting learning processes to converge; there must not be strong action by the machine on its environment (Hesse 1980a, 182). The relevance of a model of machine-learning in the natural sciences is explained by the fact that their subject-matters variously fulfil its conditions of applicability. In the human sciences however, the subject matter does not fit particularly well these conditions, especially the third one. Nevertheless, the model of machine-learning is not entirely irrelevant here: sometimes the conditions are satisfied such as in those case studies examining primary behaviour patterns.

One may also, in contrast, question whether the subject matter in natural science always fulfils the conditions of the machine-model. According to Hesse, science historiography indicates that in disciplines like cosmology, the evidence provided towards theories are not verifiable; only mathematical formulae can support their validity.

Biology, Hesse notes, is another case in point. In this case, the stability of the environment and the absence of interaction between the "machine" and the environment are conditions which are far from being guaranteed. Theories of evolution, ecology, or genetics, embody concepts such as functionality, selection, survival, that are "infected by man's view of himself" (Hesse 1980a, 185). In those areas, "it is impossible ... to separate mode of knowledge relating to technical control from a mode relating to the self-understanding of man" (ibid.). Hesse contends that cosmology and biology cannot be excluded from the domain of the natural sciences. This makes her conclude that important areas of the natural sciences "seem to evoke both the analysis in terms of learning, and the hermeneutic model of personal dialogue" (ibid.).

In general, the development of the natural and social sciences may be explained by means of a sociological and historiographical model; this model is conveniently described in five elements (ibid., 187ff). First, all scientific theories are underdetermined by facts; secondly, all of these theories are subjected to revolutionary modification; thirdly, and much significantly, further criteria other that a correspondence to facts must exist, and these must be rationally discussed and include considerations of values; fourthly, the pragmatic criterion has deleted a priori value judgments in the development of theories in natural science; finally, such sifting of values has led to the universal adoption of the overriding value (criterion) of increasingly successful prediction and control of the environment in the justification of scientific knowledge in general. Comparing the extent to which such a model has suited the natural and social sciences, Hesse inevitably notes a discrepancy: the model has made natural science a powerful and paramount form of knowledge, while disclosing some of the weaknesses of the human sciences. Therefore, two additional elements should figure in the model to suit the social sciences. First, as very few social theories have fulfilled the pragmatic criterion in their domains, or else very inadequately, their results must be proven case by case instead of in general terms. In turn, the model of a development of scientific epistemology must be adjusted for the social sciences by the adding of two elements to the model employed to describe the natural sciences. The sixth criterion stresses, therefore, that the application of the pragmatic criterion is unable now to satisfy (and possibly permanently incapable of satisfying) the ontology of general social theories. Hesse argues nevertheless, that even though the model of machine-learning does not apply successfully in social science, it has been difficult, throughout modern history, to find an alternative. A survey of hermeneutic studies may indeed indicate that social scientists rely ultimately on a prediction of behaviour and its corollary model of machine-like humans, in order to validate interpretive hypotheses[7]. The seventh criterion posits that it might be possible to replace the pragmatic criterion; but this would involve a change of value-goals, in favour of other overriding value-goals for social science. As Hesse contends, hermeneutics, symbolic interactionism, and Marxist historical and dialectical materialism are certainly the most significant of these alternatives.

In brief, the central distinction between the social and natural sciences is likely to lie in the relative adequacy of one or the other to "fit in" specific sets of social and cognitive values. Hesse distinguishes in fact, between two roles imputed to values in scientific epistemology: these are the values ruling over how we use scientific results and implement them; and the values that serve to ground theory-building.

The Roles of Value Judgments in
Scientific Epistemology

Value judgements in science can be discussed upon the negative or the positive usage of technologies or ideologies inferred from "pure research". Warfare, ecological imbalances, brain washing, are all examples of the way we judge negatively implementations of scientific theories. Technologies of production, soil fertilization, sanitation and immunology may be judged as positive outcomes. Social interests are directly involved in these discussions. However, the examples given above convey the idea that "usage values" of science may always, in a given time and context, be consensual rather than conflictual.

But there are other types of value judgments in science. Value judgments may play an integral role as assertions in theory construction. In this case, value judgments presuppose the desirability of the universe being of a particular kind, and whether the universe is or is not broadly as it is desired to be[8]. According to Hesse, the onus of value judgments as assertions is that they involve a transition from an "ought" judgment to an "is" judgment, while possibly never being demonstrated as such. These values are seen as descriptive statements, filtering out their evaluative content. This does not, however, undercut the possibility of making logical inferences, because once we agree on assertions about what the world "is", then we are obligated to build a normative (rather than universally true) system of criteria which helps to rule out 'errors'. The role of assertions, as Hesse puts it, is to choose only a few hypotheses among many others possible "in the hope that the world will be found to be as good as the accepted value system describes the good" (Hesse 1980a, 189).

As the reader will understand by now, it is really at the junction of value assertions that the social and natural sciences diverge. At this point, the natural sciences follow the pragmatic criterion and cease to be pluralist, whereas the social sciences remain pluralist all along. In this epistemological context, the specific problem of social science theories lies in the underdetermination of evidence which is much more fluid, problematic, and discordant with the value-goals which usually characterize their views of what is desirable in the universe.

Indeed, Hesse singles out <u>two types of value assertions</u>: the <u>value goals</u> of science in general, and the <u>value-ladenness</u> of individual theories. According to her, the social sciences encompass diverse bodies of value-goals which are not the objects of consensus: historical and dialectical materialism, structuralism, hermeneutics, phenomenology, functionalism, are classic examples of such a diversity. But in addition, within any given social science paradigm one may say that values figure in the equation twice over. Evolutionary theories such as Darwinism and Lamarckism are cases in point. We may also add Marxist theory and the feminist theory of social relations. Within one paradigm, the value-goals are the same, but the value-ladenness of conflicting theories diverges significantly. Marxism, for instance, embodies different theories, and so does feminism. Phenomenology is another example of a tradition in social science which has given rise to bodies of theories as different as Marxism and functionalism. Hermeneutics and structuralism, however, seem to diverge relatively more drastically on the grounds of value-goals.

But more importantly perhaps, the social sciences, owing to their obvious lack of consensus on both value-goals and value-ladenness in their theoretical frameworks, have been characterized historically by an awareness of the evaluative character of all scientific theories. Yet Hesse reiterates that in the end the pragmatic criterion still remains our best guarantee of scientific validity, in both the natural and the social sciences. As a result, even though social theory is more like arguing a political case than an empirical natural-science explanation, it should nevertheless, "seek for and respect the facts when these are to be had" (1980a, 203).

To summarize, Hesse emphasizes the idea of a special epistemological status of the social sciences -- and of biology -- among the sciences. In light of her arguments, we may suggest that biology is entrenched in between social science models and natural science models with respect to its suitability to the pragmatic criterion. Biology seems less paradigmatic and more pluralistic than the physico-chemical sciences in its theory goals and theory choice, but not to the extent of being considered a social science. Hesse also argues that hermeneutics does not replace the pragmatic criterion of successful prediction altogether. Finally, she emphasizes the distinction to be made between the controversies relating to values judgments entering theory-building, and the failure of the pragmatic criterion as a valuable value-goal for social science -- and biological -- explanations, a distinction which feminists have tended to neglect.

As far as feminist biology is concerned, I would now like, in light of Hesse's arguments, to make some clarifications about the extent to which feminists have contributed to a replacement of 'traditional' or 'patriarchal values' of scientific research. The biological theories examined by feminist critics (as noted in previous chapters) are in the main part of the observational sciences (e.g. primatology, sociobiology, and other bio- behavioural studies). In these theories, value-ladenness is controversial indeed, and it has been shown reasonably well how these have reflected the biases of "patriarchal theory". As far as usage values in life and environmental sciences are concerned in general, however, feminists are not the first critics to urge for a more responsible politics of science in order to save the planet. Yet feminists have certainly been the most prominent denunciators of the detrimental treatment of women in biology and clinical studies. Finally, their critiques of the value-goals of science are not very different from those of "mainstream biologists" (at least from what the latter say in principle, as will be shown in chapter 6), and also, given that biology has always been fraught with internal debates about reductionism and holism, as will be shown in chapter 4.

Thus, even though feminists tend to reject the utility of the pragmatic criterion in science on the ground that "biology is not womens' destiny", yet feminist biologists cite it as the basis of validation of their theories. The rejection of a determinist account of woman's behaviour in favour of a more sophisticated, holistic model of explanation of interactions between organisms and environment should not lead automatically to a dismissal of the reductionist method, or of causal models, in that discipline. This is well indicated by an examination of Gellner's arguments.

Gellner's Sociology of Cognitive Norms and Models of Explanation

Gellner's contributions to epistemology, especially to the sociology and epistemology of the social sciences, is manifold (1959, 1964, 1970, 1974, 1982, 1987). As a social anthropologist and philosopher, he has drawn from cross-cultural studies to substantiate his analysis of the transition between systems of knowledge. He has also paid attention to the functioning of these systems, including that of his own society, conceiving of them as bodies of "norms which are to govern and limit our cognitive behaviour" (Gellner 1974, 31).

According to Gellner, epistemology, theories of knowledge, and systems of knowledge gain in plausibility when they are read as normative instead of as accounts of what knowledge "is really like" (Gellner 1974). But, normativeness should not be (in this case) linked so intimately with private, subjective, or arbitrary value judgements. Rather, normativeness should be construed as sociologically constructed, and fundamental for the legitimation of the given system of knowledge to which it relates. Socially constructed sets of norms of knowledge are necessary for the system to exist. They are not arbitrary and should be perceived as being selected from a rather small group of norms capable of suiting the cultural and structural conditions of the society in which they take place.

Gellner proposes that the transition from pre-industrial to industrial society was characterized by a crisis of cultural identity. But at the same time, as a society it needed a vantage point to regenerate. As a result, Gellner advances the notion that what this transition was concerned with was a change from an epistemology based on illusion or metaphysical beliefs, and not on an epistemology of ultimate truth. Rather, it highlighted an epistemology more characteristically based on "controlled doubt and irony" (1964). It is the centrality of epistemological doubt which has provided new norms of cognition, new value judgements. According to Gellner, these new value judgments are that certain things are good either because they are inevitable, or because their desirability cannot in practice be doubted: health and wealth, the affluent society, industry and technology obviously emerge as very desirable in our society, but also desirable for other non-industrial societies[9].

Gellner makes a strong case for the rejection of the idea that cognitive norms can be like "emotions". If these norms actually constitute mere emotions, this would presuppose that norms are "given" and that they might not possibly be the objects of rational evaluation. In other words, it would presuppose that cognitive norms were preeminent, superior to any possible reasoning. Emotions are definitely not the norms to which Gellner refers when he speaks of theories of knowledge as being normative, injunctive, or prescriptive (Gellner 1974).

Gellner's central argument with respect to the socio-history of systems of knowledge is that certain types of truth-claims have been more desirable at certain times in history (Gellner 1964). This is the basis for any explanation of the constitution or replacement of diverse systems of knowledge, but moreover, of the historical superiority of contemporary systems over others, namely Western scientific society over pre-scientific societies (Gellner 1964, 1974, 1982).

Gellner's views on knowledge systems, presented in the following pages, should help us to clarify the position of feminist biology in a view to epistemology and biological explanations. First, Gellner's distinction between cognitive endorsers and selectors in models of explanations should prove useful in the understanding of the bases on which feminist biology, with its strong critical stance on "patriarchal biases" in studies of behaviour, has justified itself. Secondly, Gellner's framework should help us to disentangle the theoretical confusion between a holistic view and a more reductionist, mechanistic type of explanation, a scientific problem widely discussed but left unresolved in the feminist literature on biology.

Cognitive Endorsers, Cognitive Selectors, and Models of Explanation

In Legitimation of Beliefs (1974), Gellner argues that re-endorser theories are usually based on rich reflections about the strengths and gaps of grand theories. But as their name illustrates, they only manage to "reach the conclusion that all is well with existing banks of beliefs, or at least with a substantial part of it, simply by virtue of its being the existing bank of belief" (ibid., 46).

Cognitive selectors are different in nature from re-endorser theories. Cognitive selectors' main function is to help building theory and explanations substantively. It also, by definition, "set up some criterion, some touchstone or sifter, which is to sort out the cognitive sheep from the goats... [and which] claims to be independent of the current and local set of beliefs" (ibid., 47).

There are, Gellner says, re-endorser theories of some currently dominant beliefs, but also negative re-endorser theories of other beliefs clashing with current beliefs. Thus, re-endorsers may be positive or negative. Evolutionism may be seen as a grand category of positive re-endorsers. These re-endorsers do not necessarily give substantive content to theories, but partly vindicate dominant social beliefs and norms of knowledge about behaviour. In contrast, negative re-endorsers are mainly concerned with revealing the distortion and "big error" of a set of beliefs, but without being single-minded about specifying what truth could then be ascertained once the "one big error" is unmasked. According to Gellner, the Marxist theory of knowledge and ideology is representative of negative re-endorsers, for it stipulates that "once you are free of [class-interests]... truth will be easily available and require no institutional underpinning" (Gellner 1974, 53). Relativism is a particular category of negative re-endorsers in the sense that it opposes rationality (See also Gellner 1987; Hollis and Lukes 1982; Wilson 1970).

Selector theories require a positive specification of truth, and their substantive contents are open to criticism. In general, selector theories are the beliefs realized by the methods of the natural sciences by means of controlled observation (Gellner 1974, 53ff). Selectors therefore have a constructive role in ascertaining orientations and escaping intellectual confusion.

Gellner discerns three cognitive selection procedures: empirical, or the theory that knowledge is experience and sensation ("the ghost"); material, or the theory stating that the onus of reality is an underlying, unobservable, structure ("the machine"); logical, or the theory that reality is homologous to grammar ("the skeleton"). The core of Gellner's is a demonstration that the alliance ghost-and-machine (or empiricism-and-materialism) inherited since the seventeenth century is inestimable, but such an alliance is too often confused by those people who want to defend or reject either the ghost or the machine in favour of the other.

As Gellner points out, empiricism is concerned with data, and its source. The ultimate picture drawn of reality is that it is manifold, qualitative, structureless. From an empiricist perspective therefore, determinist or causal models of explanation become mere conveniences: they are manageable and yet remain fictional pictures of reality.

Materialism, in contrast, is more concerned with explanation and its structure. It draws a picture of reality as a persisting and stable structure. From a materialist viewpoint, structures remain orderly even though sense-datum tends to show that matter evolves through time. Also, materialism hardly reconciles the idea of free-choice, of human beings as free agents, since determinist models of explanations underwrite materialism itself.

Gellner continues his argument by stating that empiricism (the ghost) is pervaded by two ideas which materialism (the machine) does not confront. Empiricism posits that experiences are reality. However, while it also posits that conceptualizations are the organizers of experiences and sensations, empiricism does not really concentrate on the problematic process of thought and representations. Yet on its own, as data in the pure, empiricism is doomed to lapse into diverse fictions. It can only reach the conclusion that different worlds co-exist but cannot give any independent, non-circular reasons why that is so. Ultimately empiricism can only state mere tautology or else truisms such as "different worlds exist because different people experience them" or "only experience is true and, therefore, it cannot deceive us".

Yet experiences and sensations are ultimately independent of our will and thought and therefore "attractive as cognitive arbitrators" of our concepts and theories. Epistemologically therefore, empiricism enables the selection and sifting of acceptable ideas, concepts, and theories from fraud; but as a source for rational explanation, it "does stand convicted of impurity" (Gellner 1974, 80).

Materialism, conversely, deprives the world of subjectivity. If one takes this view at face-value, nothing is to count as knowledge unless it has the form of an explanation in terms of a publicly available concept of structure. The extreme materialists, for example, conceive of "the mind" as genuinely formed by atoms related in a mechanic way; the mind is indeed reduced to atoms. Not surprisingly, such a vision disconcerts many, for it evidences a de-humanization of the world. Opponents of materialism would stress that mechanical explanations are conceived for convenience but are not true. Alternatively they would contend that a 'totality' comes prior to the 'parts', suggesting that an atomistic view is at best incomplete, at worse spurious. Thus as a selector set of norms, materialism is dehumanizing and precisely dismisses the richness of reality which it initially aims to explain. It is not more useful as an arbitrator set of norms, for it falls short of its pretences. However, it separates itself from sense-datum, which, unlike the invisible structure, is an observable and relatively independent criteria of validity. Therefore, in contrast to empiricism, it has the quality of not being tautological; for the idea of structures belongs to a second-level language, whereas experiences belong to a first-level, common-sense language.

Gellner's conclusion is that empiricism and materialism should not be taken as alternative candidates, but rather as cognitive selectors, each very useful in their own ways. "Empiricism is concerned with selecting information, whereas mechanism selects patterns of explanation. This might at least avoid or minimise the collision, by making the two doctrines live at different levels" (1974, 83). Therefore, to his mind, even though empiricism and materialism appear to clash and conflict with each other, there are, however, some circumstances where scientists and researchers should take advantage of their compatibility.

With regard to the project of a 'feminist biology', two remarks can be made at this point. First, it seems that even though feminist criticisms of biology appear both credible and indicate the relationship between explanations of women's behaviour and patriarchal biases in biology, yet they do not really provide any viable norms describing what a feminist biology would be. In this sense, feminist biology still bears more resemblance to a "negative endorser" body of theory, than to a fully-fledged new biology. Secondly, it appears that feminists' refusal to integrate causal or determinist accounts of behaviour was overstated, and that the exclusive recourse to "experiences" and "womens' subjectivities" in the construction of biological theory, although worthy as cognitive selectors of data and also as cognitive 'arbitrators', cannot possibly make for the need of a cognitive selectors of explanation ultimately. Let us look in this connection at Gellner's discussion of holistic and determinist models of explanation of behaviour, an issue of great importance, yet the source of conflicting arguments in the scientific critiques of several feminists biologists[10].

Gellner sheds light on the necessity of discerning in our assessment of scientific epistemology, between the respective virtues and limitations of holism and mechanistic reductionism as selectors and as arbitrators (1974, 83-108).

According to him, behaviourism, as the epitome of determinist causal models of behaviour, has taken the virtues of empiricism as a selector of information and turned it into a trivial model, void of explanatory power, while still subsuming the machine-metaphor[11]. But, while the empiricist pretence of behaviourism gives a feeble model of what will best explain intelligent behaviour, empiricism in this case may still supply a persuasive answer to the question of what kind of evidence can in the end settle issues about the nature of this world.

On the other hand, insisting on the complexity of human beings and their behaviours, as posed by the empiricist insight, does not logically imply the rejection of the machine-model. In this case, empiricism should only stand as a strong incitation to explain behaviours as complex structures and machines. Empiricism is only relevant in our decision about the kind and degree of sophistication of the structure which can best represent the pattern of behaviour under study. For, as Gellner stresses, only structures can explain in a non-tautological way. Materialism and structure-models are located on a different plane from the experiences (or behaviours) they aim to explain. As such, they function as a sort of second-level language, formalizing rather than experiencing.

> The norm of 'structural' explanation is imposed by the very nature of explanation, and not specifically by the nature of the material. Whether our behaviour be simple or complex, whether in our linguistic competence we make infinite use of finite means, or merely parrot-wise use finite means for finite ends -- either way, only a structure can explain, and as it is extremely unlikely that the structure is already explicitly present to consciousness, it follows that either way we are to be explained, in that sense, mechanically and heteronomously. (Gellner 1974, 96)

As far as feminist biology is concerned, the argument advanced by Gellner could be made as a point to rebut the holistic approach as a model of explanation, yet without dismissing its critical virtue, especially in opposing rigid and over-simplistic causal models of behaviour, and more importantly, as a selector of information for the creation of better bio-behavioural knowledge.

The foregoing arguments and their relevance for our research purpose can be summed up as follows. As Gellner concludes in Legitimation of Beliefs (1974, 206ff), experience, even though it is not pure, still remains the least corrupted of our validation grounds, and therefore must be deemed as salutary. On the other hand, the materialist-mechanistic approach, although it presents a rather dehumanizing picture of the world, remains the only non-tautological model of explanation. We are therefore constrained to use it. Thirdly, although relativism has proved to be a genuine problem for the epistemology of social studies, it may be seen a good "recipe" for context-bound ethnographic exploration within a limited sphere of cultural activities and when no fundamental clashes arise regarding the evaluation of our sets of normative cognitive values. As a 'selector of explanation' however, it remains ultimately a re-endorser of beliefs and cannot expand our knowledge of the world. Finally, thought and knowledge cannot dispense with sociology for it rests on norms of validation uniquely provided through culture and history.

The first three points may prove useful in showing that feminists biologists have misunderstood, in their elaboration of a feminist approach to biology, the logical consequence of a rejection of biological determinism in favour of holism, and of the structure metaphor or causality in favour of 'subjectivities' and 'life experiences'. A feminist holistic approach to biology is not incompatible with causal or determinist models, providing these are complex enough to reflect our views of human behaviour. Also, while relativism has provided feminists with a rich and accurate analysis of "patriarchal biology", it could only fall short of setting the norms from which a substantive feminist biology could be developed and justified. Finally, as far as Gellner's fourth argument is concerned, feminist biologists and critics of science have already made a powerful and convincing sociological analysis of the patriarchal norms and frames of mind operating in biology. But, as Habermas and Hesse also suggested, feminists have assumed incorrectly that all, rather than only some, of the norms that have been generated within patriarchal society need to be rejected and reformulated if a feminist and anti-sexist biology is to be created.

Summary

By discussing the development of cognitive norms, Habermas, Hesse and Gellner have helped us pose the question whether the alternative norms of biological research which feminist have proposed are adaptable to current "patriarchal" science, or whether feminist and mainstream biology constitute incompatible systems of knowledge[12].

It is my view that feminists, in spite of their strong commitments to values like harmony, ecology, and respect for people and nature, are, like 'mainstream biologists', committed to the utilitarian values of instrumental knowledge. They do so, however, in the interests of women and for the improvement of their social conditions, which imply, if not a drastic change in the value-goals of biology, at least alterations in value-laden theories. The argument of Habermas is relevant to this. Technical interests and causal models in science cannot logically be rejected by feminists since the two have played a crucial role in the development of health and social welfare and will likely continue to do so. Having said this, feminists severely question the cogency of using simplistic ('unidimensional') rather than sophisticated ('interactive' and 'holistic') models to explain human behaviour. The issue remains, ultimately, a matter of rational debate concerning the value-ladenness of theories, and is particularly seminal within the social and the life sciences, as Hesse would suggest. I would therefore argue that feminists have a strong and legitimate voice in critiques of scientific knowledge. But these critiques do not entail, as Gellner would argue, a rejection of the utility of traditional norms of justification, such as those of empiricism, or of models of explanation based on causal determination, to judge among competing theories those which are valid and those which are not.

We shall therefore close Part II of this thesis by re-examining biology specifically in order to locate, within biological explanations, the locus of the problem of social values. We should be able to ascertain the type of scientific value-judgements (value-goals/ value-laden theories) feminist critics have challenged in their critiques of mainstream biology, and the kind of contribution these critiques might lead to in the development of projects of feminist biology.

Endnotes

1) Indeed, as early as the turn of the century, the idea that natural science was the sole bearer of true knowledge, and its method, the sole guarantee of objectivity, was challenged by Duhem (in Harding 1976) and Bachelard (1934), to name but a few. They were both well aware of "built-in" conventions in theories, instruments, and observational statements.

2) For the natural sciences as such, Feyerabend (although feminists do not pay as much attention to his works) also held a radical standpoint, suggesting that knowledge, since it is empirically underdetermined, is justified ultimately on social beliefs and coerced consensus (Feyerabend 1975).

3) Popper sees the critical attitude as one of the three most important criteria which demarcate science from pseudo-science. These are: the falsificationist epistemology, justifying the idea of a progression in scientific knowledge via empirical testings; the degrees of corroboration, which are the canons on which the selection of the best out of several hypotheses are to be made; and the critical attitude, (including permanent doubt and bold conjectures) which discriminates scientific reflection from common sense or dogmatism (Popper 1969, 1972).

4) This does not mean that it is impossible, or even inappropriate, to explain human behaviour by external factors exercising a constraint on the individual. Weber, Durkheim and Parsons's works adumbrate this. Hermeneutics also implies such a logic of explanation. Hesse gives some examples borrowed from psychology and social studies which also confirm this. Habermas provides the historical and sociological conditions of possibility for such a framework to exist.

5) The works of sociologists and philosophers of the Frankfurt School are particularly germane in that respect. See Adorno and Horkheimer (1973); Habermas (1970,

1978); also in Jay (1973), Connerton (1976) and Larrain (1983).

6) In a collection of short essays entitled Towards a Rational Society (1970), Habermas discusses modern social trends such as "the scientization of politics and public opinion", technical progress and social life, "technology and science as ideology". In Legitimation Crisis (1978) he draws a diagnosis of current social crises. He finds that the roots of these crises lie in the realization of contradictions between the development of the cultural system, the political system, and the productive system.

7) Hesse remarks:

> Traditionally various versions of the Verstehen [i.e., interpretative tradition] thesis have been appealed to provide alternative goals, but it seems that in any attempt to understand a person's behaviour, one is seeking to fulfil one's expectations about his future behaviour. Indeed, all human interaction depends on the success of some such predictions about mutual responsiveness, and this seems not unlike an application of the pragmatic criterion. This is surely correct, and it is not surprising to find successful fulfilment of expectations as a criterion in all reasoning about the world, including all lowest level inductive generalizations, whether about objects or persons". (Hesse 1980a, 201)

8) In order to rationally evaluate the soundness of value-assertions in paradigms and individual theories, one may ask questions such as (in the area of neuro-psychology for instance), is it desirable or not that the mind be conceived of as a "natural" machine, or (in the field of sociology) is it desirable that the proletariat be freed and the class system be overturned?

9) Indeed, Gellner's perspective on systems of knowledge resembles that of Mary Hesse. Both conclude that scientific society is cognitively more powerful compared to its predecessors because it has created a cognitively useful separation between symbolic ideas and effective knowledge. They also agree that the science system of cognitive norms (or value goals) has lost the overview of a humanist pre-scientific society which referred to moral and social values as important components in the production of knowledge.

10) Gellner deals more precisely with the dispute between Chomsky and Skinner about verbal behaviour (Gellner 1974, 83ff). See Chomsky's review of Verbal Behaviour by B.F. Skinner in Language, vol. 135. January-March 1959.

11) As mentioned before, Gellner argues that the empiricist explanatory model of behaviour does not prove much. Moreover, as a model of explanation of behaviour, it is tautological. He says, for instance, that the mentalistic model of Chomsky can only answer to a question like 'why does an individual do this?' with a statement such as 'because that individual intended it for he/she is a complex being'. Conversely, the behaviourist model of Skinner can only make a trivial statement about that same question by answering 'because he/she received a stimulus from outside'.

12) It may be argued that Gellner does not really want to anticipate the possible

transitions of systems of knowledge, and that his reflection is basically historical and ex post facto. Yet the future is precisely what feminists, in contrast to the social anthropologist Gellner, have been mainly interested in as critics of science: the foreseeable transformation of the oppressive patriarchal system of knowledge. As an anthropologist, Gellner has possibly paid too much attention to 'remote' foreign cultures, and not enough to the control of 'foreign cultures' within his 'own society'.

CHAPTER 4

THE SCIENTIFIC DEBATES IN BIOLOGICAL SCIENCE

It is believed that a clarification of the epistemological arguments in favour of biological determinism or, alternatively, in favour of biological holism, will illustrate more directly the arguments made in previous chapters in relation to the strengths and limitations of feminist projects of biology. In this chapter references to internal debates in biology will be made in this connection. A discussion of the arguments raised by Radical Scientists and dialectical biologists in the 1970s and 1980s will substantiate our viewpoint on the importance of certain categories and models of explanation in biology with a view to the analysis of feminist biology. I suggest that feminist biologists cannot dispense with these categories as models, even though they criticize severely how and why these are currently being utilized in the 'biological method'.

The example of Radical Scientists indeed adumbrates what are, at present, the points of rupture and convergence between the norms of research practice defended by mainstream and non-conventional biologists. This should provide a blueprint for the comparative analyses of mainstream and feminist biologies which will be effected in Part III.

First, let us examine the epistemological questions induced by the scientific debates characteristic of biology. In doing so, we may recognise the extent to which some research problems biology borrows interchangeably from both the human sciences and the physical sciences; this is particularly the case in human biology. It also borrows, more fundamentally, some of its most important explanatory categories, such as teleological inferences which serve to hypothesize living systems, and functional causality which serves to represent the relations between the individual components in these systems. By the same token, we should come to understand more completely how and why biologists have so often become involved in controversies about the ontology of biological objects, the theoretical frameworks most appropriate to capture their complexity, the values entering theory-building, and finally, the politics of science and research agenda in human biology.

Historical and Epistemological Background to Scientific Controversies in Biology

A brief examination of historical accounts of the development of biology[1] indicates a great divide between two biological approaches, natural history and experimental biology. This divide corresponds partly to the epistemological debate nurtured by the opposition of teleological explanations on the one hand, and cause-effect explanatory models on the other.

The great divide in biology was resolved, in principle, in the 1940s, when the bases of an unificatory approach to the study of living forms, the "evolutionary synthesis", were finally laid out[2]. In practice however, the traditionally separated approaches seem to have pursued their own theoretical orientations irrespectively of the idea of an integration of biological disciplines. The approach of natural history, more observational, descriptive, and holistic, still seems to greatly influence ecologists', botanists', and development biologists' frames of mind; the experimental approach, leaning towards a reductionist model of explanation (and the idea of mechanistic causation) has been sustained since the nineteenth century, first in the field of physiology and, later, in biochemistry and molecular biology (Allen 1975; Capra 1983; Mayr 1982. See also the results of interviews with British biologists in chapter 6).

As several philosophers of biology have maintained (Grene 1974; Hull 1974; Nagel 1979; Rosenberg 1985; Ruse 1981, 1988; Zumbach 1984), the issues of teleology and reductionism loom large in the scientific debates relating to the question of what should be the nature of a biological approach.

The question of teleology refers to the necessity of adopting a means-to-an-end logic in order to fully understand biological phenomena and to make sense of them. Since the theoretical integration of biological disciplines of the 1940s, it has been assumed that all living organisms embody both the information carried over by history of their development ("ultimate" characteristics), and the observable features and motions associated more directly with their organic functions ("proximate" characteristics) (Mayr 1982)[3]. Teleological explanations are more often related to evolutionary theory (and to notions such as adaptation, fitness, selection pressures, or survival) than to any other theories (Hull, 1974). Yet, even though biologists might not be aware of it, the use of teleological principles in biological language extends beyond the boundaries of natural history and emerges in other biological disciplines such as physiology, embryology, and microbiology (Canguilhem 1977; Grene 1974; Rosenberg 1985).

The notion of teleology is often opposed to causality. Causal models hold a central place within the reductionist approach to biology. The reductionist approach is usually associated with experimentalism and the concerns of modern theorists and researchers working in the fields of biochemistry, molecular biology, and genetic engineering. In general, reductionism in biology presupposes the idea that atoms and molecules are the basic units of life. Such an assumption, in turn, predicates a corpus of analytical concepts and mechanistic theories. These concepts and theories aim at ascertaining the elementary causes of all biological 'events', from enzymes, proteins and cells, to tissues or organs, and finally, to illnesses, mental defects, and certain "adaptive" behaviours, as in the case of sociobiology[4]. In a strict epistemological sense, the problem of reductionism refers to a reducibility of all the necessary and sufficient conditions of explanations of biological phenomena into physico-chemical descriptions and laws[5].

The questions of teleology and reductionism however, neither are nor should be totally dissociated. As Grene puts it,

> What is needed for the adequate philosophical foundation of biological thought is neither to get rid of teleology, nor to rely on it as the self-sufficient partner of causality, but to supplement cause-and-effect thinking and means-end thinking by reference to the still more basic concept of standards or norms (1974, 37).

In fact, according to many philosophers such as Nagel (1979), Ruse (1981), Zumbach (1984) and Hull (1974), the clash between the reductionist biologists and non-reductionist is not fundamentally ontological, for the main reason that biology, like physics and chemistry, is first and foremost concerned with molecules[6]. The clash is rather methodological. As Hull explains causal reductionism in biology refers to explanations of the "structure and events at one level of organization [in terms of] those of lower levels" (Hull 1974, 132). In contrast, teleological explanations are those in terms of "the adaptive usefulness of structures and processes to the whole organism", or in terms of the "ecological function in the communities in which the species occurs" (ibid.)

In biology, he continues, molecular biologists may emphasize the causal ascriptions in biological systems, and organicist biologists, teleological ascriptions (Hull 1974, 132). The only difference apparent between molecular biology (or physics and chemistry at that) on the one hand, and evolution theory, ecology or developmental biology on the other is that the latter may more frequently require a model of synthesis, similar to that provided by teleological models, because of the existence of highly organized systems, and of various levels of organization in the systems they investigate. It seems, therefore, that looking at the history of biological sciences (and as far as scientific explanations and method are concerned) teleology and causal reductionism are in fact not opposites, but two sides of the same coin. The specificity of biology does not lie in the fact that its objects are ontologically different from those in physics or chemistry; rather, biological explanations, as Hull would argue, resort more often to teleological systems for heuristic models, than physico-chemical explanations because the subject matters in biology will more frequently be better represented scientifically as open systems conditioned by the environment, than the objects in physics[7].

But this methodological peculiarity of biology also appears to involve much more consequence from the perspective of a sociology of biological knowledge. It points to the fact that, as biological systems become more complex, as in human biology, evolutionary theory, or developmental biology, the values figuring in teleological model-building and causal model-building become more conspicuously sociologized.

Epistemologically, Hull maintains, causal explanations, like teleological explanations, are made against the background of a theory. In the former, background theory determines which items are to be considered in the causal network and how each is to be related to the others. In the latter the background theory determines what counts as the system, its parts, and its preferred states. Indeed, Hull continues, both causal and teleological explanations are based on considerable background knowledge. Yet it is the explanations rather than the background knowledge that biologists assume to be conveying all the informative power (Hull 1974, 122ff).

The situations to which causal descriptions apply are open in the sense that a causal ascription explicitly focuses on one strand of all those possible in the causal net, this strand being selected because it is believed to be more important. Everything else in the causal net is assumed as background knowledge. Hence, in such a scheme the specification of necessary conditions is implicit, assumed to be true a priori, and yet it contributes importantly to the explanatory power of the causal model. In contrast, situations lending themselves to teleological ascriptions are open "because the organization of teleological systems is maintained by a continual exchange of energy and modifications in the parts of the system" (ibid., 123). In these situations, the background knowledge is explicit, expressed in the teleological statements direct, while the additional explanatory-content (which may be causal as such) seems less informative. In sum, teleological explanations seem obvious, almost tautological, and as such, are believed to be less explanatory than causal models.

Epistemological opposition between teleology and reductionism, has had (as mentioned at the beginning of this section) long-term effects on the internal scientific debates of biology. The divide has been expressed under diverse forms, for instance between mechanistic biologists and vitalists in the mid 1800s[8] (Allen 1975; Capra 1983; Collingwood 1945; Mendelsohn 1977), or morphologists and physiologists in the late

1800s[9] (Allen 1975). Other debates emerged between organicists and mechanists in the early 1900s[10] (Allen 1975; Caron 1988; Collingwood 1945; Haldane 1931), and in the 1930s and 1940s between diverse schools of thought concerned with the reorientation of biological explanations and approaches in the wake of the evolutionary synthesis of modern genetics[11] (Abir-Am 1987; Ayalla and Dobzhansky 1974; Fuerst 1982; Mayr 1982; Olby 1974; Waddington 1969, 1974).

Since the 1960s, holists and dialecticians, on the one hand, emphasizing the idea of "emerging properties" of biological components and "interaction" between "levels of biological system", and, on the other hand, biologists leaning towards theoretical reductionism and determinism, seem to have taken on the debate (Chargaff 1978; Koestler and Smythies 1969; Maynard-Smith 1986; Rose, Kamin and Lewontin 1984; Rose 1982a, 1982b). This last debate reflects to a great extent, the ambivalence between a conceptual framework integrating simultaneously the ideas of both holism and causality in biology. Since the discovery of the properties of DNA in the 1950s, and with the ensuing spreading of the idea of the "central dogma"[12], genetic determinism and biological reductionism have increased their influence, becoming both theoretically and methodologically dominant research guidelines. During the 1960s, however, several well-known biologists, and psychologists, decided to reimpose both the ideas of holism, and of teleological explanations, in the elaboration of a more complete framework for the understanding of living forms. At a Symposium held in 1968, in Alpbach, Austria, Weiss, Bertalanffy, Koestler and Waddington, among others, discussed the principles of "new perspectives in the life sciences" which would extend "beyond reductionism" (Koestler and Smythies 1969).

During the course of the Alpbach Symposium, Weiss argued that the reductionist method neglects the study of levels of organization and the "emerging properties" of organic elements as they move from one organic level to the other. The approach of genetic determinism, for instance, glosses over the problem of multi-levelled organized living forms. Hence it is necessary in biology to rely on the system concept, and on an approach in terms of "stratified levels of determinism" (Weiss 1969, 15). There is, Weiss admitted however, a major difficulty with an approach of holism and systems: the gist of this approach reflects empirically the existence of genuine organic processes and transformations, but their properties are not tangible objects. The holistic approach is indeed concerned with the information content (i.e. the governing rules of equilibrium and coordination between organic parts and systems) of biological processes. In contrast, reductionism directly operates from observations of tangible organic forms and motions. And this implies that the theories developed from a holistic or systems approach are much more difficult to handle and to test experimentally than those developed from a reductionist approach.

In the same vein, Waddington sought to adapt the genetic theory of evolution to the perspective of a multi-layered system of spaces and selective pressures. He contended that the study of the impact of the environment on the actualization of genetic potentialities is a more sound line of research than genetic determinism. He believed that genetic theory needed to be expanded via an approach concerned with how the genetic information (the genotype) actually translates into characters (the phenotypes) which are active and interactive with their environment, rather than assuming that these are permanently fixed biologically.

All the arguments presented at the Alpbach Symposium manifested the need to reintroduce into biological thinking a framework of hierarchical systems of biological interactions between organic components. But one may ask: 'what was really the rationale underlying the defense of the non-reducibility of biological processes to physico-chemical concepts and laws'? Was it simply developed in order to construct a new methodological stance, including a theoretical approach and experimental models better suited to the idea of biological systems and interactions? Or was it to adduce the evidence of biological forms as indeterminate; as constantly transforming themselves?

Two epistemological arguments, previously discussed in chapter 3, might be raised at this stage in order to shed light on these questions. The first argument concerns the utility, in the production of knowledge, of the idea of mechanistic determinism and of causal models. Causal models, as we saw above, have not been, and cannot be, avoided in biological explanations, even though, empirically, a synthesis of results in terms of holism, interaction, and information, seems to be more complete. This position on biology would lean towards that of Gellner who argued that the machine-metaphor, even though it may sound inhumane (especially in studies of human behaviour), cannot be dispensed with in the explanation of complex phenomena.

The second argument explored the extent to which teleological references and assumptions enter theory-building in biology, and more importantly, the likelihood that social controversies might arise in biology over the utilization of certain teleological references. As we have seen, the value-assertions defining the 'preferred states' of living systems in biological theories may, in several instances, refer directly to political and social ideologies about 'the preferred states' of the social behaviours and the gender structure[13]. This position concurs with the argument of Hesse.

This second argument might also give an idea of the scope of hermeneutical reflection and the more encompassing socio-critique underlying feminist criticisms of science and biology, more especially of studies of sex-gender behaviour and sexual dimorphism. This means that the controversy about 'the preferred state of the patriarchal system' as highlighted in feminist critiques seems to have an impact not merely on one but on two aspects of theory-building in biology. There seems to be an impact on <u>the value-assertions of theories about women</u>, and more profoundly, on <u>the ontological property of subject matters in human biology</u>. That is to say, the feminist critiques appear to affect more dramatically the conventional 'methodological' position of biologists: it directly challenges both the teleological status (or properties) ascribed to biological 'objects' and systems (more especially to womens' physiology and behaviour and their link with womens' life conditions in society), and the methodological and theoretical tools traditionally used to investigate these.

Teleological systems indeed can be used to represent diverse systems oriented towards a goal, all of which do not necessarily predicate the ideas of intentions in human action and of cultural plasticity of behaviour. The principle of teleology in biology may refer, as several scholars have shown (see for example Ruse (1981), Rosenberg (1985), Mayr (1982)), to diverse objects or systems and, in turn, diverse strategies of equilibrium for these systems to maintain themselves. For instance, the same set of goals and adaptive strategies might not apply equally to physiological functions or to animal behaviour[14]. Similarly, the social sciences, biology, and, to some extent, physics lend themselves to theories or models of self-regulated systems in which individuals or particles interact rather than being determined unidirectionally by a cause. But this may well be where the analogy between biology, sociology and physics ends. The systems described in sociology, biology, and physics are intuitively and empirically distinguishable.

The systems described in biology and psycho-sociology are separated for one main reason. As Ruse (1981) points out, in biology, teleology is only heuristic, and refers to systems which appear as if they were designed towards a goal. In psychology and sociology, the teleology of intentional behaviour in humans is not a mere functional artefact, but really is "telic". In comparison, evolutionary genetics, population genetics, paleontology, physiology, embryology or ethology deal with objects which lend themselves more or less easily to teleological explanations, or else do not underline ontologically similar types of teleological references. Teleological references therefore, may have various substantive contents depending on the subject matter or the discipline from which they are being examined.

Teleology always refers, in the last analysis, to "the preferred state of the systems". As Schaffner (1977), for instance, argues, the definition of the boundary conditions and preferred states of teleological systems in biology is always of central importance. When it comes to human behaviour or human phenotypes, teleological models may always end up being overly sociologized. Thus, causal models in human biology are never too remotely situated from these teleological models of interactions and equilibrium which, as feminists have rightly pointed out, have been elaborated on the basis of androcentric and patriarchal beliefs about the social order and division of gender roles.

As a result, and as far as feminist critiques of biology are concerned, I would suggest that it is more an hermeneutical reflection about theory (and the preferred state of systems) and a socio-critique of the ontological status of biological "objects" in human biology and in studies of sexual dimorphism, than a strict rejection of causal models which are involved. I would further suggest that in their critiques, feminists do not necessarily reject the idea of causation in biology; they basically challenge the kind of properties ascribed to "objects" in human biology and studies of sexual dimorphism, and the links between these objects as they are construed in the teleological systems operating in biological theories. Feminists indeed criticize both the tangible content and the intangible properties (intentional, interactive, causal) of the links described in the teleological system elaborated in traditional biological theories. But they do not reject altogether the idea of causal models of explanation. They, rather, reject the rigidity of certain causal models and the teleological systems to which these belong.

In sum, on the basis of an epistemological and historical examination, 'mainstream' research in biology may be described sociologically as follows. On the one hand, all branches of biology with a few exceptions, from ecology to ethology and behavioural biology, through molecular biology and physiology, have become converging areas of research, if not already adjacent within the whole subject of evolutionary biology. This reality obtains in principle only however; for the exceptional success of genetic engineering and molecular biology, combined with the impulse of biomedical research, bear heavily on biological practice. This accords prime importance to experimental research and reductionist models of explanation, both pre-empting the holistic model and the ontology of living forms as highly interactive with their environment[15].

Debates between biological determinists, environmentalists, and interactionists have been going on for some time. The feminist critiques of sociobiology and sex-gender behavioural studies have, similarly, called into question long-lasting biological controversies. Feminists have, like several other groups of biologists, confronted the ontological and theoretical underpinnings of traditional research in human biology; but, more specifically, they also have unveiled the patriarchal nature of certain theoretical assumptions. Very importantly as well, these scientific controversies seem to have had (in contrast to physics or chemistry) a non-negligible resonance in social debates outside the strict confines of the biological sciences themselves. Applications of biological theories to medicine, psychiatry, pharmacology, reproductive technologies, and industrial production, have stirred the criticism of large segments of the public not only with respect to the abuse of biological discourse in the political arena but also with respect to the truth-content of certain theories about human biology and behaviour (Arditti and others 1980; Goodfield 1977)[16].

Epistemological insight into biological thinking is particularly interesting for sociologists for it reveals that the patterns of explanation used in biology are not 'closed systems' as in physics, but somehow 'open patterns' of thought closely related to social ideologies. These patterns always involve references to the 'preferred states of nature', including organs, individuals, species, and social organization; to the "normal" and the "abnormal" state of nature, as Canguilhem puts it (1977).

In fact, at the core of the bio-behavioural models used in ethology and evolutionary theory, and in other studies of the biological determinants of phenotypes and human characters, dominant ideologies are directly involved in the scientific reflection about biology's explanatory models. As a result, in societies where social control is largely effected through the diffusion of patriarchal myths and ideology, the definition of (teleological) norms of behaviour within biology itself emerges as a fundamental sociological issue[17]. As such, both scientific and social issues may interfere directly in the choice of value-assertions ascertaining research questions and individual theories concerned with human biology.

Bearing this in mind, we shall, in the rest of this thesis, explore the norms of research practice and models of explanation used by mainstream and feminist biologists. This should enable us to see the extent to which representatives of these two groups reproduce or else confront certain dominant beliefs and, as such, repress or activate both scientific and socio-political debates within biology.

Radical Scientists and Dialectical Biology

The case of Radical Scientists appears as prima facie evidence of how tightly intertwined can be the biological and socio-political value-assertions entering theory-building in biology. This, in turn, displays the extent to which in the process of justification of biological theories, biologists are often forced to use arguments borrowed from the 'soft', observational sciences; yet always they return in the last instance, to the canons of justification of 'hard', experimental sciences. The attempts of Radical Scientists to elaborate the approach of a 'dialectics of biology' is a good example of this.

Preliminary to the comparative study of the practices of biologists in Part III, the examination of Radical Scientists' critique of biology is very useful for two reasons. First, since the bulk of our empirical material is British in provenance, it appears appropriate to examine the works of British Radical Scientists, particularly those of the neuro-chemist Steven Rose. Secondly, since the feminist biologist Lynda Birke has herself been a militant Radical Scientist, the parallel with Rose is likely to be uniquely informative.

The Historical Background

Radical Scientists inherited their political militancy from the National Union of Scientific Workers (NUSW) and from its most prominent leader, the socialist scientist J.D. Bernal (Rose and Rose 1969). Bernalism, bearing the concerns of its time, of the inter-war years, was mainly concerned with the abuse of a science practice constrained by capitalist interests and military purposes (Bernal 1939). It never reached, however, the stage of a hardcore critique of the intrinsic philosophy and theoretical value-assertions of modern scientific activity which Radical Scientists would undertake in the 1960s and 1970s (Rose and Rose 1969, 1976a, 1976b).

The reflections and activities of Radical Scientists in the 1960s and 1970s were geared to the use and abuse of capitalist science. They denounced science when it cautioned the goals and interests of a capitalist system of production and the oppression of minority groups. And they made an effort to democratize science for the benefit of all, pointing to the scientific flaws of certain research programmes, and participating in the diffusion of a "science for the people" thereof.

The works of the sociologist and biologist Hilary and Steven Rose are, in Britain, paramount in this connection. Both have been involved in the movement British Society for Social Responsibility in Science; both have contributed to the left-wing periodical Science for People; both have published or collaborated in several collections of essays on science and society.

As Marxist socialists, Radical Scientists consider that science is linked to, rather than autonomous from, politics and the economy. They also borrow from Engels the idea of dialectics of nature: as society evolves upon its internal contradictions, so science changes upon its own, and so nature constantly transforms itself (Levins and Lewontin 1985; Rose 1982a, 1987; Rose and Rose 1976a, 1976b).

According to Radical Scientists, biological science uses the methodology of reductionism (i.e. experimentalism) for the wrong ends. That is to say, even though they consider the methodological tools provided by reductionism as crucial for the development of scientific knowledge and technologies in the interest of all, as a philosophy, they perceive it as flawed. Biological reductionism "explains away" people; it focuses on individuals instead of pointing to external factors; it blames the very victims of unequal social conditions instead of revealing social injustice.

Under the guise of biological determinism of mental behaviour (or "biologism"), the philosophy of biological reductionism has become the tool of a policing State hiding its real colours behind the pretence of scientific objectivity. It represses the consciousness-raising of oppressed minorities such as women, blacks, deviants and convicts, themselves the targets of reductionist biological theories of behavioural disorders or intellectual inferiority[18].

In sum, Radical Scientists conceive reductionism as flawed in three respects. As a research programme, it uses its scientific authority to justify a theory of social life rooted in the ideology of dominant social groups and maintaining the oppression of social minorities. As a methodological (and experimental) standpoint, it must be seen as limited, especially relative to theories of behaviour. As a philosophy (or epistemology), it posits incorrectly that causal determinism and the machine metaphor reflect the real nature of living forms. Rose and Rose have indeed commented that biological reductionism is a "poor science experimentally", but moreover, as a theory of social life and a philosophy of nature, it is "bad science ideologically" (Rose and Rose 1976c).

Steven Rose has advanced a more systematic argument. As an epistemology, he points out, reductionism contains four flaws which "are far from being trivial" (1987, 28ff). First, reductionism incorrectly assumes an ontological priority of lower-level biological components (such as genes); secondly, reductionism operates a reification tantamount to the "localization fallacy" in the investigation of brain and behaviour[19]; thirdly, reductionism leads to the arbitrary quantification of human characters (such as aggression or intelligence), and moreover, to the separation of these human properties from the social context in which they take shape; finally, reductionism projects human qualities onto those of animals and then uses the animal behaviours thereby identified to reify the idea of "a human nature" as biologically determined once and for all.

Radical Scientists have in fact elaborated their critique of science with a view to biological science more particularly. Under its form of biologism (or biological reductionism) modern biology appears as the epitome of a science bearing the imprint of dominant ideologies, and this at all levels, institutional, theoretical, methodological, and epistemological (Birke 1984a; Birke and Silvertown 1984; Rose 1982a; Rose and Rose 1976a; Rose, Kamin and Lewontin 1984).

Yet, not content with criticising current biological science and its bedfellow, biological reductionism, Radical Scientists have in the 1980s, embarked on the elaboration of a new approach, the "dialectics of biology". Steven Rose with his colleagues at the Open University, among them Lynda Birke and Patrick Bateson, and also Leo Kamin, Richard Levins, Richard Lewontin, and Ruth Hubbard in the United States, have been the main proponents of this new approach to biology. This undertaking culminated in the convening of a conference in Bressanone, Italy, in March 1980.

The Bressanone Conference was rather informal, attended by roughly fifty scientists, mostly biologists but also including sociologists, psychologists and mathematicians. The majority came from Britain and America, and some from Europe and Australia. During this conference, participants in the "Dialectics of Biology Group" (as they would call themselves) discussed the limitations of the "philosophy of biological reductionism", more specifically in relation to neurobiology and human behaviour (Rose 1982a, 1982b). The idea of a dialectics of biology was referred in the main, to the dialectical nature of "the relationships between biology and society" in the production of beliefs and of behaviour (see also Birke and Best 1980a).

One of the conclusions of the conference was that neither "environmental" reductionism nor "biological" reductionism constituted sound approaches to biology. For 'dialectical biologists' the existence of a material basis of bio-behavioural phenomena did simply not allow for a view of biology as totally determined by external, non-organic, factors. Still, their main concern was to find a replacement for the idea of an 'allegedly fixed biological basis' of living matters, since, in human biology more particularly, that idea literally meant to divorce biological development of phenotypes and patterns of behaviour from a series of factors which might precisely explain these (e.g., familial, cultural, social factors) (Birke and Best 1980a)[20]. Biology, they argued, definitely needed a new theory of 'human nature' and this theory could only be based on a dialectics between biology and environment. (Some dialecticians were even more radical, arguing that the idea of an 'human nature' itself is intrinsically oppressive and must therefore be avoided (ibid.)).

More recently, the idea of a dialectics of biology has been expounded at greater length in the works of Rose, Levins, Lewontin, and Kamin (Levins and Lewontin 1985; Rose, Kamin and Lewontin 1984; Rose 1987). The approach in terms of a "translation relationship between correspondents" was believed to change biologists' frame of mind, and to lead biology beyond the philosophy of reductionism. Let us thus examine the extent to which this approach has extended beyond the parameters of conventional debates in biology and might contribute to an improvement of scientific knowledge of human life and nature.

The Dialectics of Biology: Theoretical, Ontological and Methodological Tenets

A dialectics of biology would replace reductionism with a view of nature in which organisms and the environment are not separated, but, rather, influence one another in a dialectical movement, and might even alter their respective 'natures'. In their writings, Rose, Kamin, Levins and Lewontin give several examples showing that nature actually is heterogeneous, and that natural objects are highly interactive and constantly transforming themselves. For these biologists, the need is urgent for biology to develop new concepts and a new methodological programme in order to capture the real character of life and nature[21].

As an alternative approach, the dialectics of biology would assume that properties of 'parts' (e.g., genes or neurochemical pathways in the brain; human behaviours and phenotypes) do not exist outside of their environmental context, outside the 'whole'. Likewise, it would not assume that properties of the 'whole' account totally for the properties of the 'part'; but, rather, that 'whole' and 'part' would contribute to shape and alter one another. In short, a dialectics of biology entails both an ontological and a theoretical renewal of biological thought.

In fact, dialectical biologists are much more sophisticated, if not eclectic, in their scientific enterprise. A genuine understanding of biology, they argue, should concentrate on the ever-changing form of natural objects rather than on the idea of their constancy and permanent form. Heterogeneity and contradictions between components of a living system, they contend, are normal rather than exceptional conditions of nature (Levins and Lewontin 1985)[22].

In their view, a holistic approach to biology is not radical enough; for it neglects the presence of contradictions and the ensuing transformations of living objects. As they write,

> For us, contradiction is not only epistemic and political, it is ontological in the broadest sense. Contradictions between forces are everywhere in nature, not only in human social institutions. (Levins and Lewontin 1985, 279)

For dialecticians, change is the "normal condition" of life and nature, while "constancy" is only apparent, a scientific artefact (ibid., 277). Biological objects are heterogeneous; they bear their own contradictions and self-negation, and these are the reasons why a sound approach to biology ought to lean "toward the explanation of change in terms of the opposing processes united within that object" (ibid., 278). By the same token, and from an epistemological viewpoint, dialectical biologists wish to warn "mainstream" biologists to be more modest in their scientific claims. Since nature and human biology are constantly transforming themselves in interaction with the environment, then no one, they contend, can predict with certainty the development of living forms[23].

Several arguments in favour of dialectics of biology however, present logical inconsistency. It is, also, not entirely clear how the conceptual tenets the authors propose may be implemented; in fact, in the present context of experimental research and methodological rules in the empirico-analytical sciences, the conceptual edifice of a dialectics of biology seems to collapse. The position of dialectical biologists and the credibility of dialectics as a fully-fledged approach or methodology are thereby weakened.

Several times also, animal or plant 'behaviour' is cited in order to buttress the dialectical problematic of a biology of human behaviour. It is surprising that, being themselves aware of the ontological difference between human behaviours (i.e., teleological systems based on agency or rational action) and non-human behaviours (i.e., teleological systems of cell physiology, internal metabolism, or selective behaviour)[24], dialecticians nevertheless seek recourse to such examples.

One of their weakest arguments in this respect is, in our view, symptomatic of their political discourse about the biological basis of behaviour. The example, drawn from Not in Our Genes (Rose, Kamin and Lewontin 1984, 286-7), reproduced in The Dialectical Biologist (Levins and Lewontin 1985, 273) runs as follows: no one can fly individually or in a crowd, yet technological advances in a society (i.e. property of the whole) give individuals the property of flying[25]. It seems fairly obvious to us that the latter example is a spurious illustration of the interactive properties of objects predicating the dialectical approach. Evidently it is not individuals which fly; it is airplanes! In this instance, the flaw in the argument stems from unwarranted logical leaps between symbolic representations ("we fly" because of aircrafts), linguistic conventions ("we fly"), and properties of things in themselves (human beings can decidedly not fly; human anatomy militates against this).

Although the above example ought not to be taken as a disproof of dialectical biology, one may suspect the rhetoric underlying some of the arguments of dialectical biologists. One might be led to suspect, in fact, that as far as human biology is concerned, the discourse of dialecticians aims primarily at the political rhetoric relating to a biological determination of behaviours, and only secondarily to the scientific debate about biological determinism as a methodology. One might, therefore, wish to examine further the extent to which a 'dialectics of biology' really imposes itself as a scientific alternative to reductionism.

In this respect, it is important to remember that as was posited at Bressanone, the reformulation of scientific tenets of human biology and study of the mind does not consist in the rejection of reductionism cum biological constraints altogether. Rather, it involves considering a fairer balance of biological constraints, "reaction norms" (i.e., social determinants), and "human freedom" and synthesize all these under a single framework of analysis of human behaviour (Rose, Kamin and Lewontin 1984). An idea such as that of 'dialectical determinisms' may in fact be the answer to the practical difficulties entailed in the dialectical approach to biology. For this reason it seems, dialecticians have introduced the notions "hierarchy of determinations", environmental feedback, and more importantly, "epistemology of translation" (Rose 1987, 5; Rose, Kamin, Lewontin 1984, 87)[26].

But what does it mean, in practical terms, to have a "translation epistemology" of different levels of "determinations"? Does it help to sustain the explanatory power of a dialectics of biology; or does it only try to expand the theories developed within conventional biological methods? Does it really provide a new epistemology for biology? Is it revolutionary with respect to holism or to biological determinism? These questions are particularly important to address for they would help to test whether a "dialectics of biology" can be considered as a fully-fledged alternative approach to current reductionism or if it only stands as a dogmatic position within the biological sciences.

Translation Epistemology in the Dialectics of Biology: a Failed Attempt to Ground an Hermeneutical Analysis in Biological Empiricism

In their attempt to develop a multi-level analysis transcending the conventional descriptions of biological reductionism, dialectical biologists have proposed a "translation epistemology". This principle posits that descriptions of objects at one level of the biological system are not reducible to those of other levels, but, rather, that they may be "translated". On the basis on this principle, it is argued, biology should be able to generate a more complete picture of reality. Biological explanations, dialecticians maintain, are partial, because the philosophy of reductionism forces scientists to focus the explanation on certain objects or functions alone. In contrast, according to a translation epistemology, biologists would produce a more accurate understanding of living objects, looking at the "whole" rather than at parts alone, thereby shedding light on the relationships between different levels of biological phenomena.

Let us examine another example revealing the rationale behind the "translation epistemology". This example is drawn from Rose's Molecules and Mind (1987), and deals with memory formation[27].

Memory, Rose says, includes at least two processes: learning something about the surrounding world; and recalling (or remembering) that thing at some later date. What lies between the learning and the remembering is assumed to be some permanent record, a "memory trace" or "engram" within the brain. Biochemical "engrams" on the one hand, and the behaviour reproduced by memorising on the other, can both be subjected, according to Rose, to a translation epistemology of "biological correspondents" since they both describe the same phenomenon, memory formation, but at different levels.

In Rose's experiment, two groups of chicks are trained to peck at beads: some beads are water-coated, some others are bitter, methylanthranilate-coated. It is expected that the first group will peck at dry beads later but not the second group. In other terms it is assumed that the chicks will have learned "not to peck" at the bitter beads.

Standard neurochemical techniques were used to measure whether the amount of glucose utilized by the brain cells of the two groups of chicks was significantly different in three specific regions of the cortex associated neuroanatomically with different behavioural responses. Six experimental criteria were formulated in order to test whether memory indeed corresponded to the metabolic biochemistry of "engrams"[28].

Does the "epistemology of translation" in the above example represent a new epistemology? Does it vindicate the approach of dialectics? Does it even stand as a new research programme for the biological sciences?

First I do not see what is so uniquely novel about a description in terms of "corresponding levels of language" in comparison with a conventional description of the biochemical and behavioural manifestations of memory formation. The effect appears purely an artefact of language.

I would even contend that the term "translation" is actually a misnomer in the present case; is it not, in fact, two different objects that Rose has described here, the chemical reactions on the one hand, and bodily movements on the other? If so, then the example does not qualify either logically or empirically, as a dismissal of reductionist models of explanations[29]. One would need a different kind of argument to sustain the irreducibility of chick's behaviour to biochemical markers.

My second counter-argument is borrowed from Arbib and Hesse's discussion of the link between brain theory (or cognitive science) and a psychology or sociology of mental behaviour (Arbib and Hesse 1986). Arbib and Hesse maintain that neurons descriptions and mental descriptions are complementary, yet neither identical nor interchangeable[30]. Neuro-chemical pathways form the material basis necessary for mental behaviour to occur. But mental activity (and behaviour at that) can only be properly understood in terms of human volition, representations of objects in everyday life, and social norms or expectations.

For Arbib and Hesse, it seems that the idea of a reductive science, incorporating the results of both neurochemistry and psychosociology is not as 'dangerous' as Radical Scientists would allege. For them, reductionism in this case would indeed help for a rapprochement between the sciences (or levels of analysis) concerned with cognition and mental behaviour. Reductionism ought not to be construed as a reduction of the concepts of one science into those of another. Empirically, even a fine microanalysis of lower level neurochemistry will never provide the additional information or predictive accuracy of a theory of mental representations for instance. Indeed, from a philosophical point of view, Arbib and Hesse maintain that the "scientific study of the brain will need both the 'high-level' language of [schema] theory to describe mental states and behavior... and 'low-level' language to describe the anatomy and physiology of neurons" (1986, 71). This does not seem to diverge drastically from the point of view of dialectical biologists: Arbib and Hesse simply do not refrain from using the word 'reductionism' to mean a 'rapprochement' of the levels of analysis of brain functions and mental representations, while dialectical biologists prefer to use the term 'translation' to mean the same thing!

As a result, I would contend that Rose' s "translation epistemology" does not differ so much from the views of Arbib and Hesse or of any biologist proponent of a realist epistemology. They would all certainly agree with the idea that a progress of scientific knowledge about human biology and human action lies in the impetus to produce "a bridging theory that can provide a coherent level of description", to use Arbib and Hesse's formulation, between cognitive studies of the brain and social studies of behaviour (ibid., 71). In brief, scientists and philosophers who claim a materialist and realist epistemology, would conceive of organic and mental phenomena as interconnected spatio-temporarily, but this would not prevent them from distinguishing organic components and mental components for purposes of explanation (i.e., methodological), for ontological reasons, and, in the case of Radical Scientists more specifically, for political reasons. It is true, however, that Radical Scientists seem to be among those few scientists who aim at dismantling the institutional barriers preventing the very integration of social studies and the natural sciences, and who are prepared to fight for the creation of the organizational conditions favourable to the elaboration of a genuinely new research agenda for biology.

In the last analysis, therefore, Rose's "translation epistemology" appears to simply be a discursive strategy challenging crude biologically reductionist interpretations of human phenomena. His actual investigation of the different levels of biological phenomena lies entirely within the confines of conventional realism and the methodology of biology as an empirico-analytical science. As far as this is concerned, I see in Rose's views essentially a reemergence of the scientific problematics discussed at the Alpbach Conference.

In fact, Rose's interpretation of animal behaviour allegedly justifying his "dialectical framework" to human biology and behaviour seemed to me less unconvincing, than readily mischosen. I can only see a dogmatic (or political) rationale behind his adamant opposition to biological reductionism as an explanation of chicks pecking or not pecking at beads![31]

As for his proof that dialectics and translation stand as sound alternative approaches to human biology of the mind, I do not believe that his arguments are strong. In fact I would argue that if there is a place for a defense of dialectics in biology, it is not to be found in Rose's experiment on chicks, but rather in a good hermeneutical argument about the 'real nature' of organic and non-organic determinations on human biology and behaviour.

Summary

In this chapter, we have highlighted the parallels between some important scientific issues addressed within the history of 'mainstream' biology on the one hand, and 'dialectical biology' on the other, with a view to the analysis of feminist biology. We have noticed some contradictions in the scientific discourse of dialectical biologists. We showed, for instance, that they tend to gloss over heterogeneous examples relating to human behaviour and plants or insects, but also, at the same time, to emphasize the specificity of human biology and action with respect to sociopolitical factors. We observed that to presuppose constancy and determinations of biological forms is a methodological pre-requisite for experimental biology, even though, theoretically and ontologically, living forms, and more importantly, human beings, appear, in reality, to be interactive and permanently self-transforming "objects". Thus we would qualify dialecticians' opposition to the theory of biological reductionism as a rejection of rigid determinations but not of biological determinations in general. Indeed, dialectical biologists, like scientists in general, have a minimal faith in scientific realism: they believe that the methods of empirico-analytical science may help in the discovery of better explanations about nature and human biology.

I would like to suggest, therefore, that from a sociology of scientific knowledge, the position of dialectical biologists towards biological reductionism does not diverge so much from that of a great many mainstream biologists. For if the former make a strong stand in favour of a dialectical and interactive ontology (or view of the intrinsic characters) of biological objects and processes, the latter also accept this view to a great extent, as will be shown in chapter 6. Also, even though Radical Scientists emphasize the limitations of methodological reductionism, namely experimentalism, they are far from dispensing with it. Likewise, many mainstream biologists will interpret very cautiously laboratory results which, as they admit, do not replicate well what is actually going on in nature. They only continue with experimentalism because it is more manageable and is the surest way for a biological explanation to earn (at least partly) credibility.

Having made these clarifications, I would argue, on the other hand, that dialectical biologists and mainstream biologists diverge on the issue of militancy and research orientations. That is, from a sociological (or institutional) viewpoint, dialecticians' critique is an impetus to transform the norms of practice both scientifically and politically. Dialecticians are very critical of the research agenda of biology, which, they believe, is less oriented towards human welfare and the unveiling of the social sources of human diseases, than to industrial pay-offs or institutionally organized social control. Also, with regards to the politics of science, the former are openly socialists[32] and politically active. The latter would, in contrast, generally abide by the institutional rules of practice, notwithstanding the criticisms they make in private circles.

Since the current research agenda of biology tends, according to dialecticians and Radical Scientists, to exploit the flawed philosophy of biological reductionism and to maintain public beliefs in the authority of scientists on knowledge; the former consequently feel prompted to voice their disagreements. In sum, their dialectical viewpoint makes Radical Scientists feel especially concerned with the powerful authority of biology in the sphere of behavioural studies on the one hand, and, not least importantly, in sociopolitical debates and on commonsense beliefs. In contrast, mainstream biologists do not seem to think that these social concerns are matters of professional responsibility.

In brief, Radical Scientists' critique of reductionism is motivated partly on methodological and theoretical arguments (and these are justified scientifically but do not resolve, in the last analysis, the fundamental problems of biological epistemology), and also partly on political grounds, this time with a view to the social responsibility of the scientific milieu in the social arena, and, lastly, to the opening up of disciplinary barriers in biology and social studies. From a strictly scientific viewpoint they argue for a reinstatement of the concepts of holism and dialectics at the levels of experimental design, observation protocols, and the interpretation of results[33]. But they make such a strong case against the political abuse of biological reductionism as a model of explanation of social (i.e., class, sexual or racial) differences that this tends to overstate the real scope of biological dialectics as an alternative to methodological reductionism. As such, this accords more weight to the idea that Radical Scientists and dialectical biologists diverge from mainstream biologists much less on scientific matters proper (their disagreements relate to rather "conventional" debates in biology) than on political matters such as research priorities and disciplinary jurisdictions.

Finally we saw that dialectical biologists have not convincingly come to terms with the epistemological dualism relating to studies of human biology and action. For their dialectics of biology is not, in the last analysis, a genuinely new epistemology; it remains basically the realist epistemology of empirico-analytical science.

It is undeniable, however, that the root of their opposition to biological reductionism lies in the blurred epistemological space occupied by biological studies of human action, a space where social values enter directly theory-building and models of 'the preferred states of human systems'. It is also in this epistemological 'niche' that feminist biologists have contributed most originally to certain specific areas in the field of biology and may continue to do so, as our forthcoming comparative study will endeavour to show.

Endnotes

1) Obviously, this sketchy review is not exhaustive, neither in terms of the disciplines covered nor of the scientific debates discussed; on the other hand it highlights the major scientific and epistemological problems encountered in the development of biology, problems of major interest with respect to the sociology of knowledge and the feminist critiques of biology.

2) During the first third of the twentieth century a deep gulf still existed between experimental geneticists and naturalists. They were talking different languages, using different concepts, asking different questions. It was believed that in order to unite the two camps, geneticists had to accept the limitations of their explanatory framework and laws of heredity, and become interested in parameters relating to evolution, natural selection, and systematics. On the other hand, naturalists had to integrate the idea of genetic mutations into their interpretation of evolution. The unification of the two approaches came about in 1947 after a dozen years of scientific exchanges between the two camps, under the designation of "the evolutionary synthesis". The term refers to J. Huxley's book of 1942, Evolution: the Modern Synthesis. See Allen 1975; Mayr 1982; Webster 1981.

3) These two broad categories of characteristics point to the root of the opposition between teleologists and reductionists. Having said this, and as several biologists and philosophers would suggest, biological objects can be examined from more than two different perspectives: one can focus, for example, on the function, on the structure, or on the phylogenic transformation of these objects. See Grene 1974; Rose 1987; Rosenberg 1985; Ruse 1981 (see also note 21).

4) As shown in chapter 1, sociobiology posits that selected behaviour is to be explained by the possession of certain genes. This is the epitome of a 'reduction' of the adaptive behaviour of species (usually construed in terms of teleology) to the survival of the best genes. This logic of explanation may, however, be easily turned upside down (as in Wilsonian sociobiology) and mistaken for a causal model in which behaviours and organisms are 'caused' by genes.

5) Ayalla and Dobzhansky (1974) distinguish the three different domains to which a discussion of reductionism in biology may pertain: ontological, methodological, and epistemological (or theoretical). Epistemological reductionism is the domain most often discussed by philosophers: it refers to the issues of derivability and connectability of biological theories from the laws of physics and chemistry.

6) Dealing with the question of teleological explanations, organic systems, and reductionism, Marjorie Grene (1974) comes to a different conclusion. She holds that biology is ontologically, as well as methodologically specific.

7) As an example of this, we can imagine at least five instances in which the notion of teleology emerges. First, it appears in studies of evolution of organisms, from simple forms to complex ones, from phyla to species. Teleology is mere speculation in this case. In the second instance however, the theory of natural selection, speculation is limited by some conditions and 'laws' that govern the evolution of certain species and rule over our interpretation of paleontological data: these 'laws' are, for example, those of "fitness", "adaptation" and "survival". Teleology may refer, in a third instance, to the explanation of regulation mechanisms and organic functions of certain living forms and organs. Fourthly, teleology may refer to a 'multi-tiered' system. As cells become tissues, then organs, then organ-systems, the cell develops in a hierarchical order of sub-systems and is activated according to different equilibrium strategies. Finally, and as natural historians and evolutionists have pointed out, a teleological system may contain contradictory forces: for instance, in some theories of natural selection, for instance, individuals and species are defined as having distinct needs and reacting with different strategies of survival (e.g. what is good for the survival of the group is not automatically good for the individual).

8) Vitalism was a perspective on biology which assumed the presence of some 'vital force' intrinsic to living organisms and not reducible to the categories of physics and chemistry. Driesch and Bergson held this view, until it eventually faded into oblivion in scientific circles. As for the main proponents of a mechanistic view of living forms, there were von Helmholtz, Brucke and the Berlin School of "medical materialists" in the mid-1800s, and, at the turn of the century, the embryologist Roux and the physiologist Loeb.

9) At the end of the nineteenth century, morphologists, who were mainly interested in speculating about the origin of specimens and species, were harshly criticized by physiologists who defended the experimental approach in the name of scientific objectivity.

10) The scientific authority of mechanistic biologists was seriously eroded in the 1920s and 1930s when Sherrington, Speeman and Haldane (to name a few) developed the conceptual bases of an approach in terms of holism, directed to capturing the highly interactive character of living forms. The notions of "emerging properties" and "hierarchical

system" of organic components in nature resurfaced, but in a less metaphysical fashion than vitalists would have argued.

11) The major debate in those days arose in molecular biology between the structural school (with Pauling, Bernal, Perutz, Hodgkin, Bragg and Astbury), and the informational school (with Schroedinger, Bohr, Delbruck, Luria, and the "Phage Group").

12) In the production of DNA, the process of translating nucleic acids into peptides and proteins was proved to be uni-directional rather than bi-directional. This discovery is commonly referred to as the "central dogma" in molecular genetics.

13) We are borrowing the terminology of "goals and preferable states of the system" from P. Machamer. "Teleology and Selective Processes" in Logic, Laws, and Life, R.G. Colodny, ed., 129-42 (Pittsburgh: University of Pittsburgh Press, 1977).

14) We should like to mention, here, that Ruse (1978, 1981, 1987) and Rosenberg (1980) also argue, rather surprisingly, that human sociobiology is a fully authoritative scientific discipline.

15) The debates revolving around the priorization of bio-medical research and molecular biology have indeed extended into the public domain. Every week, newspapers publish articles in this regard. See for instance, the debate between S. Rose and D. Weatherall in "Free Speech: Does medical research focus on the real cause of disease?" in The Independent, 17 February 1990; and N. Russel "Biology and Politics" in The Independent, 26 February 1990. See also the comments of biologists on this issue in chapter 6.

16) With reference to genetic engineering and the resurgence of eugenics, we would especially refer to the critiques of Radical Scientists presented in the next section. See also Arditti and others (1980), Goodfield (1977), Roll-Hansen (1988), Wright (1986).

17) As the paleontologist S.J. Gould wrote, "Evolution is one of the half-dozen shattering ideas science has developed to overturn past hopes and assumptions, and to enlighten current thoughts" (1985, 14). The notion of evolution, indeed, has a personal appeal for individuals, for it speaks directly to us: how did we arise; why do we differ as human beings? In this sense the value-assertions entering theory-building in evolution theory and human biology are likely to remain at the core of many political controversies, both within circles of biologists and among the lay public.

18) In their introduction to the collection of essays The Radicalization of Science (1976e), Rose and Rose propounded the five tenets of a programme of Radical Science. First, a Radical Science should explore the ideological components of Capitalist science, and complementarily, ask what a socialist science would be. Secondly, the goal of breaking down the barriers between expert and non-expert should remain paramount. Yet Radical Scientists should remain cautious with projects such as "Science for People", for within bourgeois society, popularisation of science often carries more emotional charge than substance; that forms a third tenet. Fourthly, it is crucial that Radical Science raises the consciousness of all scientists, in order to make them realize the dual role of science, rather

than let them perpetuate the sense that scientific knowledge is objective, salutary and "above ideology". Finally, Radical Science should seek to achieve a less sexist practice. This, it is held, has the valuable property of stimulating self-reflexivity within the movement of Radical Science itself. It also constitutes a new angle from which to analyse the political implications and forms of social control via science.

19) For instance, the source of "aggressiveness" should not be seen in individuals, in their genes or in some area of their brains. In <u>Not in Our Genes</u> (1984, 145-46), Rose, Kamin and Lewontin contended that, despite the fact that particular brain regions can be associated with certain behaviours, <u>none</u> of these areas is <u>sufficient</u> as an explanation of these behaviours, referring to the "localization fallacy" of biological reductionism in studies of behaviour.

20) Indeed, since dialectical biologists argue for an integration of nature and culture, this raises a problem in terminology. Unwilling to separate 'biology' from its 'environment', the qualifier 'biological' itself becomes blurred.

21) It might be useful at this point to present the classification of Kamminga who suggested distinguishing the approaches of five groups of biologists according to three parameters: ontological, epistemological and methodological. Those five groups are: the Classical Mechanists (Helmholtz, Loeb); the Classical Vitalists (Driesh, Bergson); the Reductionists (Crick, Watson, Monod); the Materialist Holists (Pasteur, Waddington, Levins, Lewontin); the Idealist Holists (Kant, Bernard, Woodger, Polanyi). According to Kamminga, the Materialist Holists differ, for instance, from the other groups because they <u>do not</u> believe in the discontinuity between the living and the non-living domains; they <u>do not</u> believe that it needs explanatory principles fundamentally different from those used in the physical sciences; they <u>do not</u> believe that the behaviour of living things should be investigated and explained solely with reference to the properties and structural relations <u>of their constituent parts</u> (my stress). Dr. Harmke Kamminga, Public Lecture on "Whole, Parts and Aims of Explanation in Biology", Kings College, London, 2 March 1988.

22) Two instances of this are given: the metabolic process and ecological system, in both of which the 'whole' may be construed as constantly oscillating and rarely in equilibrium.

23) As such, they seem to stand for a 'negative-endorser' epistemology, to use Gellner's terminology: they do argue for a realist epistemology in biology, but reject any form of predictive theories. It is noteworthy that Rose decided to use a mere footnote to stress his rejection not only reductionism but holism as well (1987, 32: footnote 1). This, it seems to me, reveals a negative-endorsement of dialectical biology.

24) They maintain that biological determinism is indeed "the essence of the differences between human biology and that of other organisms. Our brains, hands, and tongues have made us independent of many single major features of the external world" (Rose, Kamin and Lewontin 1984, 289-90).

25) They write further,

Only an individual can think, but only society can have a class structure. At the same time, what makes the relation between society and the individual dialectical is that individuals acquire from the society produced by them individual properties, like flying, that they did not possess in isolation" (Rose, Kamin and Lewontin 1984, 287).

26) But dialecticians avoid the concept of hierarchy. So they prefer to speak in terms of "wholes", "processes" and "structures" (Rose, Kamin and Lewontin 1984, 277-78).

27) Rose gives a first indication of the principle of translation by referring to the twitch of a frog's leg. There are at least five ways of explaining the contraction of a muscle in a frog's leg: phylogenetics (i.e. the selective behaviour of the frog "to flee predators"), ontogeny (i.e. development of the frog embryo), biochemistry (the action of actin and myosin), holism (i.e. of the type "because the frog wanted to jump"), or chronological sequence of events. These five levels of analysis are neither contradictory nor reducible to one of those levels, Rose contends.

28) The criteria run as follows:
1. The process or metabolite must show neuroanatomically localised changes in level or rate during memory formation;
2. The time course of the change in biochemistry must match the time course of the specific phase of memory formation of which it is the correspondent;
3. Stress, motor activity, or other necessary but not sufficient predispositions or concomitants of learning, must not in themselves, and in the absence of memory formation, result in changes in Criterion 1;
4. If the cellular/biochemical changes of Criterion 1 are inhibited during the period over which memory formation should occur, the memory formation should be inhibited, and vice versa;
5. Removal of the anatomical locus (or loci) at which the changes of Criterion 1 occur should interfere with the process of memory formation and/or recall, depending on when, in relation to the training, the region is removed;
6. Neurophysiological recording from the locus (or loci) of the changes of Criterion 1 and 5 should detect altered cellular responses during and/or as a consequence of memory formation.

These criteria should form, Rose hopes, an approach which "combines a respect for reductionist methodology with a commitment to a non-reductionist, dialectical (or integrationist) philosophical standpoint" (Rose 1987, 127).

29) C. Sinha ("Negotiating Boundaries: Psychology, Biology and Society" in Towards a Liberatory Biology, ed. S. Rose, 26-39. London: Allison and Busby, 1982) argues that Rose's translation thesis is "reductionism reintroduced by the back door". For his translations of level-descriptions presuppose an "isomorphism" of theories in diverse disciplines rather than a real irreducible complementarity.

30) Arbib and Hesse's reductionism is nothing to do with Wilsonian biology, for instance, or with Chomskian "mentalism" (see note 10, chapter 3), or with the notion of "emergent properties" in the works of Sherrington (see note 10) or Weiss (infra).

31) It seemed to us that the reductionist model could convey very high explanatory content without distorting excessively the 'real nature' of these animals' behaviours. Equally we are not convinced when Rose writes as his ultimate justification: "even so simple a memory as a chick's avoiding a bead it has once experienced as tasting bitter, can(not) be reduced to molecules"; "memory does not lie in the molecules at all, but in some sense in the reorganised cellular networks that the molecules form" (Rose 1987, 136).

32) Interestingly, the majority of British, Canadian and American feminist biologists interviewed in this research also are socialists.

33) Rose in fact wrote as part of his discussion of models of explanation in biology in <u>On Molecules and Mind</u>:

> I am critical but I am not trying to be negative ... A unitary materialist understanding of mind/ brain relationships, of the sort for which I argue, must reject [the] dichotomy [between social and medical models of mental behaviours and disorder]. (1987, 111)

CHAPTER 5

RESEARCH DESIGN AND FIELDWORK WITH BRITISH AND CANADIAN BIOLOGISTS

This brief chapter will discuss the research design, type of material, and method of analysis of the empirical studies presented in Part III of the thesis.[1]

Among the various methods available in sociology, and the approaches more common to the sociology of scientific knowledge, it became obvious very early in my research that certain avenues were inaccessible while others remained manageable. I chose to conduct a small scale survey of 'mainstream biologists' and to contrast the results with data from two 'case studies' of practising feminist biologists. These two methods are fairly standard in sociology (Festinger et Katz 1953; Bryman 1988). However, the singular fashion in which I utilized these methods, especially in view of the methodological and epistemological debate in recent sociology of scientific knowledge, seems to be in need of further justifications.

In this chapter, I shall assume that the choice of a method may rest on large epistemological considerations, or may result more directly from technical assessment of the options available for elucidating a research question, given the kinds of data at hand (see Bryman 1988). In the first section, I shall discuss the approaches commonly offered in sociology and their advantages and limitations from the point of view of my research problems. In the second section, I shall attempt to justify, on a theoretical basis, my choice of a small scale survey of academic biologists in London and two 'case studies' of feminist biologists, British and Canadian, including survey of their published material, interviews, and direct observation, while leaving out the option of conducting participant observation of scientific work or examining cases of feminists who, in contrast, have abandoned biological research as such. Finally, I shall discuss the practical decisions I had to make throughout the research process, and the compromises involved, with respect to the quality and quantity of my sample of interviewees and two cases of practising feminist biologists.

General Approaches to Empirical Investigation in Sociology in General and in the Sociology of Scientific Knowledge in Particular

As we shall see shortly, the epistemological issue of methodology is most problematic in the sociology of scientific knowledge. Yet it is also true that more pragmatic considerations have played a role in the choice of research designs and methods in this discipline. The usual reliance on written texts documenting 'bodies of knowledge' of the past, or on formal and informal oral and published material related to breakthroughs in scientific research are cases in point. Documents and human recollections are the only means to recapture phenomena of the past, remote or recent.

Similarly, taking account of the resources at my disposal and the kind of issues I wanted to investigate empirically, I had to make decisions about the material I would gather in terms of the validity of this material to the problems being studied.

In the sociology of knowledge, the debate between qualitative and quantitative research does not emerge as a major methodological feature. The constitution of the sociology of knowledge as a discipline has in fact been sustained by the defence of qualitative research methods alone, primarily phenomenological and interpretative (Outhwaite 1975; Winch 1958; Bleicher 1982). Thus, the question as to which qualitative method offered by sociology is the most suitable for the purpose of understanding the process of production of knowledge is more crucial than the common debate between the respective virtues of the quantitative, 'scientistic' method and the qualitative, 'descriptive' method.

Having said that, one can use survey methods for studies of opinions or attitudes, for these permit the study of recurrences of opinions, widespread patterns of thought, and collective 'frames of mind'. More often than not, however, in the present day the object of study of sociologists of knowledge is not the search for this or that pattern of ideas. It is more a search for deep structures of thought and homologies with the material structure of social life. The focus of attention will very often also be a major piece of work in literature or in science for which a content analysis or 'context-bound', hermeneutical reflexion are more suited (Goldmann 1964; Bloor 1976). In the sociology of scientific knowledge such macro-analyses of large or smaller bodies of knowledge rank high on the research agenda of the 'Strong Programme'.

Thus, the preference of sociologists of knowledge for content-oriented (e.g., archival analysis, interview, content analysis) or process-oriented (e.g., participant or non-participant observation, ethnomethodology, ethnography) methods for understanding the relationship between social conditions and cognitive products remains paramount. This does not mean that more standardized quantitative methods are irrelevant, or that statistical data are of no use. For instance, in studies of macro-structures of science, figures of investment in R&D come to complement and document more accurately the context in which 'systems' of knowledge evolve and interact (Sklair 1973; Ravetz 1971; Rose & Rose 1969). But the focus on micro-processes is now more stringent and prescriptive on the agenda of the sociology of scientific knowledge. And to pursue Bryman's dichotomy, this has more to do with 'epistemological' motives than with 'technical' reasons. Such a shift of focus has brought about what Knorr-Cetina and Mulkay call 'methodological internalism' (1983, 6).

The accrued influence of this 'new' version of the internalist approach in the field of the sociology of scientific knowledge can be explained by two sets of reasons. Firstly, in epistemology, there is a consensus regarding the empirical underdetermination of theories. As a result, the investigation of how the 'missing links' between scientific claims and reality are being 'constructed' leads directly to the minute, qualitative field studies of laboratory work and scientific networks (Latour & Woolgar 1979; Collins 1985; Lynch 1982; Knorr-Cetina 1981).

Secondly, the emergence of 'methodological internalism' is concerned with the empirical re-enforcement of theories in the sociology of knowledge. The sociologists who favour an internalist approach do so because they acknowledge that previous studies assumed too roundly what precisely needs to be studied in order to understand the link between the social and the intellectual (or technical) components of science. Thus, 'methodological internalism' endeavours to establish satisfactorily the weight of social constraints on the technical rationalisations of scientific claims. The sociology of science of the past fifteen years has markedly embarked on the task of documenting properly that "knowledge is socially or existentially conditioned... and to work out in what sense and to what degree we can speak coherently of knowledge as being rooted in social life (Knorr-Cetina & Mulkay 1983, 6). The sociology of scientific knowledge has therefore taken an 'empirical turn'. That is, it is now far less concerned than before with macro-sociological parameters affecting social knowledge or with the question of why these bodies of knowledge take place. It now prefers to focus on the question of how scientific claims are produced, gain legitimacy, and finally assert their validity (Knorr-Cetina & Mulkay 1983; Latour & Woolgar 1979; Collins 1985).[2]

The sociology of knowledge intended to become a sort of 'self-reflexive' methodology for the whole discipline of sociology, paying particular attention to the process by which it produces its own validity-claims[3]. In the same vein, the methodological question is particularly important for the sociology of scientific knowledge.

In this respect, discussions about the 'scientific' or 'objective' character of the hermeneutic method has raised particular problems (Outhwaite 1975; Habermas 1976; Bernstein 1983; Hekman 1986). The notions of contextualism, meaningfulness, and representation are at the forefront of social studies in human knowledge. The question of the role played by language in the production of knowledge has manifestly impregnated the whole field of study (Gergen 1982; Knorr-Cetina & Mulkay 1983; Schutz 1962; Berger & Luckmann 1967; Barnes & Bloor 1982). While the quantitative and more formal methods of enquiry tend to take discourse at its face value and to overlook the deep structures of social knowledge, qualitative research offers more insight into the processes creating those very bodies of knowledge and discourses (Bryman 1988). Be it historiography, content analysis of archival work, ethnography, participant observation of laboratory work, in-depth interviewing, phenomenological analysis of discourse, or epistemological reflexion, the specificity of the sociology of scientific knowledge lies in its aim to understand bodies of knowledge in their social context as individual cases, rather than describe large-scale patterns of conduct or opinions -- although one does not exclude the other.

Surprisingly however, feminist studies of knowledge have tended to emulate the earlier traditions of the sociology of knowledge. That is to say, they have tried to elicit <u>why</u> science and technology came to be shaped as 'void of emotions', oblivious of 'subjectivity', oriented towards 'control and domination of nature', 'reductionist', 'patriarchal'.

The explanations feminists have proposed are sensible and some of their ideas are empirically justified. But often feminists will tend to overstate the causal relationship between patriarchy and epistemology, or bluntly assimilate 'patriarchal' and 'male-biased' interests with 'scientific motives' with which feminist themselves, in their own research practice, do not dispense -- such as canons of 'objectivity', 'experimental control', 'causality', and so on. Overall, the empirical investigation of how patriarchal and androcentric biases came to shape the 'scientific method and endeavour' and to maintain themselves as 'epistemologically mandatory' remains unsatisfactory in feminist studies of science. The actual outcome of the qualitative empirical investigation conducted by feminists has shed light solely on the question of the gendered division of labour in scientific practice (Homans 1989; Rose 1986; Kahle 1985; Rosser 1985, 1988a; Reskin 1978). It has not yet enlightened our understanding of the actual process of producing scientific claims and legitimating them through the 'institutional' channels of the 'scientific community'. Field studies focusing on the key notions of interpretation of evidence, negotiation of meanings, and persuasion are yet to come[4].

This trend is especially unfortunate since the core of the arguments in feminist theories of knowledge concentrate on how the notions of 'womens' interests' and 'feminine values' can play a revolutionary role in the negotiation of new meanings and challenge the predominantly patriarchal and male biased research designs and interpretive statements 'built-in' in scientific theories.

Feminist studies of science are also, first and foremost critical rather than empirical. Several works of feminists, for instance, address established corpuses of knowledge from a critical, feminist standpoint, akin to postmodernist examination of text and discourse (Bleier 1984, 1988; Fausto-Sterling 1985, 1987; Hubbard 1979, 1982). The historiographical type of method is also common. Several feminist historians deal with the role and status of 'knowledgeable women' in history (Abir-Am & Outram 1987; Alic 1986; Rossiter 1982; Rosenberg 1982). Obviously, a great number of these studies have shown that men have dominated science while women were literally refused access to it. But the precise process of how patriarchal statements come to be validated, at the expense of opposing claims, proposed by feminists for example, remains as yet unexplored.

True, there are very few examples permitting the study of such process, certainly due to the fact that the principal creators of challenger claims are mainly women and feminists and there are still very few of them in science. But this is not a good reason for resorting to circular arguments and contend that feminist science will be vindicated only when we live in a feminist world. It is high time to explore critically what the few cases of feminist biological practice can tell us about the form and content of feminist natural science, and possibly recast the feminist theory of scientific knowledge on these grounds.

However, as will be shown in the next section, the option of following 'methodological internalism' to study these cases was hampered by the necessity of comparing two 'trends' (namely, mainstream and feminist) in biological practice, thus augmenting the number of settings to be observed. Also, since one of our hypotheses involves distinguishing between biological specialties and subject matters, this generated logistic complications for the implementation of the otherwise insightful 'internal approach'. I decided, therefore, not to go ahead with the 'internal approach'. As noted above, my methodological choice does not denigrate this approach. I simply believed that, all things being considered, a question such as 'how do feminist biologists "construct" scientific facts' could begin to be addressed by means of more normative questions such as, 'how far does the handful of feminist biologists challenge traditional biological practice' or 'to what extent have they renewed the norms of research in biology, both in terms of organization and of "scientific" method'?

How the Methods Chosen Apply to my Research Question

Why were the methods of survey, interviews and observation, and documentary analysis adopted in my research? Why did I conduct a 'purposive sample' of interviews with 'mainstream biologists' rather than a case study; why semi-structured interviews rather than a participant observation? Why did I choose to carry out 'case studies' of feminist biologists instead of interviews as well? Why did I focus on them alone and not on their departmental 'mainstream colleagues' as well, and draw a contrast? Why did I focus on their publications rather than on the records of granting committees or departmental boards? Why did I choose not to investigate the cases of feminists who have abandoned biological practice, as a contrasting example?

In my fieldwork, I used a combination of qualitative methods in line with my theoretical aims. I was looking for elaborate arguments on the part of 'mainstream' and 'feminist' biologists and believed that face-to-face interviews would be preferable than questionnaires in seizing important aspects of their views on their discipline. It was believed that the choice of typical cases would better document my central arguments regarding the inconsistencies in the discourse of feminists about 'conventional' biology and its procedures. Although the responses from my survey could not be taken as statistically representative of the larger population of British biologists, the hope of drawing generalizations from my material was sustained.

My decision to conduct fieldwork with 'mainstream' biologists was based on two theoretical reasons. First, I believed that the relevance of 'feminine' values in biological production could only be assessed by looking at how, within 'non-feminist' settings, the style of practice and research interests of women would emerge. Second, my main goal was not to look at feminist practice alone, but to evaluate the harsh criticisms of feminist theoreticians of 'mainstream biology'. On this second count, I was able to obtain interesting insights into the so-called 'revisionary' views of mainstream biologists on their discipline and to test the soundness of the feminist criticisms.

Surprisingly enough, my first research design only included 'mainstream biologists', and aimed at comparing their positions with those of feminist theoreticians. But I faced two problems. My training as a sociologist did not equip me for even a basic understanding of their published material. In the same vein, I could not critically evaluate all the scientific writings relevant to feminist critiques, which dealt with various disciplines and topics. In any event, it is a common point of methodology for sociologists of science not to rely solely on published material of scientists, because such material depicts only partly the real features of the scientific 'world'. Publications are often believed to 'filter' the results of a process of formal and informal activities giving rise to scientific claims (Medawar 1985; Collins 1985; Latour & Woolgar 1979; Ziman 1968).

The idea of case studies of feminist practitioners arose from the results of the pilot survey of mainstream biologists. These pointed to an aspect of biological practice, namely its practical and 'cognitive' constraints, that has been overlooked by feminists theoreticians in their evaluation of the limitations of 'reductionist biology', but acknowledged in the writings of practising feminist biologists like Lynda Birke.

One could rightly argue that well-chosen participant observations in two different settings (i.e. 'mainstream' and feminist) would have conveyed a good picture of 'real', 'hard core' discrepancies between two trends of biological practice. Notwithstanding the costs in time and nuisance for all the people involved, I preferred to examine more varied settings and examples of biological discourse than to carry out in-depth interviews or long-term participant observation in only one or two laboratories. Also, all kinds of permissions must be sought when one wants to do participant observation. In the biological field for instance, there are some fast moving -- and rather secretive -- sectors where it is not easy for a sociologist to gain access. Lastly, while it is common in qualitative research not to have a strong observational framework at the onset of a participant observation, this approach is often thought of as a 'loose' and 'soft' by scientists and this may make them reluctant to cooperate. In one instance of my research, I actually had to submit a written project to the head of department, before I finally had the green light to conduct interviews in her department.

It became obvious to me quite early in the process therefore, that it was not possible to carry out long-term participant observation of 'mainstream biologists' for at least two reasons: time scale of my study, and the nature of my theoretical aim. First I did not want to confine myself to one laboratory because I needed to control for the specialty of the biologists being studied. I saw it very time-consuming to devote time and effort to conduct a reasonable study of preferably two settings (if not more), taking into account the time to make myself familiar with the jargon, experiment techniques and scientific agenda of the laboratories before I could even begin my investigation proper. Second, because my aim was not to compare in details <u>two 'ways' of 'building scientific facts'</u>, i.e. the 'patriarchal' and the 'feminist' way, I did not see the necessity of doing an 'ethnographical' type of participant observation. I merely wanted to get data at a first-level of discourse, on the topics being criticized by feminist theoreticians.

Notwithstanding their limitations, semi-structured interviews would help to discern the 'deeper', 'implicit' activities of scientific production, and give a 'wider' insight into the biological norms of research provided I had access to several settings rather than one alone.

They would enable me to address directly matters that are not usually discussed by practising biologists (and less so in their publications!) such as their political views on research structure and funding, institutionalized competition, quality of and impediments on research, and of course, feminism.

Obviously, interviews can only give a second-hand set of indicators, a range of 'opinions' that do not always reflect accurately 'real practice'. But as mentioned earlier, my aim to collect data on a variety of disciplines, research areas, and 'social' settings had priority. Hence, the goal of 'controlling for' a number of variables (e.g. sex, discipline) for purpose of comparing views on issues relevant to feminism in biology took priority over that of composing a detailed 'reconstruction' of only one or two cases of laboratory practice.

It is worth mentioning that many questions on the agenda did not elicit any proper answers and needed either to be reformulated or simply dropped. This was noted in the pilot survey where I interviewed fourteen female biologists, and this was particularly noticeable when I brought up issues related to the role of women and 'feminine values' in biology. But the method of surveying was flexible enough to accommodate adjustments in the final set of interviews.

I was led to revise my design and to include men in my survey. I thought it important to weigh the thesis of feminine values in biological knowledge against an account of the power of negotiations of women in 'male-dominated' fields of practice. Interviewing both men and women would provide an insight into how men and women act on the structuration of power in the biological institution at large or at the level of team work. Obviously, a participant observation in any of these labs would have conveyed relevant, first-hand information, on such a prescient process. But it is our main argument that the discipline was a factor, an epistemological factor, qualifying the thesis of the 'feminine' in scientific knowledge, and we wanted to pay less attention to the political factors that may affect it. We think that the material gathered from the interviews would reflect the existence and scale of power relations between the sexes in many labs. Indeed, women of diverse disciplines did not give the same amount of weight to the idea of feminine values, and our use of a semi-structured interview framework provided the flexibility to enable interviewees to elaborate or qualify their views on the matter.

The age structure, the sector (industry, health system, academia, and school education), and the discipline were posited as important theoretical variables. The age structure was denoted by the respondents' academic status. We have already mentioned the importance of the discipline variable. The disciplines (zoology, genetics and bio-medical research) were chosen in accordance with their theoretical relevance to the issues addressed by feminists. The sector variable was finally dropped, and interviews were confined to the academic sector, although medical/clinical and purely academic units were distinguished in the survey. The problem with this design is that the actual mapping out of two 'mega disciplines' does not correspond to how feminist writings usually describe them, or to our common-sense view of how modern biology is structured. As will be understood more clearly when results are discussed in chapter 6, zoology, for instance, encompasses studies of the genetic, physiological and neuro-chemistry of animal specimens, from brain cells to neurones, from genes to embryonic tissues, from mammals to insects, and is not necessarily typical of an observational discipline. Nor is genetics strictly experimental, for it may also involve long field trips and observations, mathematics, computations, and a lot of 'pushing of the pencil', such as in the discipline of population genetics.

It must be acknowledged at this point that our analysis of mainstream biologists did not convey a 'thoroughly critical' view of their practice, as we merely relied on their perceptions and opinions of what they do or ought to do. But in accordance to our theoretical goal, we only aimed at detecting recurrent patterns of opinions in order to test whether these opinions were partly shared by feminist practitioners and came to contradict claims of feminist theoreticians.

The study of feminist practitioners was similarly justified in terms of the discrepancies between how theoreticians portray feminist science and what feminist practitioners really do. A first-hand interview with feminist biologists would possibly reveal some of these differences. It was assumed that practitioners would not necessarily conceal their disagreements with theorists, and this presumption was based on the actual writings of the former. The interview would also be a good way to confront the contradictions between 'feminist' discourse and 'scientific' writings of these biologists. We therefore conducted an analysis of their scientific publications. This helped to document the convergences and divergences among mainstream biologists, feminist theoreticians and feminist practitioners.

As mentioned earlier, my training as a sociologist made it very difficult for me to analyze 'mainstream' scientific publications, most of which were densely filled with mathematical and bio-chemical formulas and jargon. With the publications of feminist biologists, this exercise was less difficult because the bulk of feminist work in biology deals with the 'social sources' of illnesses and development of the human body. The interview material in our two case studies of feminist biologists could therefore be cross-checked by external sources, something which could not be done in a similar fashion with our survey of 'mainstream' biologists.

I did not undertake case studies of feminists who tried unsuccessfully to implement 'feminist projects of biology' for two main reasons. First, my goal was to assess the weight of cognitive constraints on the viability of feminist projects. I believed that if I had relied on the evidence stemming from feminists who had abandoned biological practice, this might have lessened the weight of cognitive constraints and overstated that of political and organizational obstacles or overtly hostile attitudes vis-à-vis feminists within the biological institution.

Second, the number of feminists actually trained in biology and interested in the sociology of science is small. With respect to the development of projects of feminist biology per se, the number of feminists who actually have tried to implement them concretely is even smaller. Many authors have claimed that a feminist biology could not emerge in the present context of science for either political or epistemological reasons. But these feminist critiques of biology have been primarily the accomplishment of historians, sociologists and philosophers. My opinion was that these reasons needed to be re-examined upon further epistemological (or analytical) reflexion and relevant empirical material regarding the actual practice of feminist biologists.

An interesting aspect to investigate as part of this question concerns the ways departmental committees or funding agencies deal with 'feminist biologists' and their 'feminist research projects'. This could be an important sociological contribution to the understanding of the dynamics of change in science. Although I acknowledge the necessity of such enquiries, these should not, in my view, be carried out at the expense of studies about the cognitive (or epistemological) factors, or the normative procedures by which biologists, mainstream or feminist, seem to abide.

This is precisely why I decided to focus on those feminists who had succeeded in practising feminist biology, rather than those who had 'failed' or simply abandoned for various reasons. In brief, it is not so much because of a lack of interest in a question such as 'why have feminist biologists so far failed or abandoned the project of implementing a feminist science' that I decided not to study such cases. Rather, it is because of the urgent necessity to clarify the theories about 'how' and 'in what kind of epistemological and normative conditions' can genuine projects of feminist biology be viable and sustained.

Points of Detail and Critical Evaluation of Methodological Choices

In this section we shall look in some detail at the three sets of data collected. The procedures followed in the collection of material and the practical difficulties encountered will be highlighted. A discussion of the quality of data and the ways the material was analyzed will close our examination of the strengths and limitations of our fieldwork.

I shall explain now the steps I took to carry out my survey of mainstream biologists. Chapter 6 and the Appendix carry details on the structure of the interview protocol and the characteristics of my 'purposive' survey sample.

Having determined the importance of assessing the relevancy of the 'feminine' thesis of knowledge put forward in feminist writings, it seemed to be most appropriate to find some answers to this question by going to the practitioners themselves and to ask women biologists direct whether their actual experience gave credit to the idea of a 'feminine style' of doing biology. I thus embarked on a pilot survey of fourteen women biologists, to whom I was referred either by word of mouth or via a list of academic staff in a directory of the University of London.

At that point, the design of my research was sketched in a somewhat quantitative fashion. I wanted to highlight patterns of responses from various settings rather than to contextualize more consciously the views of my respondents in, say, a theoretical framework of patriarchal or 'professional' and institutional normative constraints. I gathered information on the motivations of respondents to enter biology, their previous academic grades and their ambition, and their experience of discrimination. One third of the questions in the protocol directly concerned the notion of feminist biology and related issues.

But it became obvious that I would not obtain a lot of useful information on this last topic since the respondents did not understand the questions, or that they thought out their answers very carefully and emphasizing the need for some 'hard evidence'. The inclusion of teachers in the pilot survey (on the hunch that they would be more conscious of, or informed about feminist issues in science) did very little to enhance the relevancy of my material at that point. The information I wanted to collect was elusive because the issue of feminism in the scientific organization is somewhat politically loaded and compromising, or the potential for a 'feminine style' or 'feminist-oriented' type of knowledge in biology remains entirely speculative, or else the issue is squarely anti-scientific, to 'mainstream biologists'.

This first survey shed light on my research strategy in a number of ways. There were differences between older and younger women with regards to topics such as discrimination and professional difficulties, which was expected if one takes into account the variability of opinions during one's 'life cycle'. I found that the views of men needed to be included in order to balance the portrait that women gave of their male colleagues (which was not always complimentary!). I had to focus on some biological specialties that were more directly concerned with -- or exposed to -- feminist critiques of knowledge (such as human and animal genetics). I needed to treat my empirical material as 'opinions' and 'patterns of discourse' rather than as reflecting 'real practice'. I finally realized that under present methodological conditions, my data would be considered valid if I followed certain procedures. These are a careful selection of interviewees with an awareness of possible auto-selection, the possibility of checking interviewees' responses with those of their colleagues, by addressing similar problems from different angles and formulated in different ways during the course of interview ('internal triangulation'), and a minimal awareness of the context of a given departmental life ('external triangulation').

Under such conditions, the analysis of my survey material could be done according to canons of quantitative analysis, i.e. by 'tabulating' answers upon 'emerging' categories or patterns of response, 'counting numbers' in each of these categories and 'illustrating' patterns of responses by means of representative quotations. But given the small size of my sample, and the 'noise' factors I was able to take into account thereof, the analysis of data resembled that of a combination of 'standardized categories' applied by the observer and 'interpretive analysis' grounded in the context described by the 'interviewee' and re-created ex post facto by the observer. This mixture of both qualitative and quantitative methods is by no means new in sociology (Bryman 1988). A judicious overlap of methodological tools often pays off and fully accounts for the richness of empirical material. This in no way contravenes the epistemological differences between those two traditions of social enquiry but rather takes into account their respective perspectives on social objects (Bryman 1988;

see also our discussion on these matters in chapter 3).

The options envisaged for the second stage of my survey were the following. I could embark on a specific participant observation to see for myself if the 'feminine thesis' would hold, and there is a need for this sort of enquiry in the feminist research agenda about science. But I decided to continue with the survey approach because of the importance of getting data from various settings and disciplines. I could focus on biologists' personal experiences, providing data from which I could try later to generate more general conclusions. Finally I could simply switch my investigation, from the question of the 'feminine thesis' in particular, to other aspects related to the conditions of possibility of projects of feminist biology as spelled out by feminist theoreticians.

I carried on with the interview approach, but this time focused my attention on the self-representations of men and women as 'good scientists', the difficulties they personally encountered in scientific work, and their views on 'scientific' problems relevant to feminist criticisms, such as sociobiology and biological determinism of behaviour and 'disorders'. My sample for this second stage was chosen according to those three main 'criteria', selecting biologists of both sexes, of two different academic status (students and teaching staff), and in three different disciplines: genetics, bio-medical research, and zoology.

During the summer of 1988, I attempted to contact some biological associations in England to obtain a sample of interviewees. Six were contacted but they could or would not provide me with a list of their members. I was therefore decided that I would go for a 'snowball strategy' and deal with a statistically non-representative sample.

My 'purposive' sample was mainly based on a directory of the University of London. I also used different channels to enhance my chance of contacting more respondents. Six departments were contacted through their directors (one declined) who either advised me on who might be available or worthwhile to interview, or simply gave me a list of their staff. Personal letters explaining the aim and procedure of the interview were sent off to a same number of men and women, students and staff, chosen at random.

One head of department was more stringent and asked me for a written research proposal which she would circulate herself.

All in all, the access to respondents was not too difficult. But I must stress that half of the selected biologists in my sample did not acknowledge my request for an interview. (Only three individuals wrote me to apologize for not being free for an interview.) In one instance I made a special effort to get an interview. She was referred to me as being a feminist by several of her colleagues, which was unusual. She did not reply to my first request for an interview so I sent her a second one which she did acknowledge. She appeared better able than any of her colleagues to discuss knowledgeably the issue of 'feminist biology', sociobiology and reductionism.

The interviews generally ran smoothly and took place at times and venues chosen by the respondents. Some informal remarks were common. For instance, a great number asked me how I would analyze my material. Several also asked how far I was in my research and 'how many interviews I would do', perhaps showing some apprehension about being chosen to represent the 'whole community' of biologists. More importantly however, it was a common feature that controversial questions (e.g. are women more careful, 'better' scientists than men; are they more conscientious; is there any discrimination in your department?), were dodged on the allegation that they needed to be 'better defined', or backed up by 'more facts' and 'figures' to be properly answered. This contrasted with the relative ease with which respondents seemed to comment on 'non-controversial' issues such as: is biology a competitive sector; is molecular biology unduly fashionable; are women in general less confident than men?

All in all, respondents were co-operative which reflected the fact that they accepted to participate in my research without being forced. With very few exceptions of 'bouts of hostility' (I recall three women in particular), dismissiveness (mainly from the men) or obligation (the case of one woman), they were attracted by the topic and the refreshing idea of having a 'pleasant pause' from work. As a matter of fact, I had the feeling that several men seemed to take the interview agenda casually, while most of the women seemed enthusiastic to discuss ideas about the place of women in science, and to 'air' some of their grievances. A study of the motivations leading scientists to participate in sociological research could merit an investigation in itself, I thought.

The decision to end the survey was taken when I started getting the same types of answers and discerning some patterns. Obviously, my material would be treated carefully and as a 'reasonably sound' source of information only. I figured that for the purpose of comparing 'the views' of mainstream biologists about their disciplines with the discourse of feminists, some 70 hours of interview with 45 biologists was a reasonable basis to start assessing the claims of feminists about 'patriarchal' and 'male-biased biology'.

My attention was first drawn to Lynda Birke after a discussion with a sociologist at the LSE who suggested I should rather study feminist biologists, than interview 'mainstream' scientists. After I read her book on 'Feminism and Biology', I contacted Birke, first by letter, and then a week later by telephone. She agreed to an interview to discuss her views on feminist biology. The interview took place in her office at the Open University in July 1988.

It was a long informal and quite relaxed discussion that lasted roughly three hours. No tape recorder was used and notes were taken by hand throughout. Like many feminists I was to interview afterwards, Birke suggested that I not study mainstream biologists but concentrate on feminists alone. Though the suggestion seemed sensible, given the position of a majority of biologists towards feminist biology, I remained convinced that I needed my other set of data in order to weigh the criticisms put forward by feminists and the viability of their own projects of biological practice.

I embarked on an extensive reading of Birke's publications on biology, especially her sociological critiques of biology. I had previously arranged a discussion with Steven Rose[5], a colleague of Birke and head of the department where she was based. This interview with Rose helped me to qualify Birke's views on the potential role of women in biology, on the potentialities of feminist biology, on the state of equal opportunities within biological institutions, and on the relatively favourable environment for feminist research at the Open University. He also emphasized that Birke was a rare case of feminist practising biology and that she had probably had difficult times in the course of doing so.

At this point, I realized I needed to interview more feminist biologists since Birke's discourse departed from feminist critics in their appraisal of reductionism and the biological bases of behaviour. I contacted Birke for possible references to other feminist biologists, but she could only think of non-biologists. After unsuccessful attempts to contact feminist biologists in South-east England (I was constantly referred to feminists interested in science and technology but none of them were practising biologists, and one 'liberal' woman biologist referred to me by Birke never did reply to me), I decided to interview Birke a second time in order to document one case study and perhaps gain access to her laboratory work. This was in March 1989.

Unfortunately, she was about to leave her post and abandon biological research altogether in order to invest her time in writing and teaching womens' studies. On the other hand, she had indicated, as Steven Rose did, that her work was very solitary and did not compare with big, competitive science[6]. Her style of practice was hardly a case of 'team work dynamics', worthy of sociological observation. She nevertheless agreed to a second interview. This time however, the ambience was not so relaxed and Birke was more hostile but she allowed me a two-hour interview which I taped.

By then, I knew I would have to focus on a content analysis of Birke's published works and asked her if she could provide me with a complete bibliography. She said she did not have one at hand but would send it by mail. She never did, even though I reiterated my request in a subsequent letter. My case study was therefore jeopardized, for I had to rely on disparate bibliographical resources and would certainly not be able to 'face' Birke again.

I should like to try to explain Birke's attitudes by putting them in their context.

Birke was on the verge of having to leave the Open University. She was quite apprehensive about unemployment given her age (late thirties) and the covert hostility of employers towards feminists. She had been quite lonely as a biologist, and my own research was very critical of the endeavour of feminist biology which, she must have felt, certainly reflected in my questions about her work as well.

All these factors certainly undermined my opportunities to have her as a 'better' respondent and not least, to secure a rich case study for the understanding of how feminist biology can -- or cannot -- operate and develop in 'mainstream institutions'. This first case study in my thesis, therefore, has its limitations. The institutional context and professional relations in which Birke's activities took place cannot be ascertained. It came as a surprise when she acknowledged that her scientific work never was the object of blatant resistance, and that she had been fairly well treated as a woman and active feminist in her scientific circles[7]. Nevertheless, it was still necessary to include her as a 'case study' in the thesis, as it represented one of the few examples of feminist biological practice.

My attention was also drawn to a woman geneticist from Montreal whose work, as she put it, was 'to do science in order to change things'. Karen Messing's scientific work was tightly linked with her feminist engagements, both outside and within the academic circles. In the fall of 1988 I contacted her and we planned to meet in December during on of my trips home. We had our first encounter at her home. She was easygoing and enthusiastic about the fact that I was a sociologist interested in her work in occupational health and her research group. She invited me to visit her laboratory (which actually was a series of office space in the department of biological sciences at Université du Québec à Montréal, with laboratories located in the basement) and held an informal round table over lunch with her students and senior research assistants. This first meeting helped me to focus on the internal dynamics of the group and the relative lack of feminist commitments of the members, especially the young ones, in contrast with the involvement of their director.

GRABIT, as the group was called, therefore emerged as one possible venue to be explored in order to document feminist biological practice. After my failure to contact British feminist biologists, I felt pressed to contact Messing in the early spring of 1989 and we agreed on the practical arrangements for my field study in Montreal. It included interviews, analysis of published documents, and some 'observation' of daily work.

The fieldwork was held in May 1989. Although this is a period busy with correction work and preparation for the annual national conference at the Association canadienne-française pour l'avancement des sciences (I myself attended conferences given by members of the group), this was also the best time to have everybody present, just before the summer holiday.

Messing and her co-director Mergler were kind enough to provide me with a desk in a room which I shared with other students. (The actual office space, computer and secretarial facilities at GRABIT would surely be the envy of many sociologists!). Suzanne Bélanger, a senior researcher, introduced me to everybody and explained the structure of the research group and its status within the university. I started my field study right away and on a very positive note.

I began scheduling individual interviews and to read the impressive publications of the group available in the reading room. During a typical day, I would also conduct informal discussion with one or several members of the group, and be present at lunch time when they would all get together. Discussions and comments about trade unions, granting agencies and progress of research work -- done mostly in plants and workshops -- were common. I soon became part of their group, and was invited on different occasions to join some of them for a meal or drink after work. (I was even invited to replace a speaker who cancelled at the last minute and gave a paper about my research.)

I stayed a month at GRABIT. I had gathered a lot of published material at GRABIT, information about the institutional context, the background of researchers and the dynamics between members of the group, men and women, students and staff, non-feminists and 'avowed feminists'. Talking to different people in the laboratory also helped me to secure some kind of 'internal validation' of my research material. I felt I had collected enough data in Montreal in order to compare this material with my survey and the other case study of Birke. Although the study of GRABIT was more sophisticated than the one with Birke, it did not allow me to 'reconstruct' the way feminists 'build biological facts'; for I had only mustered indirect oral and written material relevant to this issue. Finally, I did not carry out interviews with officers of larger institutions such as the department of biological sciences, the university, or a crucial granting agency like the Institut de recherche en santé et sécurité au travail, although such interviews could have helped to validate my material 'externally' and given me an idea of the 'macro-institutional dynamics' within which feminist biology is bound to develop. This aspect of feminist biology definitely needs to be examined more thoroughly in future sociological studies[8].

In the summer and fall of 1989, I analyzed all my material and produced drafts of the three chapters that will follow. In December 1989, I returned to Montreal and discussed my results and the chapter I wrote on GRABIT with Messing. (I also had feedback from Mergler and Brabant, a Ph.D. student with a relatively long experience with the group.) This helped once again to validate my material, especially the institutional and biographical facts, and my depiction of how the directors and members of GRABIT viewed their own research endeavour.

I would like finally to add two other components of my empirical material. First, in addition to my two 'case studies' of feminist biologists, I interviewed key informants in New England and in London during the summer of 1989. Phyllis Robinson (referred to me by Birke), Ruth Hubbard (introduced to me by Messing), Anne Fausto-Sterling (with whom I arranged an interview by phone from Montreal), and Judith Masters and Dick Rayner (referred to me by Hubbard), all trained biologists and feminists, discussed some of my results and analyses. They helped me to appraise the strengths and limitations of my material, the latter of which they especially remarked by pointing to the political constraints induced through larger institutional channels affecting feminist activities. But they also gave additional credibility to my analysis of the scientific and cognitive factors constraining the development of feminist biology.

Second, in order to put the survey material of mainstream biologists in context, I spent some time reviewing statistics, historical accounts, and the coverage of professional matters in the journal of the Institute of Biology (mainly oriented towards education and the career market). This material will be presented at the beginning of chapter 6. This obviously cannot make up for a first-hand field study of the biological institution per se and its decision-making instances, but it could reasonably act as a substitute for contextual factors from which our analysis of the interview material could benefit.

Endnotes

1) I would like to thank my good friend and colleague Wilma Mangabeira for her advice in the writing of this chapter on methodology. She helped me to find a way of combining intellectual standards and personal satisfaction in the writing of very often tedious methodological discussions. Although I am far from reaching her meticulousness and narrative skill, I gained a lot in doing the exercise. My thanks also go to Drs S. Walby and H. Collins for pointing out the utility of revising 'self-consciously' one's research strategy.

2) Knorr-Cetina and Mulkay warn that this in no way entails the inevitability of 'judgmental relativism', or that all bodies of knowledge are equally valid, given specific cognitive or practical goals. It entails rather, an 'epistemic relativism'. That is to say, since knowledge is rooted in time and culture, "scientific knowledge does not merely replicate nature" (Knorr-Cetina & Mulkay 1983, 6) but is produced via the mediation of social influences. I would call this perspective a 'weak version' of the realist account of knowledge. See on this point, Manicas & Rosenberg (1988) who discern insightfully between 'double-barrelled' and 'single-barrelled' realisms.

3) Choosing the best method possible for this purpose has always been a central problem on the agenda of the sociology of knowledge (Bauman 1978; Outwaite 1975; Abercrombie 1980; Law 1986; Hamilton 1974).

4) The only type of works that lean towards this problem are those about gender and technology, primarily in the sociology of labour but also in the new reproductive technologies in medical sociology. The works of C. Cockburn, for instance, Machinery of dominance. Women, men and technical know-how (London: Pluto. 1985), are worth

mentioning. See also the works of R. Klein on reproductive technologies, especially her recent article with R. Rowlands entitled, 'Hormonal cocktails: Women as test-sites for fertility drugs' in Womens' Studies International Forum, 12 (3): 333-48.

5) Interview of Steven Rose by the author, London, 14 April 1988.

6) This is also what Ruth Hubbard and Anne Fausto-Sterling pointed out in their interviews with the author: the rare cases of feminist biology are alternatives to 'little science' rather than alternatives to 'big science'.

7) Birke's statement must be qualified on three grounds. First, she admitted that it has been "extremely difficult" to be a feminist biologist, even though she could not really tell the extent to which this had impeded her scientific work. She said "Most institutions claim they don't discriminate, but to prove that is actually very hard. It is hard to pin down instances of overt discrimination. But I'm sure it has been a factor in my life, why I don't have a permanent job." Second, she suggested that she has had more problems being a lesbian than being a feminist. This is probably "because men don't know how to interact with women if there is no flirting involved", she said. Finally she declared she could not recall that her colleagues or peers had ever deliberately put down her scientific work, adding that all things considered, there was usually no blatant resistance to the works of "99 percent of all scientists".

8) Messing was slightly disappointed to realise that I did not intend to look at the policy and action of granting bodies proper. It became obvious however, that 'the political games' of granting agencies were of a major concern for people at GRABIT. Messing and Mergler had several adverse experiences and stories to tell on the subject.

CHAPTER 6

ASPECTS OF THE SCIENTIFIC ETHOS OF 'MAINSTREAM' BIOLOGISTS

The goal of the present chapter is to investigate the extent to which the 'scientific ethos' of 'mainstream' biologists is coloured by the conventions of scientific practice criticized by feminist and Radical Scientists. Relevant data was collected by way of interviews with male and female British biologists working in the University of London during 1987 and 1988. In this chapter we shall examine the opinions of biologists with respect to feminism in biology from different angles: from the perspectives of individuals' attitudes, the organisation of research work, and the current norms of scientific production.

In the foregoing chapters, we made several assertions concerning the manners in which male biases afflict theory-building in biology and, on the other hand, the extent to which epistemological constraints affect the production of biological knowledge.

In our consideration of the first of these two issues, we suggested that there were certain 'blurred spots' in feminist theories of science which had led, unfortunately, to overstating the adequacy of 'feminine values' in the replacement of 'male-biased' epistemological canons and methodological procedures in biology.

We contented that feminine values <u>cum</u> loving and caring relations with nature and people were cognitively inappropriate for developing biological explanations, to the exception, perhaps, of pointing to moral and ethical issues of importance to scientific questions. The early feminist Marxists who defended the notion of 'feminine values' as having the potential to replace 'male values' did not, in our view, contribute to build a new epistemology on these bases. Certain points made by feminist Marxists certainly paved the way, however, for an increased participation of women in decision-making in scientific research. Although the assistance of feminists more actively involved in the process would always be emphasized by these authors. These points, we believed, pointed to the possibility of increasing, via the idea of 'feminine values', the numbers of researches of direct interest to women, their health and living conditions, and to successfully achieve of the avowed goal of feminist critics of science.

Second, we hypothesized that there was, in theory, a potential for 'feminine values' cum subjective experiences, direct knowledge of women's life conditions, and accessibility to women's bodies and physical symptoms. For the idea that 'feminine values' could be activated as a platform to produce changes in theory-building was justified epistemologically, although only for some areas in biology. We demonstrated the similitude of biological knowledge (in human biology more particularly) with hermeneutics and interpretive methods in the human sciences. On this basis, we acknowledged that researchers' interests could, in the case of women biologists, be manifested differently from those of men, especially in clinical and behavioural research. This argument would apply to theory-building, particularly in fields closely related to human behaviour and its 'disorders'. We admitted, however, that, yet again, for this process to take place, some degree of political awareness about sexism in behavioural studies needed to be raised.

Third, we granted credit to feminist research promoting the role of women in science. We stressed especially the historical and sociological studies showing how the institutionalization of science has been conducive to a deprecation of women's role in scientific work, studies which contributed to a sound theory of gender and the science system. In this type of theory, patriarchal strategies are identified as the main obstacles to the full deployment of female resources in the scientific domain. The empirical content of historical and sociological studies, in turn, supported the view that men have kept dominating the field via both macro-institutional tactics (e.g. hiring practices, funding procedures, work facilities and provisions for child care) and micro-organizational operations (e.g. networking, team work relations, division of labour in the laboratory). We inferred from this that a 'feminine style of working' and 'female research priorities' were not given their fair chance or fair share, and that the make-up of day-to-day scientific activities was not yet congenial to the fully-fledged integration of women scientists and 'feminine values' thereof.

With respect to the issue of epistemological constraints as such, we went at length on the inaccuracies of feminist theories and criticisms of biology. We suggested, on one count, that the limitations of biological reductionism as a theoretical paradigm were real, but that nonetheless, methodological reductionism was to be safeguarded as such, provided an increase in sophistication of experimental designs and biological explanations. We also criticized the apparent virtues of holism, especially in terms of the 'inseparability' of biology and environment, and the neglect of biological constraints and determinisms. We finally contented that the logic of biological knowledge always inevitably generates theoretical controversies about what is a good balance between the reality of 'vital processes' and the 'inhumane', compelling necessity of having to apply mechanical or causal explanatory models in order to produce new knowledge. Never did we, however, coalesce the 'nature' of these epistemological constraints with the presence of 'male' and 'patriarchal biases'. For we assumed that formal categories of explanation were not so much local, 'male-laden', but rather that it was their substantive content which was subject to 'male' or 'patriarchal' ideology in the first place. From this we suggested that it is primarily in human biology that concepts, explanations, and interpretation of data may lend themselves to 'patriarchal' prejudices and 'male biases'. And that these might, therefore, be replaced by more adequate 'feminist-oriented' schemas or 'feminine interests' when dealing with certain subjects in biology.

We shall now, in this first segment of our empirical study, consider the above issues upon the responses of a survey with 'mainstream biologists'.

We shall first examine the viewpoints of informants concerning career barriers and discrimination against women scientists. The differences in the opinions of men and women will be highlighted. This should enable us to see if feminism, as a political force, is being judged more or less positively within the confines of the biological profession by the two groups.

We shall then, examine the perceptions of biologists in relation to scientific skills. This will suggest how far the actual workstyles of men and women and also the gendered images they have of themselves as scientists invite biologists to conceive of men and women as two groups which may contribute differently to the development of norms of biological practice and perhaps also to the production of new biological knowledge. The comparison between the responses of men and women should therefore suggest ways in which the variable gender contributes to shade the 'biological ethos'. This forms an empirical basis against which a key aspect of the feminist projects of biology, that which presumes women to have a particular role to play in the construction of new biological knowledge, might be tested. Among the other variables which may affect this, we shall examine the discipline (e.g. genetics and molecular biology, zoology and ecology) and the age of the respondents in order to 'control for' their effects. As mentioned previously, the idea of a feminist biology based on 'feminine values' looms large in feminist thought; and if this idea is being called into question in both theory and practice, it is nevertheless far from being dismissed altogether. This is why we shall be examining the gender variable in the present empirical study. Our own line of argument is that a 'feminine/feminist' biology may possibly be generated by women, but only contingent on certain epistemological conditions. One of these conditions has simply to do with the subject matters in biology, that is, whether the objects of study are gendered, and more importantly, whether they are human individuals.

We shall also explore the discourse of biologists concerning the strengths and limitations of reductionism in biological practice. This is a contentious issue in biology, and it has been the target of the critiques of Radical Scientists and feminists. We shall explore this in light of the rationale of mainstream biologists relating to the possibility that scientific errors and sexist biases may be creeping into the production of biological knowledge.

We shall finally close our analysis on a more encompassing discussion of the issue of feminism in biology, comprising the gendered structure of biological practice, the social biases in the biological knowledge produced, and the politics of biological research. These three series of aspects of 'mainstream' biological practice (i.e., individuals' attitudes towards women's work and feminist politics in science; the rationale of biology as valid knowledge with respect to the issue of holism versus reductionism; social critique of the biological agenda with particular attention to the idea of a feminist biology) will form the axes of our comparative analysis with feminist biologists. These should also form a threefold background against which the criticisms of feminists theoreticians of science and the originality of feminist projects of biological practice will be tested. The material in this chapter will thus provide part of the documentation needed to explore the points of convergence and the discrepancies between mainstream and feminist biological practices, and the actual sources of resistance of mainstream biologists towards the idea of a feminist biology.

The Institution of Biological Science in Great Britain

A brief presentation of the process of institutionalisation of biological science in Great Britain, with special respect to the insertion of women in that process, should help to put the interview material analyzed into its sociological context. We shall look at the development of the biology curriculum and at the expansion of the role of biologists in society.

The historical data and statistics to which we refer in this section relate to Great Britain. But it is reasonable to think that the tendencies hereby presented apply to other Western industrialized countries, more specially Canada which is the source of the second of two forthcoming case studies of feminist biologists.

Compared with the other natural sciences, biology has striven harder to get its credentials into the science curriculum. During the initial decades of the century, several areas of biology were considered descriptive or concerned with the observation of natural specimens, rather than based on experimental trials subjected to objective methods of testing and amenable to scientific 'laws' of explanations. Biology was not considered a full-fledged scientific subject until it showed, as physics or chemistry did, its utilitarian potential for industry and medicine (Goodson 1987). Indeed, it was believed that botany and zoology, the two main biological subjects at the time, lacked practical applications (ibid.).

In the 1930s however, biology became, in Great Britain, more directly geared to social policies. Its application to social health and domestic hygiene were recognized more widely. This partly explains why it increased its importance in the school curriculum during the first half of this century (Gordon and Lawton 1978; Jenkins 1979).

Jenkins (1979) reports that from the mid-eighteenth century to the early 1930s, botany was, by and large, the only biological subject taught in schools. Botany was then joined by zoology, and by courses in general biology aimed at the education of the citizens and their welfare. The 1930s were years which witnessed the establishment of biology in the science curriculum at the secondary level. During the 1940s and 1950s, the medical curriculum was separated from general biology. In the late 1950s and early 1960s, 'hard science' biology appeared. Disciplines such as molecular biology and biochemistry, comprised aspects of sciences like chemistry and physics. As of the 1960s, three main divisions emerged within the structure of biology: ecology, cellular, and molecular biology; and this, at the expense of traditional subdivisions of zoology and botany (Jenkins 1979).

But the rapid development of molecular biology and biochemistry in the last two decades, and the increasing disciplinary specialisation within biological science far from obliterated criticisms. There was a cry for the integration of diverse subjects within the biological curriculum (Chalmers, Crawley and Rose 1971; Giordan et al. 1986; Levins and Lewontin 1985; Sapiro 1985). This did not merely seek for the scientific recognition of the complexity of living phenomena. It also appealed for more interdisciplinary studies within the life sciences, and for a recognition of the sociological ramifications of biological phenomena in the manner of Radical Scientists and the dialectical biologists. This last issue has certainly coloured the debates about biology over the last two decades[1].

Also characteristic of biology, and more importantly botany, was its predominance as a girls' subject in school science. In 1951, the ratios of girls to boys among O-level entrants in botany, zoology, and biology were of 7.84, 1.33, and 3.18 respectively. Among A-level entrants however, the ratios were lower, falling to around 0.6 in the first two subjects and to 0.53 for biology; in 1962 however, they increased to 0.87 in botany, 0.84 in zoology and 0.64 in biology[2].

The gender division in the science curriculum between physics and chemistry for boys, and biology (or botany) for girls, is "a strange heritage" (Gordon and Lawton 1978). Girls' schools were not equipped to engage in laboratory work in physics and chemistry: they did not have the material nor the teaching staff required. The resources necessary for teaching chemistry and physics were, by and large, endowed to boy's schools (Jenkins 1979). Jenkins suggests that the way biology was taught in schools has always been more relevant to the personal and domestic interests of girls, and in direct line with the social dimension of biological knowledge as girls were expected to become nurses, midwives, or housewives (ibid.). Jenkins offers more evidence of the gender delineation in the development of school science. In the early 1900s, he reports, the Board of Education stated that the socially widespread deterioration in the health and material state of British households was to be addressed by providing pupils "destined to be mothers and fathers" of our future generation with elements of "domestic science" (ibid., 173). The changes in the curriculum actually affected mainly girls, and was also to lower the levels of science education in girls' schools. The Thompson Committee of 1918 acknowledged the lack of teaching resources in physics in girls' schools, but this did not seem to drastically change the orientations taken in girls' science education, writes Jenkins.

During the inter-war years, biology and general science gradually replaced botany in girls' secondary schools. But due to a lack of teachers in physics, chemistry and mathematics, biological science consolidated its domination in the girls' school science, which resulted, in the early 1960s, with very few girls' grammar schools offering physical or general science in their curriculum. Yet, Jenkins also reports, in single sex schools, more than in mixed schools, the resources available for science tended to be similar to boy's schools, and this was reflected by the higher tendency of girls to choose physical science in the former type of schools.

Currently, there are more girls than boys in biology. The figures for 1985 show that the percentages of girls' entrants and passes for GCE 'A' levels in biology were slightly over 60%. 'Hard' sciences such as physics and chemistry were still male-dominated. The percentages of female entrants and passes in those subjects were roughly 40% in chemistry and 20% in physics[3].

At the university level also, the percentages of women in biological sciences have always been higher than in other sciences. Women even outnumbered men in botany and zoology between the two World Wars. During the year 1931-32 the percentages of women among Honours graduates in botany and zoology were 57.5 and 51.9 respectively; 11.6 in physics, 14.9 in chemistry. But it should be stressed that botany counted four times less graduates than chemistry and half of the physics graduates; zoology counting even fewer degrees awarded (Jenkins 1979, 188). In 1948-49, the differences had not changed appreciably: 55.5 of women in botany, 45.8 in zoology compared to 5.7 in physics and 9.7 in chemistry (ibid.).

The figures of 1971 for full-time undergraduates in biological science disciplines[4] show that 37.8 % were women, compared to only 27.2% of women in all the sciences. The figures for 1980 show an increase to 43.1% and 31.7% respectively. The figures for 1986-87 show a more important increase in biology, this time up to 54.1%. This increase is partly due to the inclusion of psychology (a subject in which women's participation has always been important) within the biological disciplines, but it also reflects a real increase of women entrants in disciplines such as general biology, zoology, biochemistry, and biological subjects combined with physics, mathematics or chemistry[5].

These statistics show that biological subjects such as botany and to a lesser extent, zoology, have always been favoured by women interested in science. They show the trend of a significant increase in the participation of women in biological sciences over the past two decades, and challenge us to reflect on the sociological impact of the presence of women in biology.

These figures hide the phenomenon of attrition of women scientists between the undergraduate and postgraduate levels. In 1971, there was a loss of roughly 15% of female full-time entrants in the passage from undergraduate to postgraduate studies: down to 21.5% of female postgraduates in biology, and to 12.5% for all sciences combined. In 1980-81, the loss was reduced to roughly 11%: down to 32.7% of female postgraduates in biology and to 20.6% in all sciences combined. Finally, in 1986-87, the loss was 13%, down to 41.9% of female postgraduates in biological disciplines (including psychology)[6].

Finally, women are still poorly represented at the higher levels of academia[7]. In 1966, they formed 13.4% of the staff in the group of disciplines formed by physics, chemistry, biology and mathematics, of which 14.6% in the higher echelons (i.e. professorship, readership and senior lectureship) in the academic hierarchy. In 1980, they formed 9% of the staff in biological and physical sciences, of which 8.4% in the three higher echelons. In 1986-87, they formed 12.4% of the staff in mathematics, biological and physical sciences, and 5.0% in the three higher echelons.

It seems therefore that consistently, biology has been considered the science subject of preference towards which women scientists were directed. But it did not affect the fact that biology was dominated by men for they are still occupying most of the higher posts within the academic and scientific institutions. If the trend is to an increasing number of women in biology in general, their representation is still minimal in top ranks[8]. As Rossiter

(1982) has contended, there has been a 'proletarianization' of women's work in biology which is reflected in their low numbers in high status posts, and their high numbers in jobs of technicians and teachers (see also Murphy 1980; Rose 1986; Kahle 1985). Even the president of the prestigious American Association for the Advancement of Science recently pointed out that the attitudes of the males who largely dominate science could have acted as a possible deterrent on the careers of promising women students[9].

At another level, a survey of the official journal of the Institute of Biology[10] may help to highlight the progressive professionalization of biology, and more importantly its increasing function for industry, while giving an idea of the mode of insertion of women in the professional openings since the 1950s.

In the late 1950s, the demand for biologists was much lower than that for chemists and physicists (Journal of the Institute of Biology, February 1957, 33). In 1956, there were roughly 5,000 posts for biologists, compared to 10,000 posts for physicists and 20,000 for chemists. The demand for 1959 was expected to continue to favour chemists and physicists with an increase of 22% and 23% in the numbers of new openings, compared to an increase of only 12% in the number of new jobs for biologists (ibid.). In the early 1960s, the main outlet for biology graduates was teaching (roughly half of the population), while graduates in chemistry were mainly oriented towards industry (Institute of Biology Journal (IBJ), April 1960, 46-48). In that period, biology was still not totally constituted as a profession, which was especially well reflected in the school and university curricula.

In the second half of the 1960s however, the need of industry for biologists became more pronounced. Advertisements for jobs in biology in the IBJ became more numerous during the period, and the Institute of Biology placed an emphasis on industrial employment for new graduates. In 1966, 10% of biologists were in employment in industry, compared with 41% in schools and 20% in universities (IBJ, May 1966, 40). In the subsequent years, the demands for biochemists, geneticists, pharmacologists, microbiologists, bacteriologists, toxicologists and immunologists increased, all these disciplines being relatively industry-oriented; while the demand for botanists, zoologists and general biologists diminished (IBJ, November 1966, 13ff; and The Biologist, November 1969, 141ff, and January 1970, 40-41).

In 1976, the destination of new graduates in biology was still a major concern, for only 13.7% of them (509 out of 3726) were employed in biology-related sectors: among these, 31% went into hospitals, 22% into universities, and only 19% into industry (The Biologist, August 1976, 142-43). In 1979, 14.7% (678/4620) of new graduates had jobs in related scientific sectors: of these, 31% were in the health service, 27% in non-teaching posts at universities, and 20% in industry (The Biologist, November 1980, 248).

Overall, between the years 1965-66 and 1978-79, the granting of degrees in biology more than doubled (2,027 degrees were granted in 1965-65 compared with 4,682 in 1978-79), while it relatively stabilized in physics (1,659 compared with 1,789) and in chemistry (1,967 compared with 1,937). But the average budget of biology departments was still half of that in physics departments (The Biologist, January 1981, 139ff).

How have women integrated the labour market along those same trends? It seems that in this case too, the thesis of a proletarianization suits the statistical portrait. Female biologists have tended to go into school science education in greater proportion than their male counterparts; also, they have not entered industry as much. Among new graduates of 1972, 35% of the men (866 out of 2,456 male graduates) chose academic research or further studies compared to 23% of the women (352 out of 1,508 female graduates). In that same year, 10% of the men (246 individuals) compared to 24% of the women (366 individuals) decided to proceed with 'teacher training'. Finally, among the graduates who obtained their first employment, 20% of the men (78/400) compared with 31% of the women (111/359) found employment in hospitals or joined local health services, while 44% of the men (174 individuals) compared to 23% of the women (84 individuals) ended up working in industry (The Biologist May 1974, 76). In 1979, 28% of the 2693 new male graduates compared with 22% of the 1989 new female graduates entered research, while

6% of the former and 12% of the latter decided to follow teaching careers (The Biologist June 1981, 140). It is interesting to note that during the 1950s and 1960s, a major concern of the Institute of Biology was the loss of women biologists due to marriage. As women had always formed over one third of all biological graduates, this was an important issue. At the onset of the 1970s, the only mention of women biologists in a study of "biological manpower" [sic] was indeed "the early retirement of women to raise a family" as a "source of wastage" [sic] (The Biologist 1971, November 1971, 175). In that same period, however, the Institute established a programme of allowances for their female members who were married in order to help them to participate more fully in the profession (The Biologist, November 1975, 171). In the early 1980s, the problem of women who were leaving employment was still being acknowledged but not to the extent of being alarmist.

At another level, regarding the increasing role of biology in the social economy, the 1970s witnessed both great optimism and the germs of a latent social back-lash for the profession. A reading of The Biologist over that period illustrates the extent to which the Institute of Biology considered the preceding thirty years as remarkable for the profession. British biology developed important knowledge in disciplines like immunology, microbiology, cell physiology, and agriculture, both in relation to domestic and developing countries' social needs (The Biologist, special issue of February 1980, 6-8). In 1979, after years of negotiation, a Royal Charter was granted to the Institute. It was held that this would accord biologists a professional status on the same grounds as physicists and chemists who had already had their own Charter for several decades (ibid., 31-33). With the onset of the decade, the conservation of the environment emerged as a growing social concern and as a major responsibility for the profession. In the mid-seventies, debates over the hazards and benefits of research in genetic engineering gave rise, in 1975, to the Ashby report which proposed that "subject to rigorous safeguards... such work should continue because of great potential benefits" (The Biologist, May 1975, 71). But the social controversies about genetic engineering were to survive several years the publication of the report (see Goodfield 1978; Wright 1986).

It is common at the turn of the 1990s to read of research advances in biology (especially in medical research) in newspapers and popular magazines. Research on the human genome and genetically related diseases is fast moving and highly competitive. But as biology becomes more conspicuously oriented towards industrial manufacturing, agricultural production, and clinical medicine, it is also the source of several social and ethical debates[11]. In relation to reproductive technologies and research on embryos more particularly, feminist groups are joined by several other groups in the political confrontations over ethical issues and projects of legislation[12]. Genetic engineering still precipitates political controversy, even though it now seems to be arousing the hopes of large parts of the lay public[13]. Molecular biology and research on DNA have indeed become the most prestigious scientific fields of the 1970s and 1980s, superseding physics[14].

In the present context, how can the 'professional' ethos of biologists be defined? How do biologists see their professional role in society? Do they still see their science as socially neutral; in their opinion, what is the impact of politics on biological science? What is their viewpoint on the situation of women in biological science? Do they see any possibility for a particular contribution of women as a group to the practice of biological research? How do they judge the present state of biological research? What has been or could be the specific impact of feminism in biology? What do they think of the feminist critiques of biology; of the projects of a feminist biology? The findings of recent interviews with British biologists should now help to answer these questions.

Organization of empirical study

The central aim of our empirical study is to marshall data illustrating the scientific beliefs, norms of practice, and socio-political attitudes of biologists towards feminism, rather than to have a statistically representative portrait of the whole population of biologists in Great Britain. Given this, it was decided that a purposive sample of informants should be used. This purposive sample, as its name indicates, was selected according to certain parameters relevant to our central research questions. It was therefore chosen so as to include three key variables theoretically justified, but also empirically motivated by the results of our pilot study: gender, age, and discipline.

Since our study aimed at a substantive set of data concerning the scientific and political discourse of biologists, the use of an interview protocol was required, in preference to a questionnaire. The form of the interview protocol provided flexibility and the possibility to develop the discussion on some issues if necessary.

Finally, based on the range of viewpoints displayed by respondents in the pilot study, it was believed that a small sample would be sufficient with respect to the purposes of our main study. A sample of thirty-one male and female respondents provided an array of responses reasonably exhaustive on each of the issues discussed in the study. Since we could not obtain statistical representation from our sample, the two "control" variables of age and discipline could not offer as such the possibility to make definite comparisons; it was assumed that they might, however, suggest whether generations and disciplines are important factors for an analysis of differentiations in biological discourse. It was, in fact, according to the results of our pilot study, that some issues (e.g. political attitudes in general or towards feminism) were believed to differ more importantly on generational and gender grounds than on disciplinary grounds, while scientific beliefs might differ more on generational and disciplinary lines than across genders. The differences expected among these cross-sections of respondents on specific questions were believed to be reflected to an extent in a sample of roughly thirty respondents.

Two preliminary versions of the interview protocol were piloted before it was decided to embark on the study proper. The possibility of distinguishing the impact of disciplinary backgrounds on the respondents' scientific views was explored thereof. But more than anything else, the pilot study tested the clarity and relevancy of questions dealing more specifically with issues like objectivity in biological science, biological reductionism, and feminist biology. It was also decided that the main study should include male respondents. Since most of the women in the pilot study were only able to discuss superficially the idea of a <u>feminist biology</u>, it became relevant to explore if there were noticeable differences between men and women on the role of feminism <u>in</u> biology in more general terms.

There were two series of interviews in the pilot study. A first series was carried out with ten female respondents, four doctoral students (aged 23 to 30) and six faculty members (in their late forties or fifties), between November 8, 1987, and January 18, 1988. They were all involved in research in medical schools or in colleges attached to the University of London at the time of the interview. Respondents were selected through the directory of the Association of Commonwealth Universities, the <u>Commonwealth Universities Yearbook 1987</u>, personal contacts, and lists provided by departments. A second series of interviews was carried out with four women teachers of biology (all in their thirties) between April 18 and June 20, 1988.

The main study included thirty-one respondents, eighteen women and thirteen men[15], doing research at the University of London, and interviewed between October 25, 1988 and January 13, 1989[16]. Five different departments (including three MRC research units) and one medical school are represented in the sample. The disciplinary and age representations of the sample is presented in the Appendix[17]. The sample of respondents is not statistically representative, but rather selected along the parameters of gender, age, and disciplines in relation to the purposes of our research.

For all pilot and main studies, a semi-structured interview protocol was utilized. Each interview was carried out by the author and tape-recorded[18]. Each was transcribed non-verbatim for analysis[19].

The data were analyzed so that we could discern patterns of response and occurrences, and 'count' them. Thus, our method bore some similarities with quantitative analysis. We also managed, however, to take account of some contextual factors, which enabled to qualify the responses of our interviewees when needed. This will come out more clearly in our presentation of the data in the next sections. Having said that, the construction of patterns of biological discourse focused primarily on the three main variables of sex, discipline, and age, and according to the three-fold thematic of individual attitudes, political views, and scientific judgments, with a view to a comparison with feminist biologists.

The pilot study protocol put an emphasis on the issue of discrimination against women biologists. The study showed indeed that the 'double-bind' of women scientists was considered as important by nearly all the respondents. The section discussing the impacts of politics and gendered styles of practice on the scientific process was modified, focusing on scientific issues more commonly referred to in biology (e.g., the strengths and limitations of reductionism in one's field of research; the scientific relevance of Dawkins's "selfish gene" thesis, of Wilson's sociobiology, and, more generally, of biological explanations of human behaviour; the actual or potential impact of the skills, workstyles and professional views of women biologists on the norms of scientific practice). This section was crucial, for it formed the grounds on which the identification of diverse types of 'biological discourses' was carried out, with special reference to issues such as a biological explanations of gendered behaviour and the neutrality of biological science. (In the analysis of data, it was borne in mind that the actual experiences of informants on the one hand, and their speculative remarks, on the other, ought to be differentiated.) Finally, and as mentioned above, the pilot study suggested modifications for the final sample of respondents, insisting on the disciplinary variable (with regard to their scientific views, especially of holism and reductionism), the age variable (with respect to individuals' attitudes towards the politics of science and sexism in the scientific milieu), and the utility of introducing the gender variable (to explore further differentiations in biologists' views of feminism in biology).

The elements retained for the final version of the interview protocol were the following. First, special attention was given to the issue of reductionism versus holism, and the biological explanations of behaviour. General questions were formulated in order to capture the imagination of biologists and to elicit their viewpoints on the role of science in society, the scientific status of biology compared to 'soft' and 'hard' sciences, and the importance and limitations of biological science in an understanding of human life. These questions were believed to help document more substantially the 'scientific ethos' of biologists. Secondly, with respect to feminism in science, a reworking of the questions in terms of personal experience vis-à-vis discrimination at work, and in terms of the perceptions of the differentials in verbal, intellectual and manual skills between men and women and their actual impact on research work was more likely to elicit relevant answers. In this connection, it also appeared helpful to ask respondents to position themselves relative to feminism in general, rather than feminist biology more particularly, by asking them to define how the feminist politics applies to the scientific milieu.

The final version of the interview protocol therefore contained six parts; 1) socio-demographic and curricular background; 2) personal motivations to become a biologist; 3) self-appraisal of scientific work; opinions on the importance of certain professional assets for biologists; 4) discrimination in the workplace; 5) feminism in the workplace and differential aspects of men and women's work in biology: 6) opinions on general scientific issues regarding biology, focusing on the reconciliation of holism with reductionism; the main functions of biological science in society; relevancy of sociobiology as a model of explanation of human behaviour; major criticisms about the current structure of biological research.

As mentioned at the beginning of this section, the forthcoming results were primarily based on the interview material of thirty-one female and male biologists. But the findings of the two pilot studies (fourteen additional women) were integrated in some appropriate occasions, as will be specified in the text.

The analysis of results will be organized in four sections: discrimination against women at work; differentials in skills and workstyles between men and women; the scientific issues relating to reductionism, holism, and explanations of behaviour in biological knowledge; and finally the impact of feminism in biological research.

Segregation and Discrimination at Work: How Female and Male Biologists Perceive the Situation

Respondents were asked if discrimination against women was still prevalent in the scientific milieu, taking their own personal experience or that of acquaintances as a reference. Overall men and women thought that discrimination did not handicap scientific institutions any longer. But the perceptions of women and men differed inasmuch as womens' opinions were more elaborate, more qualified. In comparison, men did not -- or could not -- elaborate on the subject. The age factor also seems to affect the perceptions of respondents. The following quotes[20] illustrate these differences. A middle-aged woman lecturer said:

> As far as I know it's equal opportunity [in industry] and that's fairly well up here [in the university] too. I do happen to know that when I was considered for a particular grant, the fact that I was a woman and married was raised and whether I would stick it out. Fortunately I had somebody to speak up for me and say, 'Yes, I would stick it out', and 'I was O.K. as a woman'. But that was twenty years ago. I think it's much more equitable now and there isn't very much discrimination. Although I think probably in promotions. There are very few women professors. In fact I don't know of any women professors in biology at the moment... And I think even if [women] apply, it's more difficult for them [to be promoted] against their male colleagues. Because it's generally male colleagues that are appointing them and they have slightly built-in prejudice that women can't do the job

as well. (i17)

In comparison, a male senior lecturer said,

> I believe there is no discrimination... in careers and promotions, not at all, not at all, I am sure; but [my experience] is based on a small sample. The [women] I have known have done very well... but... there was something which might be called discrimination. When I first came here, there was a man-only common-room. Although there was a female room as well. But now they're all mixed. So there's a slight change. I don't know if you can call it discrimination since women had their own room as well... Sexual harassment no doubt has occurred... there is some occasional harassment; it is deplorable... so there's a slight disadvantage for women. (i101)

Two younger respondents, both single, had different opinions. A young female Ph.D. student (who had previously studied arts and humanities at university) said,

> Yes, I do think so actually [that being a woman affected the judgment of male academics on my value as a scientist]. But it wasn't something that bothered me too much, except at one particular interview when the guy said, 'You know, you're obviously an extremely fickle female', which I think had I been a man he wouldn't have said to me. I think they [sic] would have just admired the fact that he had changed his mind and changed drastically... In this [present] place, it's not so much that you are discriminated against [as a woman]... But there was a recent article in Science magazine on self-visions of female Ph.D. students and how you do have to supervise them slightly differently ...I do not think it's so much that [my supervisor] talks down to me. It's just that he doesn't discuss results with me; he doesn't explain ideas so much. (i9)

Her young male colleague, for his part, said,

> I think [discrimination] is the other way around here: a lot of women are employed, but for sexist reasons [because the head of department is pro-females]... [as far as sexual harassment is concerned] there is nothing direct, but certainly the kind of teasing of a sexual nature, but that occurred between the men and the men, so [laughter] ... (i110)

As a matter of fact, one third of the women indicated that, in general, men do not seem to understand the scope of the impediments their female colleagues might have, as mothers or simply as women, to face in their daily routine and career. It seemed that although most male respondents were aware of the problems experienced by women colleagues (especially the double-duty of women with children), they obviously had a limited view of the importance of these problems, or else, were very cautious or defensive in their comments on that matter.

The issue of discrimination also raised various opinions among the women respondents themselves. There was a noticeable variation across generations. For instance, younger respondents generally realized they have never had to suffer the experience of being the token woman in their departments. In contrast, a majority of the older respondents (including four of the five women over 45 in the pilot study) said they had personally experienced such difficulties. On the other hand, younger female respondents seemed more sensitive to -- or less tolerant of -- the various forms of gender segregation and discrimination in the work milieu. But those who manifested more acutely their irritation towards segregation were mostly in their thirties and forties, and mothers of young children. This is not surprising since most of these women are at crucial stages in their scientific careers.

Not surprisingly either, our results show that biologists consider that the main source of discrimination against women is related to motherhood. A majority of respondents, male and female, believed that infant care and the emotional bonds between mothers and children remain the outstanding hindrances for women who seek professional advancement in science. In the main, women argued that since there is a prejudice against them (employers, so they said, will frequently assume that at one stage in their careers women will take a break to bear and raise children); that employers tend to choose males instead, presuming that women would not be totally committed to their jobs[21]. Yet a majority of the mothers in our sample (five out of seven in the main study, and equal numbers in the pilot study) firmly believed that having children would considerably reduce their chances of making an outstanding contribution to biology. Five of the eleven childless women (five out of seven in the pilot study) expected this would have (or would have had) such an impact on their careers. As this young biochemist said,

> At the moment, certainly, I would not like to have a child and give it to somebody else to bring it up. That's how I feel. I would not have enough left for science [if I had to raise a child]... I mean there are some people here who manage [to do both] and seem to be very good about it. But

they're not getting anywhere in science. And that's how it is... It 's so important, achievements [sic], in science. (i#9)

Indeed, three of the four women with young children (two out of the four women in the pilot study) indicated that they preferred to have a good balance between career and family rather than make the sacrifice of one option in favour of the other. These results ought, however, to be interpreted bearing in mind that a woman's moral commitment is expected to be towards her family first, and only secondarily to paid labour, and that these expectations might have produced certain 'artefacts' in our respondents' answers[22].

Five out of eighteen women (ten out of thirty-two, including the pilot study) mentioned spontaneously that the most difficult moments in their careers were precipitated by the additional stress of family responsibilities encroaching on work commitments. These respondents were all in their mid-thirties or older. (One of the respondents (i14), single, said that although she had to take care of her father, she thought it was easier for her than for married women to handle professional and domestic duties simultaneously.)

Three women (seven including the pilot study) maintained that the worst moments in their careers were when they suffered from discriminatory hiring practices or from sexist attitudes, usually on the part of older male biologists. Although it did not force them to quit their jobs (except for one), it hampered their scientific productivity to a significant extent and for an extended period of time.

Not surprisingly, among the six youngest female respondents under thirty-five years of age (twelve including the pilot study) three women (and three more in the pilot study) mentioned that the doctorate itself was the most difficult experience they had had as scientists. They did not seem to consider that segregatory behaviours of supervisors could represent difficulties, although they generally agreed that sexist remarks and patronising was still very common, especially among the older generations of male scientists.

Several women believed that there was still a lot to do before women could get equal opportunities in paidwork in general and in science in particular, but only a few believed that discrimination was still prevalent in the scientific milieu. The majority seemed to think that discrimination was an isolated practice, more common in the old days than now. Indeed female respondents preferred to use terms such as 'sexist' or 'patronizing' attitudes rather than discrimination per se. It was generally believed that although conscious discrimination still seemed to exist in hiring practices, attitudes and behaviours were considered currently to be the main predicaments in women biologists' careers. Moreover, women respondents seemed to think that in general segregation was practised unconsciously, and that as such manifested more commonly in the behaviours and attitudes of scientists, usually of older males. In sum, the majority of women indicated that sexist attitudes still prevailed, and some even suggested that this was somehow 'inevitable'. Only one female respondent said that the scientific milieu was different from other professional sectors in this regard. She argued that scientists were usually exceptionally progressive people compared to other professionals, and that this was reflected in their attitudes towards women biologists.

Women respondents were generally concerned by the fact that there were still few women at the top of the academic hierarchy. In general, they believed that this was due mainly to the remnants of the 'old boy network mentality'. A third of our female respondents, nearly all past forty years of age, said that it indeed precluded the full integration of women into relevant information networks, discussion groups, or the 'inner circles' of decision-making in the biology milieu. Younger women, as noted earlier, complained more particularly about the patronizing attitudes of their male supervisors or superiors.

Women generally agreed that discrimination was currently minimal in the scientific milieu, but that, on the other hand, male "patronising" was still pervasive and affected women who did not always feel men took them seriously as scientists. A mature research student in mycology pointed out how cross she gets when people made jokes about her job: "Oh! you mean you cook mushrooms at work?" (i6). A woman lecturer in her late forties contended that the "biggest problem women face at every level of the academic hierarchy is that men do not take them seriously" (i16) [my emphasis].

Seven of the eight oldest female informants (mid-forties or over) said that they had to be twice as good as the men in order to be accepted at university or to get a job. (The same applied for the pilot study). It seemed to have been especially the case for those women who wanted to do medicine in the first place -- before they finally opted for biology. On the other hand, some of these respondents suggested that, compared to medicine and other sciences (such as physics), biology was a field where women did not necessarily have to be that much better than the men in order to be promoted, since there has always been traditionally so many women in the field. A good number of women biologists have observed, however, that discrimination in hiring practices still existed to a significant extent, and that it was especially hard for women to get long-term jobs after the Ph.D. It was especially apprehended by young postgraduates. The following quotation illustrates this state of mind:

> [Although it was more difficult for the previous generation of women] who had to fight every inch of the way; at the same time, I think we're going through a phase where it's going to be much more difficult again. Because everything is on grant money, I think there is this attitude of, 'We've got this money. It is for a two or three-year project. If we take on this married female, will she produce at this time; will she be having days off sick every time her kids are off sick?'... We [women] have had a very short phase where it has been slightly easier, but not equal. (i#8)

What do women do when they are facing sexist or discriminatory behaviours? A few respondents said women should take a stand to abolish discrimination; but that this was risky because their jobs and reputations might thereby be jeopardized. A good number of our female informants said they simply became angry. But a lot of them also said they preferred to remain passively angry rather than to retaliate. Several of the older female informants believed that younger women were more forceful opponents of sexism. But, according to our results, it was not necessarily the case.

A great number of women, mostly students and young lecturers, said that they sometimes took advantage of "playing the game" of the ingenuous female with the patronizing male boss, in order to benefit from the situation if possible. These women biologists were not proud of themselves however, nor did they condone the usage of such a tactics. They admitted that it just helped to perpetuate sexist attitudes towards women in science. A young respondent (i10) even contended that women were their own worst enemies since "sometimes [they] behave as stupid feminine females".

These features of the biology milieu finally made several respondents conclude that, first, the milieu tended to attract women that were as strong, ambitious, and as "ruthless" as men (especially in the fast moving and competitive research sectors of molecular biology and medical genetics); or on the contrary, women who would let themselves be treated as non-serious professionals, or worse, sex objects. Secondly, it implied that, for women with children, the possibility of having an outstanding scientific career was likely to be out of the question. The idea that a scientist ought to be dedicated to her/his job was indeed still strongly entrenched in the minds of a great many women who felt condemned to choose between "the two worlds". As a result, women generally tended to minimise the weight of discrimination as a factor explaining the relative absence of female biologists in high rank positions. Only two respondents, both in their forties, suggested that the absence of women in top level jobs or on elective committees was still greatly due to the 'old boy network' ethos of the biological institution.

In a parallel with women, men were asked what have been the most difficult moments in their careers. In contrast with the women, there was not any significant variation in the responses of different age groups who, in the main, whether students or lecturers, seemed to have always had the same difficulties throughout their 'careers'. Not too surprisingly, men generally experienced difficulties that contrasted with the difficulties that were most important to overcome for women. Five out of the thirteen male respondents, aged in their thirties or so, said they have had great problems during their doctoral studies, a time when they had to build their confidence as scientific researchers. Three other respondents, the two youngest and the oldest respondents, more specifically pointed to anxiety, wasting time over experiments that proved to be unuseful, and running out of ideas. Three other respondents discussed organisational drawbacks in research, rather than personal problems as such. For instance one stressed that grant applications, teaching duties, and administrative commitments were "continuously demanding"; the others argued that to be pressured by the competition for funding and promotion was a constant source of stress.

Regarding the issues of <u>discrimination against women</u> and <u>sexism</u>, six male respondents maintained that, compared to other scientific fields, biology had always had a lot of women and that, therefore, discrimination was rather absent from it. As one post-doctoral student remarked, "University is more open to them [women] than before... There are a few women at high levels, [but] there is no reason why there should be so few" (i103). Several respondents shared his opinion. This tends to confirm the fact that men generally minimize the problem of discrimination against women in science, and seem to believe that if a women is scientifically gifted, nothing but personal reasons would interfere with her professional advancement in science.

It is worth noticing however that, among both the younger and the older male informants, several men believed that a majority of the women who have succeeded in science are relatively more assertive, if not aggressive, than the average woman. They acknowledged the fact that these women, namely the older female biologists, had to fight prejudices and to handle domestic duties and career commitments simultaneously.

Some of these men also added that they have become more aware of discrimination against women since they got married to scientists. But their awareness did precisely not transcend the level of discourse. One of the three male respondents with children was very defensive, and insisted that not only his wife, but he himself as well had sometimes had to take time off work to care for the children. The other two men simply acknowledged the fact that their wives had to bear familial responsibilities and to strive with the additional obstacles thereby imposed on their careers, although without mentioning anything about their own involvement in family care. Of the other four married men, only one criticized severely the prejudice of employers against married women. Taking his wife as an example, he rejected the idea that women could not be as committed as men to their jobs if they had children.

In fact, for a majority of male respondents, discrimination no longer overtly existed. For them, remnants of sexism and segregation were more to do with "self-imposed" discrimination or "subconscious" attitudes on the part of women and men, than on outright sexist practices. For instance, according to some men, the lack of assertiveness and confidence of women, and also their preference for only some of the biological disciplines form the very barriers that women impose on themselves. The following quotation speaks for itself: "There are quite a lot of women in this department. There is no reason why women should not come into biology..." (i103; my emphasis). In other instances several respondents maintained that subconscious attitudes were "hard to change". Likewise, the "secretary syndrome", as one respondent observed (i106), was still strong, producing unconscious self-discrimination and perpetuating the obstacles for a more full participation of women in scientific meetings and discussions.

Many male respondents considered the issue of discrimination against women in biology as tantamount to "self-discrimination". They believed that discriminatory practices no longer existed, and suggested instead that women were hindering their own scientific careers by deciding to have children and spend more time at home with them. They also maintained that their general lack of confidence and shyness greatly hindered their chances of promotion. Finally a great number of them argued that sexism at work came from both sides, from men as well as women themselves.

To summarize, the majority of men and women respondents believed that the segregatory attitudes of both women and men were inevitable, and also that discrimination was somehow self-imposed. Some women respondents even argued that it would be impossible to change the attitudes of men overnight or to transform the family structure within the next generation. However, a majority of women held that still very few men understood the extent to which women have to bear the double duty of being a housewife and a scientist, or to suffer sexual prejudices. Finally, women noted that younger male biologists were more aware of sexism than older biologists.

Differentials in Skills and Abilities Among Women and Men Biologists

Do women biologists have any specific skills or shortcomings which are more common to them than to their male counterparts? This question received affirmative response from both female and male respondents; there were few differences between the opinions of the two groups. The differences resided in the fact that women reported certain aspects of a job to which men did not necessarily pay attention. More importantly however, the differences in the skills and abilities of men and women biologists (as reported by our informants) somehow reflected the differences between what are considered to be more or less important assets to succeed in biology.

Roughly half of the respondents, male and female, observed that women biologists were more meticulous, better at manipulating specimens and more careful in conducting experiments. A male botanist stressed, for instance, that women were "remarkably good with compounds" (i102). The other half of the respondents, however, suggested that this was not necessarily true; that there were also women who were as "sloppy" as men; that feminine meticulousness is just a "stereotype".

Several female respondents contended that women were more perfectionist than the men, especially in technical work; but they also suggested that this did not change the scientific results much at the end of the day. In fact, most believed that this might be, on the contrary, more time-consuming than really beneficial. Hence, on the one hand, a good number of respondents considered meticulousness as commendable, on the other, believed that it was more time-consuming than really beneficial. However, according to some of the female geneticists interviewed (in both pilot and main studies), meticulousness was crucial, for it made all the difference between a failed experiment and a successful one. Here again however, the respondents did not really consider that womens' meticulousness could possibly be the source of an outstanding and specific contribution of women to biological science in terms of a production of new knowledge. On the contrary, most respondents suggested that if male biologists were less methodical or punctilious than the women, on the other hand, men were on average more imaginative and innovative. Moreover, imagination was considered by the majority of both male and female respondents to be of greater scientific value, for it is of key importance in the interpretation of results and the initiation of new scientific hypotheses[23].

It was largely agreed that, in general, women are not as prone to present papers, put forward new ideas and stand out as the men. Women were seen as lacking confidence, more tentative in suggesting their ideas, and more worried about doing their job well. One male respondent, for instance, said he had often observed that women were not as prompt as the men in using new pieces of apparatus (i104). At another level, one female respondent remarked, "Girls [sic] are more sensitive to criticism. It is easier to criticize the men [sic]" (i1). In several other instances, mainly raised by women respondents, men were said to be more prone to voice their opinion, to "sell" ideas, and to write grant proposals with more confidence. Some respondents stressed, on the other hand, that women were better than men at communicating, giving explanations, and teaching. In general, informants considered being single-minded as very important for a scientist, especially if he/she had the ambition to succeed and to contribute outstandingly to his/her field; and as a number of women thought, women did not seem to be as single-minded as the men. Many of them said, however, that employers were generally overly prejudiced in this regard and that, sadly e-nough, as one young woman said (i#8), they often rejected "good female biologists" in favour of "careless young men".

Several respondents suggested that men's attitudes might explain part of why women are, in general, less confident and do not put themselves forward as much as their male counterparts. As one middle-aged female informant suggested, "Women are more defensive because of residual bias" (i2). Also according to some female respondents, women are expected to be supportive wives and caring mothers in the first place, and this might affect their productivity at work, but also their relative lack of ambition and lower confidence. For example, as this young lecturer said,

> I think what happens to male colleagues is that oftentimes their girlfriends are doing things, or are trained, or believe that they should be totally supportive of their boyfriends or husbands. So they don't make such a big fuss about things... Basically [my boyfriend] didn't like me talk about science; he didn't like the people I worked with in my department... 'What every good scientist needs is a good wife', my boss used to say... (i4)

And she continued,

> I enjoyed doing work in my Ph.D.... I like doing the work, I like solving problems. But I'm not the kind of person who sort of thinks, 'Oh! that's something we should really be working on. It's a good idea; we should be doing this and that', which is what more ambitious male or female scientists do ... In the world of science today, you need a certain type of personality to actually succeed: it's not just ability, it's having a persevering and ambitious personality.

Another woman, a mother of two, thought that

> It is more difficult to be single-minded if you're a woman in that you've got responsibilities at home which are different to a man's however one equates male and female in the home. For example I find it quite difficult to go away for conferences because my daughter hates it when I go away. Now I either choose to go away and be a scientist ... or I choose to be a mother and not go away. Whereas I don't think any of my male colleagues would hesitate to go away to a conference... I don't know any of them who would stay at home for family reasons... which is a bit restricting I think because I prefer not to go away. So I think one has a drawback in that you're not part of the scientific community as you might be because you 're effectively not fully in. Plus the fact that if you've got something going on you can't stick around in the lab until eight o'clock, whereas several of my colleagues can. They just phone up, 'Well I'm not coming home until...'. But I can't deal with it... emotionally. (i17)

A number of female respondents criticized severely the behaviour of some of their male colleagues who, they believed, were single-minded at the expense of their wives, collaborators and assistants. The most revealing remark however, came from a middle-aged woman lecturer (i16) whose position illustrated how several other women felt -- that they were being treated differently, and, sometimes, not even seriously as scientists. She argued that patronizing attitudes affected greatly the professional drives and personal self-worth of women biologists.

> The attitude [of academics towards male and female students] is different: the attitude towards the woman is patronizing, towards the male it should be aggressive -- you 'have' to put a young man in his place. You feel, as you hear this, that it is like a ritual for the boy: he'll reach manhood once he's gone through this; but the woman, she will internalise that she can stay there as long as she remains silent... The main problem [for women] is to get the men to take them seriously.

At another level, a great number of female respondents pointed out that female biologists were, in general, better organized, and that they were able to handle different things at the same time. This was, according to many respondents, possibly due to the fact that women are used to having double duties. A few female respondents even argued that women were better at coping with deadlines and that they could bear psychological stress more easily than men. Finally, a majority of women said that the men would not be bothered with cleaning up the laboratory after a day's work. Although several female respondents admitted that women could be 'sloppy' too, it was generally agreed that it is the women who are expected to do the washing-up at the end of the day. Several women maintained that men needed, most of the time, to be reminded to clean up their laboratories. Surprisingly enough, none of the male informants pointed out that being tidy and keeping the laboratory clean was worth mentioning. The following quotation of one of our male informants (i107) illustrates the chasm between men and women on this matter:

> The only long-term collaboration I've ever carried out with a woman is with my colleague... I saw her as too careful and too obsessive; she saw me as sloppy. But I think that's the sort of things that happen with collaborators, period [sic].

The great majority of male respondents expressed the view that a relative lack of assertiveness and of confidence was characteristic of female biologists. The following quotations illustrate this:

> Yes, there might be a difference there [between men and women]. Quite a lot of female students here seem to talk less about their work... (i112).

> I think, but I might be wrong, that there's a lot of self-discrimination, of fear

> [on the part of girls]; a lack of self-confidence of girls. I like ambitious people, whether they are male or female it doesn't bother me... but I think a lot of [womens'] timidness is due to lack of self-confidence. I don't think there is any difference in ability or intellect. This is the social constraints [sic] which are often self-imposed, due to how they got treated as younger people; you know, 'girls ought to do so, and boys something else'. That's the sort of thing which polarizes [womens'] own impatience about themselves... [Women] are repressed from speaking their mind. They are not assertive. (i113)

> In general, and, I mean, it is a generalisation -- there are exceptions both ways [coming from both my male and female colleagues], self-confidence seems to me to be one [difference between male and female colleagues] that I find a bit bemusing sometimes. I noticed it among our students as well... female students have less confidence in themselves. They are much more tentative, they worry about things... But I don't think in later parts of careers [it is so much the case]; either you acquire self-esteem or you hang yourself! (i106)

Overall, male and female respondents observed that men and women generally display the same scientific potential and abilities as biologists. They believed that differentials of skills and abilities among biologists had more to do with individuals than with gender. The only exception to this point was referred to as a general lack of confidence on the part of women biologists. This, it was believed, might have more seriously limited womens' full contribution to the field of biology.

In another segment of the interview, a list of twelve job characteristics was presented[24] to the respondents who were asked to rank them in order of importance and to picture an 'ideal-type' of biologist. Each respondent was then asked to do the same thing with his or her own strong and weaker points. The results give some indication of how men and women biologists value their own work and evaluate their potential contribution to their profession. Several respondents said that it was hard to tell which characteristics were more important than the others, that they were all important and that every good biologist needed to have a good balance of all of them. What is most interesting is not so much the variations between men and women, or across generations, but the similarities and points of consensus. Respondents generally favoured two assets: innovation and imagination on the one hand, and self-criticism on the other. These assets were singled out as being the first two most important assets by either group of men and women. Theoretical skills, practical skills, meticulousness, and being open to criticism came, on average, in a second group of assets. Surprisingly enough, being confident or being hard working did not emerge as particularly important assets.

These results, sketchy as it were, confirm the strong impression that biologists consider intellectual ability and 'lateral thinking' as more valuable than practical skills or meticulousness in laboratory manipulations. If one compares these results with what respondents believed to be the strongest points of each sex, one might ascertain additional explanations of why women have generally tended to undervalue their scientific contribution as good experimentalists. We shall explore the meaning of these findings further in the last section of this chapter. Let us now look at more substantive issues relating to the scientific views of mainstream biologists.

Reductionism and Holism in Biological Research Practice

In this section, we enter the heart of the matter. Where do biologists, be they molecular biologists or ecologists, working in zoology or human biology, doing clinical studies or not, stand on the issues of reductionism and holism? How does this affect their views on the scientific value of sociobiology, for instance; or on the objectivity of biological knowledge in general, and on explanations of behaviour in particular?

Our results suggest that, in the main, biologists lean towards a Popperian viewpoint on biological science. That is, they believe in the idea of a progress of biological knowledge, and in the epistemology of critical realism and falsificationism. They consider biology a fully-fledged science. They seem, however, to admit that biology, in comparison to physics or chemistry, does not offer the empirical evidence of general theories that would satisfactorily vindicate the rules of falsificationism and the pragmatic criterion. The reason implicit in this is that biology comprises objects and phenomena which are relatively more complex and very sensible to forces in their environment. As a result, they do not lend themselves easily to scientific experimentalism, mathematical formulae, or scientific 'laws' of causation.

Our results indeed suggest that biologists are not reductionists in the strong epistemological sense, as the Radical Scientists' critique suggested. They do not generally endorse the 'ontological' or 'philosophical' strands in which biological forms are assumed to be reducible to molecules and chemical properties. They are also far from subscribing to a reductionist view on human biology and behaviour. In fact, they would shy away from any attempt to explain human or animal behaviour, with some exceptions which will be identified later in this section.

Instead, biologists generally lean towards methodological reductionism; they claim practical reasons to do so. These reasons are mainly dictated by the state of development of biology as a 'hard' experimental science. Of course, this could be explained partly by the fact that molecular biology and biochemistry have somewhat 'shrunk' the scope of biological questions being addressed within the current biological research agenda. This is not denied by either critics of biology or mainstream biologists. In fact many biologists think that the current domination of molecular research is susceptible to a "fashion". Hence, they foresee a return of the pendulum in favour of traditionally well established disciplines, such as physiology and genetics (or of promising branches of study such as ecology), or at least, expect a better equilibrium between biological disciplines, inviting to a better integration of the life sciences.

On the other hand, our results suggested that biologists differ on the degrees to which they adhere to the reductionist approach. Molecular biologists and biochemists, for instance, appear "more committed to reductionism" (as one of our respondents (i112) put it), than evolutionists, botanists, or even geneticists. Botanists and ecologists regard holism as a most fruitful framework for biological studies. For them, the notion of environment evokes the fact that living phenomena are constituted by, and react to, a wide array of 'conditioning' factors, and, as a result, lend themselves more readily to interactive and holistic explanations. Results indeed suggest that there are variations in the opinions of biologists about reductionism, and that these are related to both disciplines and research subjects. In addition these variations tended to reflect the age structure of our sample, for younger respondents appeared to lean towards a stronger reductionist stance. This might, however, suggest that the differences between age groups simply reflect work experience and training tradition.

In general, however, respondents believed that holism and reductionism were two necessary ways of looking at biological organisms in order to attain a complete understanding of life processes. They also seemed to think that, in daily practice, biologists must choose one approach rather than the other, and that this choice is dependent on research goals. The majority of 'mainstream' biologists does not reject holism in favour of reductionism as an approach. Rather, they consider that experimentalism is the only way of separating erroneous explanations from valid ones. In short, they believe first, that methodological reductionism is preferable to systematic observation; and secondly, that mathematical coefficients and 'mechanical' laws are preferable to "descriptive" results and "woolly" interpretations. But a preference for methodological reductionism does not entail a dismissal of holism or interactionism altogether. These latter notions are, on the contrary, considered especially important in biological theory. Consequently, 'mainstream' biologists do not differ very much from the scientific position of Radical Scientists on this question.

With regard to explanations of human behaviour, however, our results suggest that biologists hold fairly common-sensical, even relatively cautious, opinions. This offers a real contrast with the arguments for a biology of human mind and behaviour developed by Radical Scientists. The fact that mainstream biologists do not want to make any strong scientific claims concerning human behaviour is not so surprising if analyzed in the light of two factors. First, the majority of our respondents did not seem to be interested in studies of human behaviour on a scientific basis. As they suggested, modern biology (in contrast to psychology) is not so much concerned with the understanding of human behaviour as with minute bio-chemical processes and elements in living organisms and the human body. Secondly, none of our informants opposed the idea that upbringing and culture have far more importance than biological factors in explanations of human behaviours, and of gendered behaviours in particular.

All the above results appear especially interesting in view of the criticisms made by feminist critics and Radical Scientists. They tend to show first that, empirically, the feminist critiques of the biological method and of theory building are not totally justified. But they also strongly suggest that the institutional setting (or disciplinary structure) of biology does not offer the sociological condition for a vindication of a project of feminist biology, simply because, as shown in chapter 1, feminist critiques focus on human biology and behaviour rather than embrace the whole array of biological disciplines.

Our results suggest that there is a genuine challenge of molecular reductionism within mainstream biology itself. For instance, non-molecular biologists strongly criticized molecular biologists for an excess of confidence in reductionism, scientific short-sighte-dness, and professional arrogance; as well, a number of molecular biologists and geneticists are beginning to question the research agenda of their own disciplines. Some of them suggested that it is only a matter of time before a better balance between molecular reductionism and holistic-oriented biological disciplines is re-established. Subsequent to a frantic period of gene mapping, multidisciplinarity will resurface in order to fully address all the new questions molecular biology will have left unanswered.

Despite all the qualifications mentioned above, results also tend to show that self-criticism does not extend beyond a virtuous discourse in favour of interdisciplinarity in biology. Respondents did quite often, it is true, overstate the importance of methodological reductionism, to the extent of neglecting alternative methods and sparing the effort of testing holistic explanations. One of the consequences of this might well be, as dialectical biologists have said, that biologists tend to resort to an "additive model" rather than to a genuine holistic and dialectical model of explanation capturing the gist of the dynamic process between organic and environmental components in biological phenomena.

As the current structure of biological science encourages strongly-focused research and applied biology, efforts in the direction of multidisciplinarity and basic research are minimized, as many of our respondents admitted. In a nutshell, there is little evidence of a shift towards multidisciplinarity, and literally none that suggests the emergence of a dialectical paradigm as propounded by Radical Scientists, or, as proposed in feminist projects of biology, interdisciplinarity between biology, psychology and sociology in the understanding of human behaviour.

In the same vein, many respondents approved of some form of social responsibility for scientists. But their political stance appeared rather timid compared with that of Radical Scientists. For example, our respondents were aware of the new social problems arising from their field (e.g. in terms of human ethics, protection against environmental hazards, priorities in the biological research agenda). But, according to them, ideology and politics did not and could not contaminate scientific knowledge per se. In fact, although several informants suggested that the 'freedom' of biological science was dangerously constrained by the financial structure of research, none claimed that this could affect the actual 'truth-content' of good biological research.

It appears reasonable to think, therefore, that even though 'mainstream' biologists are critical of their discipline and institution in general, their criticisms remain rather low-profile. Obviously, it is one thing to be critical of biology; it is quite another to reject it or abandon it altogether. The position of our respondents is a clear evidence of this. Mainstream biologists are mainly critical of the 'abuse' of biology rather than its 'use'. On the whole, they defend the instrumental value of biological research, both as a value-goal (i.e. the epistemological tenets of biology as an empirico-analytical science governed by the pragmatic criterion), and as the basis of value-laden explanations (i.e. more or less sophisticated determinist models of explanation). As such, they tend to refrain from scrutinizing the intrinsic limitations of biology as a form of understanding human life and human behaviour.

Let us now look in more detail at the interview results. We shall present these under four headings: reductionism, holism, and interdisciplinarity; truth and objectivity in biology; sociobiology and the explanation of human behaviour; and finally, criticisms of the current structure of biological research.

Reductionism, holism, and disciplinary perspectives

Nearly all the respondents indicated that they did not have any scientific exchanges with colleagues in other disciplines of biology. The reasons given had generally to do with the discrepancy between research projects, combined with lack of time and interest. Comments such as: "It's a waste of time [to discuss with colleagues in other disciplines]" (i109); or "I have lost touch with other biologists" (i106), or "Biologists do not discuss with each other unless it is about the same research" (i104) illustrate this. It is only a minority of respondents who suggested that biological disciplines were in fact integrated. In spite of this, the majority seemed to accommodate itself well to the lack of interdisciplinary dialogue and the institutional divisions between specialities.

There seemed to be an exception to this rule however. In the main, biologists were very critical of the inflexibility and single-mindedness of molecular biologists. The domination of molecular biology of the life sciences has even begun to engender confrontations within the group of disciplines most closely surrounding molecular genetics. As one molecular biologist (i11) contended, physiologists, and even geneticists, have become very angry at molecular biologists because the latter are quasi oblivious of their work. Another geneticist (i15) concurred with that statement, saying that the findings in molecular biology are now begging a tremendous number of questions that will need to be answered with the collaboration of other disciplines. Among the nine biologists working in the research area of human genetics, those two biologists expressed the more critical views. At the other extreme, only the three youngest molecular biologists seemed to hold strict reductionist views. The other four respondents claimed to have reservations on the explanatory scope of molecular genetics, and were especially careful in their claims regarding mental illness more especially.

Among our respondents, there seemed to be very few 'strong' reductionists in the theoretical sense. If it were the case, they tended to come from molecular biology or genetics; and the younger they were, the more reductionist they seemed to be. A comparison between the comments of two geneticists engaged in medical research illustrates this more clearly. Asked about how genetic or molecular reductionism could explain a complex human phenotype or a behavioural pattern in higher organisms, a young molecular biologist said:

> You reduce it; you reduce it to the basics which is chemistry... or something like molecular biology. Or you have to classify things. You have to work down from a gross scale right down to what's basically going on. It depends what you want to look at too... The environmental interactions are causing changes at the fundamental level. You reduce it to a chemical interaction... [Biologists] are trying to pinpoint a gene for schizophrenia, so even something like that, which is totally mental manifestation of a defect, is reducible to, maybe, a gene that has a wrong coding sequence... Yes, there are other factors that act on it, yes certainly, but the primary defect is something physical, tangible. (i10)

In comparison, this geneticist in her mid-forties said,

> Geneticists are getting ... to be respectable in inherited behavioural traits, but only just... I think these sort of terms [holism and reductionism] that people exercise their mind over and write articles about are not relevant to my day-to-day life... You use the approach if you like, that is more relevant to the particular problems you are asking. You may say you reduce down to the elements, but you may want to go back the way and consider the relationship with the whole. I must say I haven't read much on it [the debate between holism and reductionism] because I find it... [is] a lot of talk that is not going anywhere very much... Although we are homy with small elements when we study genetics, our interests... I mean, human behaviour is something that people get nervous about in my particular field. But more tangible things like human diseases or human phenotypes in general, is really what interests us... Well, I suppose some people do think that they can pick their way to the final sequence of nuclear type and that will be all the

answer... But I suspect that when we come to think about things as complex as human behaviour, I mean I find it hard to imagine really how that will best be approached. I think there is no doubt you'll be able to identify genes... which will explain why Joe Blocke's family has, you know, a certain amount of manic-depressives; but that's not really telling you very much, I mean, it's a great step forward but it's not telling you much about the whole of human behaviour and how the brain functions and all the rest of it. (i15)

The botanists, more particularly, were fairly harsh critics of molecular biology. Without dismissing methodological reductionism or biochemistry altogether, they warned against the single-mindedness of molecular biology. Four respondents mentioned this, while the other three seemed to be quite happy with laboratory trials.

For instance, a botanist identifying herself as an ecologist, argued that

[Molecular biologists] have lost sight of the organism. I think they should know their organisms. The most important thing is not to allow people to specialise too early in education. We should make sure that they all have a broad background. Because if you become a reductionist very, very early, you become incredibly blinkered; so you may know a lot about one gene or chromosome, but you probably don't know a thing about the environment that that organism lives in; how the environment affecting the genome evolves in response to the environment becomes what it is; the sort of pressures that affect the genome. They're working in a vacuum on just one single abstract thing. It ceases to be biology; it becomes chemical. So I feel quite strongly that it has its place -- reductionism-- but it has to be supported by people who have a broad knowledge. (i12)

Two other botanists also worried about the fact that young biologists tended to join the crowd, "the wizzy sector" of molecular biology, without knowing much about "their specimens", and forgetting easily that biology is first and foremost about "whole organisms". In contrast, their colleague, interested in the genetics of plants, explored the paradox of breaking down objects into their minute elements for the purpose of experimentation, and then interpreting the results back from the original point of view about the whole (i4). Laboratory experiments, she said, do not reproduce phenomena in vivo well. Yet all living forms are, in the last instance, reducible to ultimate forms like genes, molecules, and proteins. She believed that although living forms were very complex, biological determinism was the key to biological explanation.

But is holism the answer to the problem? It does not seem to be. In fact there are criticisms that biologists are levelling at both reductionism and holism. What are they?

One zoologist said that he saw a lot of virtues in holism; but he did not mean that reductionism was "doomed to failure" (i108). He said that reductionism was rather a limited way of approaching biological phenomena:

> A reductionist approach that is trying to explain the central nervous system for instance is not doomed to failure... In biology, [the debate between reductionism and holism] is a very powerful stimulus for research... But if you take a reductionist point of view, it limits your perception; you've already got a preconceived idea as to what you're going to find out... reductionism is quite high on precision; but it may be quite low on innovation and driving force for throwing up hypotheses... holism is much more difficult to handle; it is intellectually more difficult to come to terms with, to formulate your hypotheses and test them adequately. I think they're probably the two ends of the spectrum, the two extremist views, I 'm quite happy with the continuum to be unresolved.

One of the ecologists interviewed was more severe. She first stressed that she had never herself been very much interested in biochemistry and preferred the observation of specimens in their natural environment, and that this might have been beneficial from a scientific point of view. Her criticisms of reductionism were, however, mainly oriented towards molecular biology. She considered that this discipline was unimaginative and represented the epitome of routine work. As she put it:

> Oh! I feel sorry for [molecular geneticists]! Because their subject is so boring, as far as I'm concerned. They'd come and say, 'I've cut this chromosome here, and I've stuck it in there, and I've put it in that. Wow!' ... I don't argue with [molecular geneticists]; I just feel sorry for them. I've learned some genetical technique [for my work] and I must admit that after a while I said to myself, 'what is the fuss about this? This is absolute cookery!...' It is absolutely simple, it is high-bound jargon, you can't become a member of the club unless you learn the right word to say... They have a sort of mystique about what they're doing. In fact it's far less demanding than designing a good ecology project. (i12)

But one of her fellow ecologists was less radical. He agreed that biology was, in general, too reductionist. Yet he considered a reductionist discipline such as biochemistry is primordial for the whole field; "biology cannot survive without biochemistry" he said (i102). (Not too surprisingly, this male respondent had been trained in physics before he entered into a 'second career' as an ecologist.)

The majority of respondents tended to agree with the suggestion that biological objects were more complex than physical objects and that, as such, they might lend themselves more readily to holistic interpretations. Yet they observed that holism was hard to handle experimentally and usually did not produce any conclusive results. In contrast, it was argued that the reductionist method used in biochemistry and molecular biology, for instance, was a formidable way to produce results amenable to scientific generalizations and biological laws. As an evolutionist argued, "all biologists talk holism but practice reductionism" (i107); or as another respondent, an insect physiologist, said:

> The problem is that with a reductionist approach you just look at this here, this here, this here, you know; you're not seeing any sort of synergisms, or what kind of effects they have on each other... Really, this is the difference between theoretical and experimental, isn't it? You have to use the two together. I'm quite interested in theoretical stuff really. But I like the two. I don't think biology is theoretical enough really. (i109)

It must be stressed at this point that not all the respondents felt at ease with the question of reductionism. Among the younger informants especially, a good number were not acquainted with the debate between reductionism and holism. Several of our informants indeed considered this debate as irrelevant to the actual practice, or else, ill-founded, since as they mentioned, biologists generally use both approaches. Indeed, the reconciliation between the two approaches did not seem to pose a major problem for biological researchers. Only five out of the thirty-one respondents thought that this problem was a genuine one for biology. For the majority, however, the tension between these two approaches constituted the driving force of biological research. Or as illustrated earlier, most respondents, be they pathologists, geneticists, plant biochemists, or population geneticists, argued that biologists merely choose the approach which is best suited for the research problem at hand.

It has been possible to identify four types of biological discourse relating to the issues of holism, reductionism, and explanation in biology[25]. There were biologists who favoured a strong reductionist view (that is, a quasi-ontological reduction of living phenomena to bio-chemistry, proteins, and cellular matter). Five respondents did so. The second and third types seemed to be the two sides of the same coin. That is, they either emphasized the methodological importance of reductionism, or the theoretical power and comprehensiveness of holism. Eight respondents, for example, approved of scientific reductionism, insofar as reductionism was best suited as a method to carry out scientific proof, even though reductionist models only approximated reality. On the other hand, ten respondents leaned towards holism inasmuch as it seemed to be the best theoretical framework for the interpretation of evidence in experimental trials or in observations of nature. Finally, a fourth group of six respondents stressed that holism (and interactionism) was a valuable approach in its own right, especially suited for the study of evolution, ecology and some heterogeneous diseases. Those biologists were mainly interested in observational studies rather than in experimental work per se.

Indeed, very few respondents had a strong belief in theoretical reductionism. Only four respondents leaned towards this view, among them three molecular geneticists. Overall, our study indicated that biologists tended to favour methodological reductionism combined with some weaker or stronger belief in holism, depending on the subject matter of their research project. The reservations against reductionism, we would suggest, seem to stem from an interdisciplinary conflict, as it has been the scientific 'tradition' within biology itself, rather than from strictly external criticisms.

It seems therefore, that all biologists are not equally committed to strong and 'ruthless' reductionist views of biology, as some critics have contended. But then, what do these visions imply in terms of scientific truth and objectivity, and more specially with regard to explanations of complex behaviours? These questions seem more likely to highlight the stronger divergences between mainstream biologists on the one hand, and Radical Scientists and feminist biologists on the other.

A strong emphasis on holism may not be specific to biology alone, but it nevertheless entails a problem for objective knowledge in biology. As has been shown, biologists believe that reductionism is their best guarantee for correct and conclusive results. How do biologists reconcile their views about true knowledge and at the same time accept the thesis that reductionism is somewhat limited as a form of explanation which, therefore, must rely to an extent on holistic explanations and interpretations? Not so surprisingly, a majority of the biologists interviewed did not pay attention to this particular epistemological problem. Several of them, especially the younger ones, contended that the experimental method would achieve if not totally, at least partially, true knowledge.

The great majority of our respondents believed that the elimination of errors in biological interpretations was secured by the scientific method of a replication of experimental trials. The suggestion that biological knowledge could be built on a streak of wrong theories was rejected on those grounds. The possibility that ideologies or social biases could contaminate interpretations of biological evidence was eliminated, based on the same type of justification. Only "bad" biology, it was argued, could be contaminated by social ideologies or personal biases. Even among respondents who seemed to be the harshest critics of their discipline, this zoologist noted

> There are mistaken ideas about the role of hormones, let say, in personality changes of women. I suspect that a lot of the so-called evidence is actually folklore, and it will be shown to be wrong in due course. But what can I or what should I do as a biologist, as a feminist? All I can do is point it out, when I get hold of the data, that this is not a scientifically valid way of reasoning... The best thing I can do is to totally ignore it and let it die its death, which it will in due course... For instance, what do you think in sociobiology has been used to the detriment of women? Actually, I've never heard anyone serious say anything, based on sociobiology, which is to the detriment of women. (i16)

A few respondents, however, felt it necessary to explain their viewpoints. They acknowledged the limitations of experimentalism in the production of conclusive results, and isolated frequent uncertainties and disagreements in the interpretations of biological evidence. In that respect, a plant biochemist (i104) suggested that the Popperian stance on scientific progress was the most reasonable way of looking at biological knowledge. He stressed the necessity of having public exchanges in order to ascertain certain ambiguous theories. He endorsed the idea that scientific knowledge was only an approximation of the truth. But he also believed in falsificationism and realism.

> On the every day level... people contribute [to increase] the existing body of evidence... Some people won't believe that evidence, and they'll do more experiments and see what happens... In the end this will put the balance one way or the other. (i104)

Only one woman out of all the respondents reasoned on slightly different grounds. She claimed that, in the production of biological knowledge, the political authority of medical doctors might be much more crucial than biologists want to believe:

> You have to look at a much more conventional mode of classification [in order to appreciate fully the process of scientific production in biology]; of who's going to treat [biological defects] and be in charge. Take for instance the GP who will use very outdated methods of classification of normal/abnormal and refer patients to a certain number of people [to treat them], discarding many others. (i11)

Interestingly, this biologist was the only respondent also trained as a psychologist. Yet in spite of her strong reservations, she believed in the possibility of advancing knowledge by way of scientific research.

But overall, biologists appeared to believe that the scientific method can secure both objectivity and true knowledge. In this sense, biological theories are deemed exempt from social or gendered biases once they have undergone scientific trials successfully. On these accounts, several respondents took the example of the 'laws' developed in molecular biology to justify their belief in the objectivity and truth-value of biological theories. These were confirmations that biology was not doomed to remain forever a 'soft' science; that biology was indeed on its way to become a fully-fledged 'hard', objective science. One paleontologist summed it up by observing that although biology is often dealing with sophisticated objects and may sometimes wander into speculations, on the other hand it is also quite often about "nuts and bolts" (i106). Other respondents remarked, along these lines, that biology in fact comprises both research areas where findings are more likely to lend themselves to applied knowledge, and types of studies in which evidence may only give rise to speculative interpretations. Interestingly, three respondents, one, zoologist and two, ecologists, suggested that biologists had to be "more objective" than other scientists since biological research requires more intellectual discipline in the interpretation of results. Thus, the relative 'complexity' of living phenomena did not mitigate the belief that good biologists have the means to discern erroneous from valid interpretations of biological data.

The nature of biological objects, our informants said, might perhaps limit the range of lawlike explanations in biology compared to physics or chemistry. But this does not diminish the objective character of results and the validity of results obtained by means of the scientific method in biology. As an ecologist said,

> Maybe 'softer' is a pejorative term [to qualify biology]. It certainly is a different science [from physics or chemistry], because in biology there are no definite answers... When you do an experiment in biology, the normal thing is that you can eliminate the impossible, you cannot eliminate the possibles... In that sense it is a slightly unsatisfactory science to work in because you can never get a definitive answer, there can always be another experiment... I think [biologists] can mentally cope with maybes, possibilities; the subtleties of interpretation ...It's very hard to prove things in biology...There are no laws that really apply in biology [like they do in physics or chemistry]...Science tries to approach truth, but in biology we're maybe just a long way behind. (i12)

Finally, literally all our informants (in both pilot and main studies) claimed that it was not biologists who propagated spurious interpretations of biological data. It was more likely to be the politicians, the media and the public these inform, and even the medical practitioners who might distort scientists' claims. These remarks introduce us to our next section, which examines the scientific legitimacy of sociobiology as a model of explanations of behaviour and the controversies it has aroused.

On sociobiology and the explanation of human behaviour

Among the forty-five respondents in the pilot and main studies, very few were more than casually acquainted with the field of sociobiology or by the debate spurred by E.O. Wilson's book in the 1970s. Only two evolutionary geneticists, in addition to one paleontologist and two ecologists interested in animal behaviour, elaborated on the subject. These five respondents agreed that sociobiology was useful to biology inasmuch as it was restricted to the fields of animal behaviour and evolutionary genetics. As one of them pointed out, "Apart from birds and mammals, all animal behaviour is inherited" (i106).

This tends to show that if biologists accept sociobiology as a full-fledged scientific discipline, they nevertheless prefer to shy away from explanations of behaviour in humans and higher organisms. This also explains why, on the one hand, they may criticize severely Wilson's human sociobiology, and, on the other, defend his studies in insect behaviour. One of the ecologists (i18) can be quoted as expressing relatively well the views shared by her colleagues with respect to the scientific value of sociobiology. She maintained that "sociobiology is not bad as such"; rather, it has been "badly applied, especially by the media, and especially in relation to sexual differences". She added,

> [Sociobiology] is best taught within a department of biology [than a department of sociology] because it's related not just to humans. I mean all the example come from animal examples [sic] and then one builds on that to say, 'Well, maybe human societies have been moulded by the same sorts of pressures'... We teach [sociobiology] here, in a limited way, looking at different strategies, mating strategies, feeding strategies, behavioural ecology, how you behave with your neighbour as an animal, as a territorial or non-territorial animal, how it affects your defense strategies. But I think it has very limited interpretation as far as human populations are concerned.

The two evolutionary biologists were, however, more severe towards the sociobiology debate spurred by Wilson. They both held that this debate was a thing of the past, at least within the field of biology, for Wilson's thesis soon proved to be theoretically short-sighted and vacuous from a biological viewpoint. "It is surely oversimplistic; too polarized between arch-reductionist and anti-biologists" one of the ecologists contended (i105). Even more radical was his colleague:

> I think the whole field of sociobiology, of human sociobiology, is rotten at its core... What the critics of human sociobiology in the early days said has tended to be dead right. I mean [sociobiology] has been misused... This has nothing to do with ... animal sociobiology or what used to be called behavioural genetics ... which is a perfectly respectable science... I was amazed when [Wilson's] book on sociobiology came out, and in chapter 27, to see him carrying out what I call a pathetic fallacy, to draw human analogies from animal trials. (i107)

In addition to these five respondents, other informants argued that sociobiology and the biology of behaviour were "unfortunately" taken on by non-biologists who did not know much about genetics, physiology, or neurochemistry. This was, according to them, the most deplorable aspect of the debate concerning sociobiology and human behaviour. It is worth repeating here what one of the zoologists, otherwise very critical of her discipline, said about the sociobiology debate:

> What do you think in sociobiology has been used to the detriment of women? Actually, I've never heard anyone serious say anything, based on socio-biology, which is to the detriment of women. (i16)

Our results showed that biologists tended, to some degree to admit that biology, as in the case of sociobiology, could be "misused" for political purposes. Their defense however, was such as to let biological research remain as free as possible from political regulations, instead of augmenting the means of public control over it. This kind of reasoning was expressed more clearly in light of the opinions reported on Dawkins's theory of the "selfish gene". The argument developed generally runs as follows: The thesis of the selfish gene is scientifically too narrow, perhaps even erroneous; yet it is provocative and in that sense it is worthwhile; it does not explain human behaviour well, and perhaps does not aim at explaining human behaviour as such[26]; but it is true that in some instances, political groupings have used human sociobiological theses to attack minority groups; yet, in the end, biologists should not be blamed for the misuse, by politicians or the lay public, of some of their ideas. As this zoologist said:

> I think [Dawkins's] contribution has been one to stimulate discussion. And I think that's often the major contribution of a lot of controversial ideas ...And I think most of the reading public now also realize that ... there is enough discussion in the media, on the television, to show that he is not always unflawed... [He] cannot explain behaviour fully... But I don't know if he sets out to do that... He doesn't explain behaviour anymore than anybody else does. But he has got some ideas... But behaviour is so complicated... (i17)

Interestingly, only four respondents in our main study admitted to being concerned by the problematic relationship between expert knowledge and its diffusion throughout society. They argued that solutions to these questions would only find satisfactory answers in a careful analysis of the present conditions of biological research and of a dialogue between social groups concerned. They admitted that, at present, the structure congenial to such a dialogue was not in place. Biologists, they contended, ought to be socially accountable, but they are too busy doing research to be bothered. In fact, and surprisingly, our results suggested that, on the whole, biologists do not claim significant scientific interest in the question of human behaviour, even though personally, they might be interested by this question. In the main, respondents agreed that human behaviour and intelligence are primarily determined by social factors and that biology plays a necessary, yet only secondary, role in explanations of behaviour. Some suggested that genes might influence certain behaviours, but only in combination with other sociological factors. Finally, they clearly contended that animal models of behaviour in sociobiology were extremely limited as a yardstick for the study of human beings.

It is therefore reasonable to think that biologists are, in general, 'weak biological determinists' as far as human behaviour is concerned. This does not necessarily imply, on the other hand, that biologists recognize psychology or sociology as fully-fledged sciences. Several respondents retorted, that they were not willing to surrender to 'softer sciences' the entire jurisdiction over explanations of human behaviour and, perhaps more importantly, of mental 'defects'. On this matter, they refrained from positing any definite boundaries between biology, psychology, or sociology, regarding the explanation of human behaviour and clinical disorders. Having said that however, they did not seem inclined to engage in a serious dialogue on this matter with the 'softer' human sciences. In fact, they did not seem to consider that institutional disputes might bar the way to a full and equitable dialogue between the disciplines concerned with the study of human behaviours and 'disorders'.

Main criticisms of the institution of biology

There was consensus as to what constituted the main criticism of biology. It did not have to do with biological knowledge per se. It had more to do with the scientific organisation of biological research. In other terms, respondents readily answered that the main current problem in biology is institutional and political, and that it can be summed up in one word: the lack of money!

For nearly all our respondents, the present structure of research funding has brought about psychological stress, work pressures, excessive competition, and restraints on research. A great many respondents said that biologists tended to succumb to the pressures of grant applications in order to be funded. As a result, too much applied research was being proposed at the expense of fundamental research, and originality in research programmes. As one lecturer (i5) stated, the lack of funding "holds back fascinating areas to be explored". Three respondents even argued that the present short-sightedness of biological research projects would jeopardize the future of human beings on this planet. Finally a few respondents contended that research was too much oriented towards medical applications and that, in some instances, it was literally succumbing to popular whims. One of the harshest criticisms came from a clinical neurobiologist (i11). She said that what she found the most difficult to accept was the scientific 'prostitution' of biologists. She did not exempt herself from the criticism either:

> The level of dishonesty... You get grants by influencing people and it doesn't matter whether the claims you're making are true or not, or even

> whether they' re appropriate; that you know if you're going to say something you're going to get money. And it's a rat race... because if you don't [get money] you can't survive basically.

Indeed, she was not the only respondent to suggest this. Others agreed that in medical research especially, biologists are often tempted to mislead the public in order to collect public funds for their research.

In the same vein, some respondents felt that the biological research agenda lacked imagination; that research was now more than ever dictated by sponsors and funding institutions. In this connection, the "rat race" in certain fields of research was denounced. Other respondents noted a decrease in the self-criticism of biologists working 'against the clock'. They also deplored the fact that young biologists tended to overcrowd the fast moving sectors of biology, and to internalise the 'unhealthy' rationale of biologists competing one against another.

Several biologists believed that the quantity and the quality of biological research had diminished as a result of bad funding. Others argued that repeated grant applications which are being turned down were a waste of time, and likely to dangerously lower the motivations of good researchers; but these opinions were not shared by all the respondents. In fact, a few biologists said that a compression of research funds had forced biologists to be more efficient with resources and instruments, and to seek collaboration rather than duplicate research projects. As one senior lecturer said

> I think that some of the restrictions are a good idea, because in the sixties, so much money was flushed around to people with no accountability. We were dreadfully spoiled really. And I think there was a need to tighten up. But it's probably gone too far now. It's discouraged a lot of people from going into basic research. (i#6)

This biologist also argued however, like a few other female respondents, that the women would 'lose out' more than the men in these conditions. She said financial constraints were likely to jeopardize the careers of many women as they form the bulk of part-time researchers who do not have the time to write grant applications and to lobby for more money.

Finally and as discussed earlier, some respondents admitted that science could be misused because of financial pressures; but they did not think this could affect the validity of scientific results as such. Only three biologists agreed that ideology and biases, whether political, racial, or sexual, could infiltrate the research process. But none of these informants thought that these social biases would survive good scientific reasoning and testing.

At another level, respondents were asked to identify the most important goals of modern biology. The vast majority of answers suggested that biology could first and foremost contribute to human welfare by, for example, reducing the incidence of hunger and disease. Almost as many suggested that a better understanding of biological mechanisms, especially in genetics and in neurobiology, were high on their list. (Respondents did not necessarily suggest that the sub-discipline in which they were researching was more important than others.)

It might in fact be argued that, in the main, biologists consider their discipline as an instrumental form of knowledge. This opinion is certainly rooted in the wave of successes of applied genetics and biomedical research over the past decade, and as a response to the growing concern over ecological disasters and wastage of natural resources since the early 1970s. Generally, they distinguished between medically-related research and biological research per se, and between applied research (again in medicine, but also in agriculture and ecology) and 'the quest for pure knowledge'. Among the most conspicuous concerns reported, the ecological crisis stood out. As a good number of biologists believed, their science ought to increase its research effort towards this matter. It is noteworthy however that, with few exceptions, very few of the respondents questioned the role of political authorities in the success of ecológical policies and agricultural reform. Only one respondent raised a totally different issue. She pressed for an integration of biological knowledge which, she said, was becoming a huge mass of information lacking in unity (i16).

To summarize, respondents considered that biology was instrumental in the arenas of medicine, agriculture and ecology, and that it should engage in contributing to human welfare; yet they also believed that biology should be committed to researching fundamental questions, in genetics, neurobiology, or developmental biology, and also to the 'quest for pure knowledge' like the understanding of species differentiation and the mechanisms of evolution; finally, with respect to studies of human behaviour proper, very few respondents thought biology could undertake a fully-fledged research programme, at least at present.

Feminism in Biology

This chapter concludes with the results which directly concern the issue of feminism in biology. The data collected in the pilot and main studies suggested that the notion of feminism in biology has found some echo in the field of biology, but that, unsurprisingly, it does not transcend the idea of institutional reforms for equal opportunities. Women respondents had various opinions about how feminism ought to contribute to biological science and what it might encourage with respect to professional advancement. Their opinions ranged from a strict instrumental role of feminism (in terms of lobbying for equal opportunities), to a total re-evaluation of the institutional rules and cultural norms relating to a full integration of women scientists. We shall see below that, in comparison, male biologists have a different -- if not 'indifferent' -- stand on these questions. But neither men nor women, excepting isolated individuals, expressed the view that the role of feminism could also comprise changes in the methodology of biology or in the type of results being produced.

The closest views to the idea of a 'feminist biology' were those of women who suggested that, in genetics namely, women's style of practice, skills and research interests, might help to renew the battery of research questions being asked and, perhaps also, might marshall significantly new evidence. This position conforms with one of the versions of the 'feminine values' problematic developed in the feminist theory of knowledge. Among the thirty-two women interviewed (in both pilot and main studies), eleven respondents agreed that women, especially in medically-related sectors, could possibly appropriate different research orientations than those traditionally followed by men. But this presumption was mitigated by several factors: first, it was suggested that childless women may share less with women who have children for certain research questions than might the men having children themselves. Secondly, that the institution or sponsor which employs one restricts one's freedom to decide on a research agenda. Thirdly, that no sociological evidence seems to vindicate this presumption. Indeed, respondents, male and female, generally believed that inter-individual differences, more than gender, could explain variations in terms of research interests or levels of 'biological imagination'. We shall return to this later.

Who are the feminist biologists?

A way to begin examining biologists' conceptions of the impact of feminism in biology is to assess whether they know of, or identify with, feminist biologists. Male and female respondents were asked if they had worked with, or knew, 'feminist biologists' and to assess how their own views on feminism might have been reflected in their work.

In general, respondents indicated they did not know any feminist biologists, but that they had feminist friends or acquaintances outside of science. In the main, women respondents who knew feminists, inside or outside biology, mentioned that those people were, in general, politically active, and relatively more assertive than themselves or the average biologists. For instance, this middle-aged neurobiologist said

> Well, at one point I think I would have said that [I am a feminist]. But the word implies something active to me. I guess I don't do anything very active. (i2)

Similarly, this microbiologist replied,

> No, not really.... because I don't actually belong to any organizations. I don't believe that some of the things feminists do are right... I believe that women should have equal opportunities, but I don't think we have them at all at the moment. (i5)

On the other hand, the male respondents were prone to refer readily to some of the women biologists they knew in terms of: "strong characters", "tough women" , "assertive", or even as "aggressive with men" and "hostile to men". They also tended to readily identify these women as feminists. For instance, a postdoctoral student, argued that in comparison with a woman of his department he would likely identify as a feminist, he would not make "a fuss [like] that woman when things go wrong between men and women" (i103). In contrast to this however, another male student of the same department indicated that "she is more assertive than other women, but not aggressive" (i109).

In sum, women respondents linked feminism more immediately with political action than with attitudes, while men generally based their judgments of feminism on the latter alone. In slight contrast, only a few suggested that some women biologists they knew were "feminist in a way" or else "probably not feminists because too individualistic". These remarks are more similar to the qualifications generally given by women respondents.

Surprisingly enough, the only female respondent who was designated by her colleagues as a feminist declared that she was herself "a failed feminist", for feminism was "not the cause closest to her heart" (i16). Several of her male colleagues in contrast referred to her as a rather strong feminist. It is also noteworthy that among the female respondents of the same department, the opinions about her diverged from those of men: the women did not necessarily think she was overly aggressive.

Our results suggest that for women biologists, the identification with feminism necessitates some political qualifications, and this seems to make allowances for whether one considers herself a feminist or not. For the men, in comparison, it appeared more important to show some degree of approval for diverse feminist causes, like childcare provisions and equal opportunities at work. In fact, only two men (i107, i110) rebutted feminism decidedly. (One found some feminists "painfully boring", the other "overly hostile to men" at times; they both were politically more committed to the socialist cause, as they emphasized.)

A good number of women reacted in the same way as the men, that is by defending themselves as being feminists in their attitudes towards women and towards men. Roughly one third of the eighteen female respondents in the main study declared themselves feminists, qualifying their answers as follows. One middle-aged woman answered that she was "yes, definitely" a feminist, because "I do not make any difference between male and female colleagues or students. I judge them on their scientific merits, not on their sex" (i7). This type of qualification was replicated in two other interviews, which tends to show that the notion of feminism had a rather superficial connotation in the minds of some women respondents. That is to say, feminism had, in their opinion, a rather limited scope, implying simply that one does not, nor should be, prejudiced against women in science. In other instances, respondents identified themselves as feminists but for different reasons. A young doctoral student indicated that she was "feminist, but very selfishly". She said she would not fight for feminism herself but she appreciated the professional dividends it had provided her and other women scientists (i10). Likewise, another post-doctoral researcher said she was feminist but not as active or as assertive as other biologists she knew (i8). Finally, a tenured researcher said that she "supposed" she was a feminist but "not an extreme feminist" pointing to the fact that, compared to other women, she has always realized belatedly the extent to which men were "chauvinistic" (i14).

In contrast to these respondents, seven women did not consider themselves feminist in the first instance. Interestingly enough, all of them seemed shrewdly aware and concerned by discriminatory practices and sexist attitudes against women in their milieu. But they tended to consider their own positions as fairly conservative and in no way combative. All these respondents were in their thirties or mid-forties. One respondent, for instance, felt that she was not feminist but definitely "gender aware" (i1). Other women said that they possibly leaned towards feminism but were either "not militant" (i2) , or "not [feminist] from a political point of view" (i4), or did "not belong to any [feminist] organizations" (i5). Yet all of these respondents made a strong point for the re-enforcing of 'equal opportunities' programmes. A fifth respondent felt that she was not feminist "because I only think about [feminism] in particular instances [when I] get really cross [about men]" (i15). Finally, two other respondents (i6, i12) thought that because they tended to 'give in', sometimes, to male patronizing, and to 'play the game' of the ingenuous female, they had never considered themselves as feminists. 'Real' feminists would not consider them as such anyway, they thought. Among these seven women, four said that they usually overtly manifested their disapproval of sexist attitudes at work and often retaliated to sexist behaviours de facto. Finally, and interestingly enough, two of these women mentioned having participated in feminist meetings in the past.

The two oldest female respondents said they resented "the frills of feminism" and anti-men attitudes. But this position towards feminism in biology was not characteristic of the oldest women alone; it was also shared by many of the younger female respondents.

Among the men, apart from two respondents who showed outright hostility to feminists, the majority declared that they were sympathetic to feminist causes. But in the main, this only reflected their approval of equal opportunities reforms and their empathy with female colleagues who had children. In this connection, three men said that inasmuch as they were for equal opportunities, they would see themselves as favouring feminism. With regard to the position of the seven married men who 'supposed they were' feminists, the following quotations are illustrative. As one of them replied (i108), "I am rather supportive to women with children... because my wife had her Ph.D. studying part-time". A second respondent (i101) felt strongly "that women should not be forced to choose between a career and a family...". Finally, a third male respondent said, "I think I am probably not a 'good' feminist, but I think I am [feminist]... in the sense that I am more aware of problems [confronting women] and prepared to discuss them" (i106). Finally a fourth respondent observed that he was "not really [a feminist]" but that, on second thought, perhaps he was in the sense that "I try to set up a non-discriminatory atmosphere [in the laboratory] and help women overcome their fears and other career barriers" (i104). Only one male informant (i102) indicated that he used to have strong positive opinions about feminism, but that he no longer agreed with feminists because their discourse had gone "way out". (This man was extremely annoyed by the kind of propaganda about, as he put it, "feminist classes of mathematics" or "physics classes for housewives".)

In sum, female and male respondents generally pointed out that they were opposed to 'strong' feminist views, with the "frills" and the "anti-men" connotations, but that they also were in favour of equal opportunities. In that latter respect, women generally tended to think, in contrast to men, that a lot of reforms still needed to be carried out before they could have equal opportunities in scientific careers. This is congruent with the fact that women also believed that even though discrimination was almost totally eradicated from the scientific milieu, on the one hand the domestic division of labour was still having adverse effects on the careers of women scientists, and on the other hand, that sexist attitudes were still making it difficult for women to become fully integrated in science. Regarding the idea of feminism in biology, differences among the female respondents' views was mainly a matter of degree in attitudes and behaviours towards sexism at work, but it also depended on whether they felt they had to make stronger political statements in favour of feminism at work.

In what ways has feminism contributed to biology?

On the whole, women and men respondents had different views as to the contribution of feminism to the improvement of the conditions of women in science. Informants thought generally that feminism has played a major role in the evolution of attitudes about the roles of men and women and in helping to increase the participation of women in science. Science education and training, the sharing of domestic responsibilities, and the struggles for more creches were examples advanced to illustrate the positive impact of feminism in science. Only two women respondents stated that feminism had not significantly changed the situation of women in science, not even to a minor extent. A few others reiterated the view that more remained to be done in order to encourage women to enter science, not least importantly, to change men's attitudes towards women scientists.

But where did respondents stand with regard to the issue of feminism as a critique of biological research and biological theories as such? Results show that most respondents were not familiar with such a critique, or else had never heard of it. This minimalised the scope of our discussion on this matter. On the other hand, it revealed the extent to which mainstream biologists are oblivious of the works of feminists in biology. In spite of this, it is important to see how those who are acquainted with these works have rated their scientific quality. It might also be useful to examine the general opinion of those who have not heard of these works with regard to the possibility for a project of feminist biology to be developed. This should also illustrate how the scientific disagreements and political resistance of biologists towards feminism are articulated.

Only two out of the eighteen female respondents in the main study mentioned having read some of the literature on feminist biology. None of them however, has been convinced by the arguments advanced in the literature they read. One of the two (i16), otherwise extremely critical of the sexism prevailing in the scientific institution, observed a chasm between the feminist discourse about biology and the reality of biological research: that feminists do not know much about biology, and are in fact criticising the popular press' interpretation of biology. She said this was very unfortunate for it undercut the very cause feminists should be defending.

> I have read some [of these writings on feminist biology]. There's a book on my shelf, the name of which I can't remember... which is something about women and biology, and it's the biggest load of nonsense imaginable. The reasons for that is I think... women who don't know much about biology are criticizing the popular press' version of biology. And it's an embarrassment as a biologist and as a woman. So, what do I do?... I don't know these people I'm glad to say. If I was speaking with them, then I would be able

> to say to you very explicitly what I object to... But as far as I'm concerned, they are people who produce a bunch of nonsense. It's such an absolute nonsense that no one bothers to argue with it. It's journalism of the Sun standard. I do know something about genetics, and I do know something about sociobiology, and they simply get it wrong. They get the theory wrong. It's like putting up something to knock it down. You know, they're not doing the womens' cause any good with this kind of attitude. For Heaven's sake, if you want to do it, do it well! (i16)

The other respondent (i13), stressing that she had read a feminist critique of medical knowledge, said that she "partly agreed" with it, and that it was "interesting because it certainly has some truth in it... but sometimes it goes too far". She specified, however, that as far as biology is concerned, she "cannot imagine a biologist whose feminist views would be reflected in his or her work... It simply does not arise".

The other sixteen respondents were "not familiar" with, or else totally oblivious to such literature. Some of them however, mentioned having read some feminist literature. But it was generally the classics of feminism (e.g., de Beauvoir, Greer, Friedan) or works about women in science; nothing relating to the critiques or projects of biology put forward by feminists. Among the thirteen male informants, only two young biologists admitted to having read 'best-sellers' presenting the feminist critiques of primatology and biology. They thought that the quality of these books was like that of other similar best-sellers, no better, no worse. A third respondent said his attention was once drawn to an article in Science about women in academic research, and that he found it informative. In sum, male biologists seem, if not more, at least no less, oblivious than their female counterparts to the feminist literature on biology.

In spite of their lack of acquaintance with feminist works on biology, some respondents had their opinions about the relevance of a project of feminist biology. Their answers varied from a total rejection, to dubiousness. No one believed that such a project was sustainable scientifically.

Our respondents were therefore asked instead, to explore the idea that women, because of their particular skills and "caring attitude", might, to some extent, possibly transform the practice of biology. One third of the women respondents in both pilot and main studies mentioned that it was reasonable to think that women might wish to orient biological research in different directions, more particularly in the areas of clinical research and genetically-transmitted diseases. But two of them (i#4, i#5) also argued that their male colleagues could certainly do likewise. On the other hand, the majority of these respondents indicated that the evidence of such a transformation had not yet occurred, at least in their research sectors. Indeed, several qualified their opinions indicating that current financial constraints made it difficult for individual biologists to research topics they would personally choose. This suggests that the institutional opportunities for women to research on 'new' biological questions is rather tenuous. Finally, two respondents (i#10, i2), one in parasitology, the other in development physiology, contended that, apart from the areas where the objects being researched have to do with gender, it is unlikely that women could contribute to biology very differently from the men.

Not too surprisingly, there was only one respondent (i#9) out of the thirty-two females in both pilot and main studies who sympathised with the idea of a feminist biology as such. But her rationale for doing so was utopian, strongly motivated by her political opinions rather than by a sound knowledge of the feminist literature on the subject. In fact, and rather surprisingly, she would identify, scientifically speaking, with the group of strong biological reductionists. That she was a biochemist probably explains why she maintained such a position on biological reductionism while simultaneously holding a favourable opinion of feminist biology. (In sharp contrast to this respondent, one woman biologist (i7) was very hostile to the idea of feminist biology: "non sense, non sensible idea: how could it be?", she retorted forcefully and without any further qualifications.)

To sum up, men and women respondents were oblivious of the feminist literature about biology, or else they seriously questioned the possibility that feminism (or any political movement for that matter) could give rise to a new type of biology. The only form of 'feminism' with which they tended to agree, and women more than men would think so, was the possibility that women might develop different research questions based on their different social interests and life conditions as mothers or simply as women[27]. Hence the opinions of mainstream biologists towards the idea of feminism in biology diverge quite drastically from the positions of feminist critics of biology. The only point of convergence, it seemed, lay in several female respondents suggesting that women might wish to orient the research questions in some areas of biology differently from how they are being addressed at present. These research areas were mainly related to medical and health studies, or, as it was also suggested, addressing questions specifically related to sex and gender. These results should be particularly enlightening for the sociological analysis of the actual scientific production of some feminist biologists.

Conclusion

We have discussed the stance of 'mainstream biologists' relative to some scientific issues addressed by feminist critics and Radical Scientists. We have suggested, based on the results of interviews with forty-five male and female British biologists working in mainstream research institutions, that 'the biological ethos' of 'mainstream biologists' is not a homogeneous set of beliefs or perceived norms of practice. That is to say, biological reductionism is not, as critics of biology have alleged, the kind of strict research approach on the basis of which biological research seems to be carried out, least of all with regard to human behaviour studies.

From an organizational point of view, however, it is reasonable to think, as critics have argued, that biological research is dominated at the moment by medical genetics, molecular biology, and biochemistry, and that these biological disciplines are more markedly tainted by a reductionist research approach. But these constraints were generally acknowledged by the biologists interviewed in the study, which, again, minimalises the critiques made towards them by feminist and Radical Scientists.

Our results also indicated that the holistic approach is more potent in botany and ecology than in other areas of biology. That is to say, botanists and ecologists would generally agree that environmental factors play a great role in the evolution and development of living forms. There were also some significant signs to the effect that biologists involved in human biology and in medical research more particularly, were aware of the possible interactions between environmental and biological factors in human diseases and defects. Our results suggested that, if among these biologists, some tended to favour reductionism, while others tended to be more open to an interdisciplinary approach, this plainly reflected the traditional paradigmatic divide within biological science between the approaches of strict biological determinism on the one hand, and holism on the other. As illustrated in our study, as far as human behaviour is concerned, biologists estimate that psycho-sociological factors may be important aspects in a clinical diagnosis, but that these are so volatile and hard to grasp that it is preferable to put them 'in brackets' and to focus rather on the molecular, genetic, or physiological reactions of certain well defined biological 'defects' and 'disorders'. Biologists thus prefer to concentrate on more tangible components of diseases like organic 'markers'. Hence, although in principle biologists agree that psycho-sociological factors affect the release of certain diseases (and very importantly in some cases), in their actual research work, these factors are usually controlled, but never closely examined. The institutional barriers between disciplines are, thereby, strictly safeguarded. And this, it seems, is the locus at which mainstream biologists and their critics usually diverge. The latter precisely aim at opening up the disciplinary barriers between 'hard' and 'soft' sciences, and between instrumental knowledge and socio-critical studies.

At another level of analysis, our results suggested that there were no great differences between the points of view of men and women on their discipline, with the exception of 'professional matters' and equal opportunities. But these differences could be easily compared to the usual worries of other women professionals.

Finally, and again with few exceptions, there were no major differences between the viewpoints of men and women regarding the impact of feminism on the methods and aims of biological research. In this connection, the integration of women in biological practice does not assure, even with the further improvement of their work conditions, that women might change the way biological research is currently being carried out. Very few women biologists indeed believed that women might change the way biology is currently being practised. If so it was more likely to occur in medical and health studies, or in areas in which subject matters relate specifically to sex and gender. Even the few respondents who seemed more politically-oriented than the others, remained far from convinced by the arguments of feminist critics regarding the idea of a project of feminist biology.

This leads us to the last two chapters of this thesis in which we compare the foregoing results with the actual scientific production of feminist biologists.

Endnotes

1) Certain controversy-laden books bear the imprint of a strong socio-political stance on scientific matters in the life and clinical sciences, such as I. Illich, La Convivialité (Paris: Le Seuil, 1973); and L. Kass, Towards a More Natural Science: Biology and Human Affairs (New York: The Free Press, 1985).

2) Jenkins (1979): table 5.18, p. 195.

3) Equal Opportunities Commission. 1987. Facts ... that figure in equal opportunities and education. Manchester.

4) I have used only some categories of disciplines presented in annual reports of the University Grants Committee Statistics of Education: biology, botany, zoology, physiology, anatomy, biochemistry, and other combinations of biology with mathematics, physics or chemistry subjects.

5) University Grants Committee. Statistics of Education 1971 (Vol. 6. Universities), University Statistics, 1980 (Vol. 1) and University Statistics, 1986-87 (Vol. 1). Rossiter (1982) gives the following figures for the United States. Between 1920 and 1938, 19.9% of doctorates in botany were awarded to women, and 15.8% in zoology. In comparison, women received 4.7% of doctorates in physics and 8% in chemistry. In 1938, roughly 3% of the jobs of physicists and chemists were held by women, compared to 10% of the jobs of botanists and zoologists, and 13% of the jobs in microbiology and biochemistry. During the 1940s and 1950s, Rossiter notes a drop. According to the National Research Council

(1983), the pre-war levels were attained in the 1960s, and increased sharply in the 1970s. In the 1980s, 25.2% of doctorates and 42.1% of the bachelor degrees in the life sciences were awarded to women, compared to 12.2% and 23.7% in the physical sciences (including chemistry). See United States. National Academy of Sciences. National Research Council. 1983. Climbing the Ladder. An Update on the Status of Doctoral Women Scientists and Engineers. Washington, D.C.: National Academy Press.

6) University Grants Committee. Statistics of Education 1966 (Vol. 6), University Statistics 1980 (Vol. 1) and University Statistics 1986-87 (Vol. 1).

7) For instance, the figures obtained through the University College Annual Report of 1983-84 shows that the first woman appointed professor in this college was Kathleen Lonsdale in 1949. Women were first appointed lecturers -- or its equivalent -- as of 1898. Out of the first twelve, six were in one department: botany. In 1983-84, there were only six women professors at University College. The college has had a convener for the Academic Women's Achievement Group (AWAG) since 1979. AWAG publishes short reports (on a non-regular basis) in the University College of London Bulletin, analyzing the under-representation of women in the academic structure and reporting on remedies such as provision for daycare centres.

8) The US National Research Council (1983) also points to the higher proportion of women than men pursuing post doctoral research instead of going up for upwardly mobile career in academia or industry in the United States.

9) Widnall, Sheila. 1988. "AAAS Presidential Lecture: Voices from the Pipeline". In Science, 241 (30 September): 1740- 45.

10) Bearing successively the names of Journal of the Institute of Biology (1954-1960), Institute of Biology Journal (1960-1969), and The Biologist (1969-).

11) Especially the debate regarding the genetic localization of schizophrenia in the prestigious journal Nature, vol. 336 (10 November 1988). See also R. McKie. 1988. The Genetic Gigsaw. Oxford: Oxford University Press.

12) See, for instance, the debates related to the publication of the Warnock Report of 1984, in Spallone, P. 1986. "The Warnock Report: The Politics of Reproductive Technology" in Women's Studies International Forum, vol. 9.: 543-550; and those about recent debates concerning the implementation of reproductive technologies and projects of legislation on research on embryos and reproduction, in McNeil. M., Varcoe, I. and Yearley, S., ed. 1990 The New Reproductive Technologies. London: MacMillan.

13) See Goodfield, June. 1977. Playing God. Genetic Engineering and the Manipulation of Life. New York: Harper Colophon; the essays of J. Rifkin and G. Allen in Arditti, R., P. Brennan, S. Cavrak, ed. 1980. Science and Liberation. Montreal: Black Rose; and Wright, Susan. 1986. "Molecular Biology or Molecular Politics? The Production of Scientific Consensus on the Hazards of Recombinant DNA technology". In Social Studies of Science 16 (November): 593-620. Also R. McKie. op. cit.

14) G. L. Lewis propounds the idea that molecular biology has reached a paradigmatic status which physics alone used to claim in "The Relationship of Conceptual Development to Consensus: An Exploratory Analysis of Three Subfields". In Social Studies of Science 10, 3 (August): 285-308.

15) There were 24 negative replies (and non-replies) out of 55 requests.

16) Three informants were actually seen outside of this period. They were in zoology, and it was considered important to include them in our sample since they represented a disciplinary variable relevant to our study, especially with respect to the issue of sociobiology.

17) It was difficult to reach the disciplinary representation on the basis of the names of departments alone. For instance, one may find several experimentalists in a zoology department (e.g., insect physiology, genetics of development) and inversely, several theoreticians and fieldworkers in a department of genetics (e.g., population genetics). As a matter of fact, zoology and botany have become biological subjects heavily encroached by biochemistry, molecular biology, and genetics. But they do retain important aspects of their heritage as natural history subjects. In spite of these difficulties, the disciplinary representation obtained ex post facto was satisfactory -- based on the range and variability of responses collected in the interviews. Overall the sample includes nine biologists in human genetics or pathology; eight in botany (physiology, genetics, microbiology, or ecology); eleven in zoology (physiology, development, genetics, neurology); one paleontologist; two population geneticists; and two others unclassified doing research in biochemistry and genetics.

18) The interviews took an average of one hour and a half.

19) All the interviews were transcribed non-verbatim. Transcripts were analyzed and parts were selected to be transcribed verbatim for purpose of illustration in the text. All the interviews were transcribed by the author and an assistant.

20) As mentioned previously in this chapter, we analyzed the survey material in a way that bears similarities with quantitative analysis, in the sense that we aimed at identifying 'patterns of response' rather than 'contextualized meanings' stricto sensu. Quotes were therefore selected according primarily to their illustrative value as patterns of response or as occurrences of similar views of respondents on a given subject. Having said that however, we were able to discern where responses were 'more singular' and 'personal' by means of the additional 'contextual' information marshalled during each interview. In these cases, we made sure to signal that the 'illustrative value' needed to be qualified in terms of its 'personal' as opposed to its 'generalizable' content. In the rest of this chapter therefore, the numbers in parentheses following the signs '#i' or 'i' refer to the interviews classified in the Appendix. The interviews of the pilot studies bear the sign '#i' before their number; those of the main study, the sign 'i'.

21) This presumption may actually be discarded on empirical grounds: it has been repudiated, for example, in the study of Homans, H. 1989. Women in the National Health Service. HMSO (Equal Opportunities Commission Research Series).

22) See the study of Dex (1988) in this regard.

23) There is an historical case in point which illustrates this: the 'technical' role played by Rosalind Franklin in the discovery of the structure of DNA. See Olby (1974), Sayre (1975), and Watson (1968).

24) These twelve characteristics are: 1) critical; 2) imaginative/innovative; 3) available to students/colleagues; 4) theoretically skilled; 5) skilled for practicals, experimentation, or observation; 6) meticulous, patient; 7) hard working, enduring; 8) single-minded; 9) open to criticism; 10) easygoing; 11) confident; 12) other (to be specified by the interviewee).

25) According to this tentative typology, the same informant may be included in different type-categories. The point of constructing such a typology is to illustrate the variations in the opinions of biologists about scientific knowledge and biological explanations.

26) As a matter of fact, Dawkins really attempted to emulate Wilson's book in his own way. The last chapters of The Selfish Gene discussed "memes", the cultural equivalent of "genes" in the human realm.

27) Interestingly, one male informant (i107) commented that his female colleagues were "obsessed by sex-related studies... although not in a sordid way".

CHAPTER 7

A STUDY OF FEMINIST PRACTICE IN BIOLOGY I: THE CASE OF LYNDA BIRKE, FEMINIST, SOCIALIST AND ZOOLOGIST

In this first case study of feminist biologists, we examine the works of Lynda Birke, a British feminist biologist. The choice of conducting a study of Birke's biological practice was guided by two motivations. First, Birke is one of the very few British feminists who is still practising science[1]; secondly, her feminism has extended beyond the notion of a critique of the organization of science (and the integration problems of women) into a reflection about biological knowledge, method and approach.

Birke has been engaged for roughly two decades in research on animal behaviour, more especially the influence of sex hormones and mother-offspring interactions on the development of sexual and behavioural differentiation. Early on in her career, she became critical of the scientific control over our daily lives, especially of biology and medicine on womens' lives, and on our commonsense beliefs; this led her to outgrow the norms of professional orthodoxy.

Birke is critical of biology in two ways. First, she maintains that biology, like any other science, is a structure enmeshed in capitalist and patriarchal social relations, and is therefore bound to defend and support the interests of dominant political and economic groups. As such, biology has a hidden politically-laden research agenda: it serves the interests of industry (military, pharmaceutical, or agro-chemical) before those of most needy people. It also helps to perpetuate the established social order by sustaining a rationale of oppression, as in, for instance, the policing of delinquents and the medical monitoring of women, homosexuals, and immigrants. Secondly, biology has progressively enhanced the maintenance of social dogmas such as competition, individualism, and male superiority through its own scientific research program, biological reductionism. Despite its methodological merits, Birke contends that biological reductionism is a flawed model of explanation of life forms, biological phenomena, and patterns of behaviour. In this connection, she has always openly denounced the ivory-tower attitudes of scientists, and has developed a more systematically severe critique of biological reductionism. This led her to undertake (in the last few years) the elaboration of a "successor science", of a "progressive science" informed by feminist and ecological movements, the principles of which residing in the notions of cooperation, equality, and holism at the levels of both the organisation of science and the approach to biology.

Through an investigation of Birke's research work in animal behaviour, and reflection on the biology of 'female' health disorders and the project of a feminist biology[2], we shall argue, however, that Birke does not succeed in laying out the grounds for a new biological methodology (and epistemology) even though she claims to do so. She does, on the other hand, propose new lines of research in zoology and in human biology, discussing established biological evidence with an original set of conceptual tools and, also, producing new biological evidence.

In the last analysis therefore, she does not build 'her science' on new epistemological rules of validation (even though both her sociological views on the structure of science and her theoretical insight into a topic like the development of sex and gender invited her to do so). The epistemology of critical realism and falsificationism still looms large in her scientific work. Her rejection of biological determinism in favour of holism and material dialectics does not imply that the new biological approach she is advancing must build on entirely new rules of validation, as some feminists critics of science have argued, as an examination of her research work in zoology will show. Finally, although her project of a feminist human biology however seems original conceptually, she has not, as yet, provided ample evidence of how it could be fully realized.

Birke has often stated that in its present form the project of a 'feminist science' seems utopian and practically unmanageable. (See especially the last chapter of her book of 1986 entitled 'Towards a Feminist Science'.) I would suggest however that the pre-conditions she poses for her project to be achieved need not be so stringent. For her project of biology could be realized conditional to renouncing <u>only some</u> of the conventions (institutional and methodological) of biological practice, as the case study in this chapter will show.

<u>A Biologist Inspired by Social Movements</u>

Lynda Birke's involvement in several social movements over the past fifteen years has greatly influenced her work as a biologist. As she recalls, thinking about being feminist and biologist at the same time has been on her mind for no less than fifteen years. "Feminism came along", she says. "I was not [a feminist] as an undergraduate though... I suppose I was a biologist before I became a feminist"[3]. Birke also considers herself a socialist. Indeed, she is a militant in a general sense and her participation in diverse social movements is reflected in her work as a scientific researcher.

She has, since her graduate studies at the turn of the seventies, allied her activities as a militant with her academic work. She has led teaching and research activities in school science, at the RSPCA, at Sussex University (in biology proper and in the sociology of science as well) and at the Open University successively over the past two decades. She has also participated in the politics of liberation movements in the 1970s, collaborated with the (late) Brighton Women and Science Group at the turn of the eighties, was a member of the short-lived Dialectics of Biology Group in the early eighties, has contributed to the leftist periodicals <u>New Socialist</u> and <u>Science for People</u>, and is now concentrating on teaching, writing and collaborating with feminist groups.

Regarding her research work more specifically, she undertook a Ph.D. in animal behaviour, with special reference to 'female' [4] hormones, at Sussex University in the early seventies. She subsequently focused on women's health subjects, directly inspired by her doctoral research on hormones and behaviour. This motivated her to teach womens' health courses, especially the physiology of menstrual cycle (which she was still teaching in 1989), and to write popular books in order to give lay women access to expert knowledge in biology[5].

It was also during the seventies that Birke recognized the dearth of research on female animals. She became acutely aware that the bulk of experiments had been performed exclusively on male animals and the results thereby obtained had been universalized to include both sexes. This confirmed her views that biology had a hidden research agenda crippled by androcentrism. In retrospect however, she thinks it did not make her change her approach drastically enough to animal experimentation. She believes that she ought to take more stringent positions on prohibiting certain types of experimentation on animals, on questioning the use of animal models in studies of human behaviour, or simply in defending animal rights. These issues, she argues, should be considered central in projects of feminist science the principles of which including primarily, as indicated earlier, cooperation, equality, and a sense of unity ('holism') between human beings and nature. We shall discuss these matters later on in the chapter.

At the turn of the eighties, Birke became involved in the Brighton Women and Science Group which published a widely quoted book, <u>Alice through the Microscope</u>, with the subtitle of 'The Power of Science over Women's Lives' (1980). In this collection of essays, she contributed two. One is about the "tyranny of the womb" in which she unveils the myths and taboos underwriting the current understanding of the female hormonal cycle. The other discusses lesbianism where she uncovers the prejudices that doctors and biologists have inherited from their society vis-à-vis homosexuality which are displayed right into their professional and scientific practice[6].

Her arrival at the Open University in the early 1980s, where she taught and researched in the biology department until 1989, was to be conducive to her changing even more drastically her approach to biological research. At first, she continued working on the role of hormones on behaviour on the basis of a distinction between a biology of sex differences and a sociology of gender differences. But her further involvement in the Dialectics of Biology Group increasingly led her to shift from a dualistic to a dialectical approach to the role of hormones and of social learning in sex/gender differentiation.

Birke has endeavoured to apply dialectics in her experimental work with animals, and expounded her results in scientific journals (Birke and Sadler 1983, 1984, 1985; Birke, Holzhausen, Murphy and Sadler 1984; Holzhausen, Murphy and Birke 1984). She has also tried to elaborate a model for the study of the female cycle from the perspective of holism and dialectics (Birke and Best 1980b; Birke 1984a, 1986; Birke and Vines 1987), and has contributed to several feminist journals in that connection. She has collaborated, as an editor or as an author, in several collections of critical essays on biological determinism in both animal and human biology of behaviour (Birke and Archer 1983; Birke and Silvertown 1984; Rose 1982a). We shall examine her attempts to build a holistic and dialectical biology in the two fields of animal and human biology of behaviour in the next two sections.

But how has she developed her position towards the project of a 'feminist biology' more specifically? Birke has addressed the question of feminist science more thoroughly in recent years in her book Women, Feminism and Biology in which she "explore[s] some of the ways in which biology is relevant to feminism ... and attempts to pull together some of the ideas that have been expressed about creating a 'feminist science'" (1986, vii). In an interview with the author[7], she was asked to define what 'feminist science' is precisely about:

> My belief in what feminist science is ... is difficult to define... one thing that worries me a lot is the extent to which we actually call things feminist that actually are perhaps more progressive ways to look at the natural world, that are actually not necessarily the prerogative of women or of feminists. And I do know a number of people of both genders who want to do more non-reductionist science. And I do see this as an important part of feminist biology. And it is a search for that that began to inform the research I did

over the last few years. It came out of my feminism and it has fed back my feminism at the level of theory, my publishing in women's studies journals and my book, at both sides of that dialectic.

Birke generally agrees with Sandra Harding's definition of a 'successor science', of a science that will replace the kind of science we have now and will integrate rather than separate[8] different ways of looking at the world[9]. Four main features of a feminist successor science seem to emerge from her writings on the subject. First, a feminist science must be informed by the values of ecology and feminism as social movements; secondly, it must change the methods, subject matters and structure of decision-making in science; thirdly, it might only be vindicated in a feminist and egalitarian society; and, fourthly, as such it will need more than mere ideological reforms, and feminist scientists will probably be forced to embark on political struggles. As a member of the Brighton Women and Science Group, she defended the view that

> We need to understand how biological theories contribute to our position in society and how supposedly liberating technological advances can in fact catch us in a trap... But we also need it [biological knowledge] so that we might build a better, and more humane, society. (1980, 13)

Moreover, she argued that only a holistic and dialectical approach to biological questions could convey proper understanding of nature and human biology, and adequate solutions to bio-behavioural problems. For her, the biological questions related to deviant behaviour, physiological ailments, or gender differences should only be understood as the results of multi-factor interactions between organic matter and lived experience mediated through cultural learning, where the whole dynamic of organic and behavioural components is expected to change through time (1986). Similarly, organic processes involving organs, organelles or organ systems should only be described in relation to their environment, in terms of a whole in which the dynamics of components is expected to change (Birke 1986; Birke and Vines 1987).

This new approach, Birke suggests, adumbrates the recent shift in left-wing feminism from a view entirely based on social constructs, to a view which links biological sex determinants with social gender roles dialectically, both mutually influencing each other. But this shift is still in the making, and feminists are still addressing the biological question at the theoretical level alone, rather than at a more practical one. Thus, as a feminist biologist in her own right, she contends, she embarked on the project of 'realizing' the idea of feminist biology.

Birke's Critique of, and Works in Zoology

It is mainly via her research on animals that Birke has tried to reconcile her experimental work as an 'insider' biologist with her ideas of a new approach to biology informed by her involvement in 'outsider' groups like the Radical Scientists, Dialectics of Biology Group and, of course, feminist groupings. What was more specifically informed by her feminism, she indicated during one of our interviews[10], was her approaching animal biology as a complement to the understanding of gender differences.

This is particularly shown in her series of papers on neonatal exposure to hormones in rats and pups (Birke and Sadler 1983, 1984, 1985; Birke, Holzhausen, Murphy and Sadler 1984: Holzhausen, Murphy and Birke 1984). In other writings on the subject, she contends that observations of animal behaviour and research in biological development ought to invite biologists to develop holistic and dialectical explanations of life phenomena in general (Birke and Archer 1983; Birke and Silvertown 1984), and not least importantly, of the relationships between sexual development ('nature') and gender development ('culture') (Birke 1986). Therefore, even though she stresses that research on animal behaviour should be done for its own sake, she proposes that the approach, method and theoretical insights she is advancing should invite a renewal of traditional ways of researching topics in both animal biology and human biology (Birke 1988).

In the introductory chapter of Exploration in Animals and Humans which she edited with John Archer (1983), Birke discusses the theoretical deficiencies of the conventional reductionist approach to the topic of animal "exploration behaviour". According to Birke and Archer, this conventional approach is unable to detect the underlying complexity of real behaviours. It produces biased interpretations of animal behaviours studied in laboratory and it underestimates the richness and wide array of behaviours displayed by animals in the wild. As Birke and Archer explain, 'exploration behaviour' is a generic category that includes discrete types of behaviour such as sniffing, drinking, making contact. These discrete sub-categories, however, are used unwarrantedly in conventional reductionist-based research, and as such do not greatly help to predict the behaviours of animals in more complex environments and over protracted periods of time in the wild. In these latter conditions, several factors (e.g. anxiety, habitus and boredom, extreme states of hunger) might contribute to alter the usual patterns of behaviour. The conditions of space and time of animal behaviour in the wild are thus not well reproduced in laboratory. As a result, Birke and Archer hold, the "ecological validity" of laboratory-controlled stimulus-response patterns of behaviour in animals must be severely questioned.

In response to the shortcomings of the conventional reductionist approach, they stress that broader distinctions between types of exploration might lay the grounds to an approach more attuned to the ideas of agency, novelty (of the milieu), and motivations changing in time and space. To justify this, they refer to authors who have discerned different forms of exploration for food among on the one hand, animals used to 'eating in meals' and, on the other, those (usually in the wild) which have to search for food. They also point to similar results regarding behavioural differentiations in animals and in children[11].

Birke and Archer also levelled other types of critiques, namely in relation to the ascription of purposes to certain types of locomotion. They contend, for example, that some behaviours are often endowed with exploration purposes, in spite of not being truly exploratory. In conventional types of research also, exploration is often quantified by the number of movements, thus neglecting the fact that animals do not all need the same amount of manipulation or sampling in order to have a good knowledge of their surroundings. Finally levels of food deprivation are re-constituted artificially in experiments which diminishes the ecological validity of the trials. Birke and Archer state however that these flaws in conventional research are not insurmountable. These could in fact be avoided simply by designing more sophisticated experimental trials.

In other instances, Birke and Archer question the cogency of concepts like "unfamiliarity", "optimal behaviour" and "exploratory motivation". In this case however, they acknowledge that the difficulty is not as easy to overcome. For a solution to this problem depends essentially in defining exploration in observational terms, which is empirically delicate to undertake, and also subject to diverse theoretical critiques.

The problem of defining 'novelty' and 'exploration behaviour' in animal research raises similar uncertainties as studies of human action. I would suggest that as the very idea of 'exploration' lends itself to a hermeneutic investigation as it is associated (at least in studies of humans) with meanings and purposes, it is no wonder that Birke and Archer must ultimately engage in the embarrassing circularity of defining the notion of novelty by that of exploration and vice-versa; for their critique heralds the traditional difficulties relating to the methodology of the 'hermeneutical circle'[12]. But since they engaged in studies of animals in the first instance, Birke and Archer are -- not surprisingly -- reluctant to enter a discussion of the 'zoological method' in terms of the hermeneutical circle -- hermeneutics being too closely related to human studies. They prefer, therefore, to stay within the epistemological arena of the empirico-analytical sciences, to use its central methodological tenet (i.e. the pragmatic criterion) and value-assertions (e.g. animals are analogous to machines). The following quotations indicate clearly their epistemological orientation, as they attempt to construct standard observational concepts rather than hermeneutical categories as such:

> An animal may exhibit a variety of responses to a novel or conspicuous stimulus. It may show orienting responses, it might attack, it might scent-mark, it might run away or freeze, it might sniff at, or pick the object up. Much depends on the type of stimulus, the species of animal, and whether the animal has had prior experience with that type of object. (1983, 6)

> The behavioural outcome will depend, for instance, on the nature of the stimulus, the animal's physiological or behavioural state at the time of the stimulus is encountered, or the context in which the novel stimulus occurs. Further, different types of stimulus may be expected to elicit very different responses from different species. (1983, 10)

Birke and Archer admit that it might be impossible to base observations of animal exploration on objective criteria that would not be tautological. And this undermines drastically their argument in favour of notions like "adaptive strategies", "cognitive mapping" or explorative "process". From their own point of view on biological epistemology however, these notions remain both theoretically and experimentally sterile, for they do not seem to add any clearer observational canons nor non-tautological explanations.

As I see it, Birke and Archer raise the debate expounded by Gellner between empiricism and materialism, but this time relative to animal rather than human behaviour; this can only add to the vacuousness of their epistemological arguments[13]. As zoologists, they have a preference for the reductionist methodology of 'Skinner-inspired' objectivism, but as critics of behavioural theories, they tend to lean towards a 'Chomskyian' (mentalistic) outlook on (animal) 'action'. Gellner helped to point this out: when one thinks the latter (mentalism/ action) is directly translatable into the former (reductionism/ objectivism), then one is expecting the impossible, the direct translatability of approaches rooted in two different epistemological categories -- selectors of information and selectors of explanation. If they only tried to argue for changes in some values entering theory-building (e.g. animals are more like humans than like machines) without overstating their critique of methodological reductionism (behaviourism), their methodological position would be more 'tolerable', and their arguments for a new theory of animal behaviour, taken more seriously.

I should like, in the prolongation of my argument, to point to the fact that rhetorical discourse is not absent from Birke and Archer's work. It is clear that in the introduction to their book on animal exploration, they adopt a manichean position, opposing terms like "mechanistic approach", "separate spheres", "end-point", "artificially constructed environment", "drives" and "incentives" on the one hand (none of which being congruent with notions of 'dialectics', 'holism', 'changes', 'agency'); and, on the other, terms they favour, such as "environment", "not readily identifiable [types of behaviour]", "interconnected", "systems", "exploration" and "ecological validity". But these latter terms only emerge ultimately as sustaining an approach which remains vague, ill-defined, and empirically inoperational from their own point of view, although more congenial to an approach to nature and wildlife as complex, rich, and 'not always' predictable. This vocabulary reflects what Gellner has identified as a 'negative-endorser theory'. That is, as a critique of traditional models of explanation, Birke and Archer's perspective might be very powerful, but as a fully-fledged approach (and epistemology) it remains shaky.

It is true, however, that in their presentation, Birke and Archer provide empirically documented arguments against 'reductionist' explanations of behaviour, reductionism here being criticized because it simplifies and misrepresents what behaviours are really like. But the counter-arguments they advance do not seem to transcend semantics; they are presented as linguistic and tautological rather than as alternative observational categories. It is somehow understandable for critics of crude mechanistic models of animal behaviour to be tempted to use <u>theoretical</u> anthropomorphism as an argument and rhetoric as a tool for challenging biological reductionism in zoology. But, as they are experimentalists themselves, Birke and Archer wish, first and foremost, to justify <u>methodologically</u> the observational design they propose. However, because they partially fail in doing so, they seem compelled to overstate their opposition to reductionism by use of rhetoric. As I would suggest, if they would accept that rational discursive arguments play a necessary role in scientific research, they would not feel so obliged to subject their new research programme to the strict epistemological conditions spelt out above.

But how does all this relate to specifically feminist science, one would ask? It relates to feminist science insofar as it is about grounding, epistemologically and methodologically, an approach to biology which would integrate biological and environmental determinisms (that is based on the value-assertions of holism and material dialectics) and, moreover, the ideas of structural determinism and agency of animals, humans and lower life forms. Let us examine some other works of Birke in this connection, and ascertain if there is more to see in them than a shaky alternative epistemology to conventional biology.

In a series of articles published in mainstream journals such as the Journal of Endocrinology, Developmental Psychobiology and Physiological Behaviour, Birke and her colleagues describe the results of their own experimental trials on young animals (Birke and Sadler 1983, 1984, 1985; Birke, Holzhausen, Murphy and Sadler 1984; Holzhausen, Murphy and Birke 1984). In these articles they make, among others, two points. First, natural forms and higher organisms change according to other life forms and organisms with which they are in contact in the environment. Secondly, and consequent to this, the differentiation between male and female animals is expected to be shaped by the interactions between hormonal make-up and environmental conditions (e.g. food supply, territorial space, 'natural' constraints) including maternal behaviour towards these animals (and assuming that the latter is also hormonally activated differently by male and female pups). Whether maternal behaviour may, in turn, give rise to differentiated behaviours in male and female youngs is a central question in the study. In their study, Birke and her colleagues designed experiments in order to investigate this entire 'bio-behavioural' process.

In one of these articles, the authors presented a study focusing on female pups exposed to maternal hormones via the milk-suckling of progestins-administered mothers (Birke, Holzhausen, Murphy and Sadler 1984). Their method is designed to control for the possible effects of other factors, and they use standard techniques for measuring hormonal secretion. Their results show that hormonal exposure via milk alters luthenizing-hormone (LH) cyclicity in the female pup. This, one would hold, is fairly similar to standard biology, with the exception that it is about female hormones (i.e. progestins) rather than, as is usually the case in animal research, about male hormones and based on all-male samples. Indeed, as the authors themselves indicated, their study is an attempt to expand the "extensive work ... carried out in an attempt to understand the effects of androgens and oestrogens on the mechanism of sexual differentiation in mammals" by researching "increasing evidence ... suggest[ing] that progestins influence sexual differentiation" (1984, 149).

In another of this series of articles (Birke and Sadler 1985), they tried to capture the possible impact of a 'reverse' effect, that of hormonally treated pups on maternal behaviour. The experiments were designed to test the hypothesis that behavioural development of pups could be due to the combined effect of neonatal hormones and changes in maternal behaviour. In the first of two series of experiments, it was found that medroxyprogesterone acetate-administered dams increase their rate of pup licking, but not of the other maternal behaviours observed (i.e. grooming, feeding, exploring, nest-material manipulations), which is a surprising result as such. In spite of this, the authors suggested that "it is also possible that the effect of the steroid is not to change the stimulus characteristics of the pups, but to change maternal behavior per se, since the hormone was administered to the dam in the first instance" (ibid., 471). In the second series of experiments, hormonal manipulations of pups actually resulted in alterations of several maternal behaviours of the rat dams. However, the authors could not tell whether these alterations were actually due to changing pups' stimulus or if they were directly affected through the ingest by the dams of the pups' excreta and urine during licking behaviour. Other findings showed overall sex differences: male pups receiving more anogenital licking than female pups in the same observation groups. But these differences, the authors suggested, might be altered by cross-over effects. As a matter of fact, dams' licking may persist in the absence of male pups, depending on whether a female pup presents itself to its mother subsequent to the dam licking a series of male pups. The authors finally concluded that, however "limited support" their study gave to their prime hypothesis, "alterations in the dam's behaviour may have to be taken into account in assessing the overall effects of the hormones on long-term behavioural consequences" (ibid., 475).

Yet the 'limited' support provided to Birke's approach on hormones and behaviour by her studies of 1984 and 1985 was, for her, only the beginning of increasing empirical evidence that she was thinking in the right way. As she put it in one of our interviews, "the more I work with animals, the more circumspect I become about the prime role of hormones in the development of adult behaviour"[14].

The question of animal research in a feminist science, as Birke stressed more recently in a conference entitled 'Biology, Process and Gender: The Impact of Feminism' (Birke 1988), has wide resonance at the level of terminology and interpretation. Referring to the field of sociobiology, for instance, she pointed to feminist denunciations of observational categories like 'rape' to describe the behaviours of animals. She also gave credit to studies made on female animals helping to counterpose the idea that males alone have contributed to the evolution of social structures[15]. The impact of these criticisms is now detectable in mainstream biology itself, as several sociobiologists and primatologists now take the results of these studies into genuine consideration[16].

Birke's approach to and actual research on hormones and animal behaviour is indeed congenial to her holistic and dialectical approach to gender development. But they also evidence the fact that this approach to animal behaviour is largely compatible with the Popperian epistemology of critical realism and methodology of falsificationism, and just fall short of affording hard-core credibility to the specific hypotheses Birke and her colleagues have advanced. In addition to this, we also see that Birke's idea of a feminist science, a feminist biology namely, lies primarily in the appeal for a women-centred research agenda, much more than for methodological changes.

In a more radical perspective on animal biology, however, Birke thinks that a feminist science might be justified to be politically more confrontational and to consider itself epistemologically revolutionary. In More than the Parts, a book of critical essays she edited with J. Silvertown (1984), Birke examines the treatment of animals in biological research. She judges that the current "callous disregard for the sentience and feelings of the individual animals" is a direct result of the dominant capitalist and patriarchal ideology; that animals are taken as machines and commodities serving the purposes of consumerism and capitalism. She argues that, as an alternative to this, a true liberatory science would "involve attempting to develop a different perspective on why and how we use animals" (Birke 1984b, 234). She argues, moreover, that scientists' obsession with objectivity itself basically perpetuates this state of affairs, for it forces a separation between the realm of humans and that of animals which, in turn, justifies the inhumane character of research on animals. A science which construes liberation as the technological control and mastering of nature is not truly liberatory, she claims. But what is more particularly interesting in her argument is her suggesting, as a great number of feminists have done, that the absence of women is mainly responsible for the maintenance of the 'dualistic faith in nature versus culture' (ibid.). For Birke and other feminists, women have been socially assigned to caring and nurturing behaviour, and to subjective reasoning. Therefore, Birke says, as part of a community of scientists seeking urgently for "respect and love for its subjects of study", women are likely to play a central role in the elaboration of a true liberatory (feminist) science.

> That science may seek non-reductionist ways of understanding the world, but in doing so it will need to accept that animals, that are part of the world, may have claims to moral rights in the same way as humans do. It would hardly be any kind of 'liberatory' science that was centred around an ideology opposed to oppression and yet maintained the present mechanistic view that nature is there for our exploitation. If we are to survive, then we are going to have to adopt -- with considerable urgency -- a more holistic and co-operative approach to nature... We may retain the power to alter our environment in a way that no other species has yet been able, but if we are to exercise that power, we will need to do it in a way that preserves the integrity of the whole. (1984b, 234)

Thus, as Birke recently emphasized[17], the question of animals in research should become more important in the construction of a feminist science. This question, as she put it, is co-extensive to the woman question in biology, in the sense that "there is an overlay between nature and culture, between the human species and animals, and foremost because women are closer to nature than men". And since feminists want to prioritize research on women's diseases they must start by asking, "at what cost"; and "shall we continue to use animals uncautiously [for this purpose]?". But these questions, Birke retorts, have not as yet been addressed seriously by feminists[18].

The elements discussed in this section give indications of Birke's feminist thinking on biological science, but also of its strengths and limitations as a fully-fledged 'successor' or alternative science to conventional biological reductionism. As a feminist and socialist, her own approach to biological thought is based on the premises that lower and higher organisms are highly interactive and more than the sum of their parts, and that nature should be in harmony with human life. One will have noticed, however, that the ideas of agency (or teleology) in both animals and humans, and more importantly perhaps, of a dialectics between individual organisms and social environment lie at the centre of her biological theory of behaviour. But taking this ontological stance has precipitated some serious problems of justification; in the end, it is the epistemology of critical realism, normative in the empirico-analytical sciences, which Birke seeks to vindicate with her new approach. The thesis of developing a humane and caring approach to animal research on the basis of 'feminine values' also seems to be, in Birke's critique, fairly speculative. There is no significant sociological evidence suggesting that this is viable, with or without the participation of militant pacifist feminists. In brief, the idea of a '<u>woman-centred agenda</u>' as the platform of a 'feminist zoology' (namely a research agenda focusing on female animals) may be justified sociologically, but from either a <u>methodological</u> or <u>theoretical</u> perspective, I do not see how a feminist approach to 'animal biology' based on the privileged accessibility of women to the 'female body' would provide a different insight from the holistic method and approach defended by dialectical biologists or even mainstream biologists, men and women. The next section, in which we examine Birke's reflection on human biology and womens' studies proper, is likely to be more relevant to a 'strong' project of 'feminist' biology.

Birke's Reflections on Biology and Womens' Studies

Throughout her 'career' as a feminist biologist, Birke has grappled with the following questions consistently: female gender development and its relation to sexual hormonal make-up (Birke and Best 1980b; Birke 1982, 1984a, 1986; Birke and Vines 1987), biological determinism of lesbianism (Birke 1980, 1986), and bio-medical research in relation to women (Birke 1980, 1984a; Birke and Vines 1988). In this section, we shall focus on Birke's approach to female gender development as a prolongation of the examination set out in the previous section. It is noteworthy that the questions mentioned above and with which Birke has dealt are all, from a social point of view, profoundly controversial both ethically and politically.

As mentioned earlier, Birke always approaches biological problems from the perspective of a two-fold dialectic -- between science and politics, and between biological and environmental (and cultural, in the case of human beings) components of living experiences. In other words, the development of biological knowledge is conditioned by social structure and the development of living forms is conditioned by the environment.

Examining the problematic of sex and gender development more particularly, Birke takes into account both the impact of biology on gender, and vice-versa, the impact of social learning on biological identity. She does not deny, like many feminists, the existence and role of biological entities per se; she minimises, however, their overriding influence. As a result, she argues against crude forms of biological determinism, more especially in the study of human behaviour and development, and favours a view based on the notions of "constraints on the system" and of "process"[19]. Her biological approach to the subject matter always takes into account the impact of social expectations and meanings on the actual physical or biological experiences undergone by men and women, but also by people of different cultures. Thus, human biological phenomena should always, in her mind, have to be understood as mediated by the cultural context in which they arise[20].

In her book Women, Feminism and Biology (1986), Birke dismisses both the bio-reductionism of mainstream biology and the constructionist (or culturalist) approach to women's biology advanced by some feminists. These views are rooted in a separation between the biological and the social domains which she precisely aims to integrate. She admits that reductionism has proved useful in the life sciences, especially in areas like toxicology, immunology, virology; however, she remarks that for the study of some phenomena (such as premenstrual tension or adult homosexuality), the integrated approach she proposes is much more cogent. The reductionist view is merely "additive", exploring the effects of social factors only once the biological components are 'controlled for'. It does not capture the biological alterations of living forms as they happen, in interplay with the forces in their environment. The constructionist view, on the other hand, totally denies the biological reality of women's and men's bodies, construing biology as mere scientific construct and the body as a linguistic abstraction, which, as such, contravenes any hard-core empirical evidence of biological facts. Birke's own approach to biology states that biological constraints exist but that these are experienced differently in different contexts: they are 'experienced very differently' for instance, "if you are living in the affluent West rather than in famine-torn Ethiopia" (Birke 1986, 170).

In a more recent paper (with Vines, 1987), Birke developed more systematically the "integrative approach". The starting point of any approach to biological development, they argue, should be based on processes of building rather than "processes of unfolding towards a definite end-points". With respect to gender development therefore, they suggest that feminist biological theory should fundamentally engage on this "change of emphasis" (Birke and Vines 1987, 555), and aim to elaborate a "transformative model" (ibid., 564). They give evidence that in both child development and adulthood, biological functions and organic elements evolve rather than are 'fixed': tissues are broken down and replaced, growth of parts of the body often occur, and hormone production is altered by stress or by other factors such as the bone structure, for example. In addition, social studies of behaviour accord additional credentials to the model of biological development they defend. The transformative model gives rise, however, to some difficulties, and these are the problems a feminist science must now begin to address.

According to them, the additive approach is, relatively speaking, much more manageable than the integrative approach, for the main reason that the idea of interactionism (between diverse components in development) implies that biological and social factors are potentially separable, even though they affect each other reciprocally (Birke and Vines 1987; also Birke 1986). Reductionism also provides useful results, even though these remain partial in view of the complexity of the phenomena being studied. Hence, they admit that separable biological 'objects' (including behaviours) are often easier to study than 'processes' (Birke and Vines 1987, 565). Indeed, Birke and Vines are reluctant to reject the traditional experimental norms of justifying biological results. They also have much difficulty operationalising their own transformative approach in order to justify its relevancy on the same experimental canons as the additive model inspired by reductionism. The following quote illustrates this well:

> Quite possibly, the very methods of science will limit how we study nature's processes of interaction. Science has so far proceeded largely by using a kind of methodological reductionism, by means of which factors are isolated and controlled. This does, however, make the study of very complex interactive processes difficult, simply because it separates out the component parts in order to investigate them. In short, once we have removed ourselves from the level of theoretical abstraction in order to study the interactive processes that comprise the theory, we may have some validity, although it is rooted in an assumption that our present methods of science are both optimal and unchanging. But the methodology of science may well change if less reductionist questions are asked in the first place, as a feminist science might well do. (1987, 565)

But there is a more serious problem that Birke and Vines must deal with if they aim to vindicate a new feminist approach to biology. They wish to modify the epigenesis model developed by Waddington[21] by allowing the existence of multiple end-points rather than pre-assigned end-points. In this new model, it is assumed that gender development may have more than only two end-points (that is male or female gender attributes), and this, Birke and Vines claim, should provide a "new line of thought", a new way of "conceptualizing the nature we study" and of "thinking about it", an "alternative rhetoric [sic]" (ibid., 568-69). However, they remain rather cautious in suggesting that they have found a new scientific method. Although they do not state clearly whether they aim at changing preconceptions and the dominant mode of thinking alone, or if they also claim to change the methods of biology, what they implicitly stress are the flaws of rigid conceptualizations of biological potentialities rather than those of the experimental method itself.

To put it briefly, Birke and Vines are well aware of the practical difficulties associated with the scientific (or methodological) implementation of a feminist approach to biology based on holism and the notion of a complex magma of interactions involved in the development of biological (or phenotypical) traits of the sexes. On the other hand, they seem reluctant to recognize that the current procedures of science are those with which they cannot dispense in order to justify their own feminist stance[22].

It is on these grounds, however, that Birke has endeavoured to develop an approach to women's biology. According to this approach, female development is to be explained in terms of a conjunction of biological traits and social meanings -- of an integration rather than addition of these two series of factors (Birke 1986, 1988). She has used the terms "lived experience of our biology", "woman's identity of herself as a woman", "constantly reconstructed sense of gender" without refraining, however, from using notions such as "biological self" and "biological experiences" in order to express that class of phenomena -- sex/gender development. She writes,

> Thus adult gender identity, as woman or man, can only be understood as the present, dynamic point in a continually evolving process which, I would argue, includes the real lived experience of our biological selves as well as the social meaning and economic context of our lives. (Birke 1986, 104)

As mentioned earlier, Birke's commitment to dialectics is not only scientific in the theoretical or methodological senses of the term; it is always scientific and political. Hence, in addition to developing a 'less rigid' approach to biological development of gender differentiations, she has always made a strong defense for a less prejudicial research agenda on the subject of women's biology and for more research on menstrual cycle, menopause and mental distress (Birke and Best 1980a; Birke 1984a). It is crucial for Birke to emphasize the value of a subject matter like that of woman and biology for its own sake in future research. But it is even more important to show that from a scientific and biological viewpoint, 'woman' is not a fixed, unalterable form, and to stress that even though 'woman' is a specific biological category different from that of 'man', it nevertheless bears in itself a potential for changes and for internal differentiation. Women are born women, and their further biological development is a hybrid product of social and biological interactions. And as much as biology is simply not womens' destiny; so gender is something which is not fixed. Birke contends:

> A woman's biology, and her experience of it, do not exist in a social and political vacuum, but in a society which is criss-crossed by social divisions of all kinds in addition to that of 'gender'. These in turn can affect women's experiences of biological events, and can possibly affect components of the biology itself. It is at the level of the lived experience of our biology that both the similarities and the differences between women become manifest. (Birke 1986, 105)

> Human beings constantly reconstruct [themselves] throughout their lives, both with respect to themselves and to other people around them. That is, gender is itself part of the interactive processes, rather than being a fixed property of individuals. (ibid., 103)

But there is more to say about Birke's commitment to the context-bound view of biology and gender. First, it is indissociable from her commitment to a biology of collectivities in general (rather than of individuals alone); and secondly, it is equally so with regard to women as a group in particular. As a participant in the Brighton Women and Science Group and the Dialectics of Biology Group, Birke has alternately insisted on holism and dialectics. A more dialectical biology, she argues, would secure three important theoretical principles: first, a view of the real links between sex and gender, these being interactive rather than in a crude cause-effect relationship; secondly, that gendered roles are largely historically-grounded and not biologically irreversible; finally, and epistemologically, a constant questioning of fundamental categories of explanation, these being shaped by the social ideologies of the time and, consequently, contingent rather than universal (Birke 1982).

An example of this is her position in "The Tyrannical Womb" (Birke and Best 1980b). In this article, Birke rebut scientific explanations proposing the universality of menstrual symptoms and of a decrease of intellectual activities during the menses, and she documents and strengthens her point with the results of studies carried out on the subject. Rather than this being a refutation of her biological approach to women in terms of a 'collective category', it instead is employed to support her approach. She points out, for instance, that if one takes women in diverse cultures, they will report diverse menstrual symptoms, but this precisely implies that differences might be due to culturally-bound anticipations of menstrual discomfort and social taboos associated with menses in different classes, ethnic groups, and countries. Birke therefore suggests that biological and psychological elements should be taken into account in order to explain a variety of symptoms related to menstruations[23]. Birke recognizes, for example, that the occurrence of hot flushes, as the oestrogens decline during menopause, is validated empirically; for the symptoms of tiredness, dizziness, insomnia, or depression "are real enough to the woman herself" (Birke and Best 1980b, 104). She also notices however, that some of these symptoms do not occur in other cultures and that "the severity of the symptoms may be due to how women feel about menopause, and how they see themselves" (ibid.) -- as culturally conditioned.

Birke also denounces the hormonal theories implemented in medical treatments and in the pharmaceutical industry, all of which foster the idea that the social inferiority and irritability of women are 'naturally' unalterable. In the same vein, she contests the reductionist rationale of women's inferiority and irritability being rooted in their biology, on the grounds that women's alleged 'nature' is in fact largely the result of social learning and oppression[24]. This stand must be made strongly, Birke argues, for if not, then reductionism will continue to pervade biological knowledge about women and, as a result, the ideology of patriarchy and individualism will remain unchallenged. In this connection, Birke has, for instance, questioned the studies focusing on the 'negative' behavioural changes due to menstruations, rather than those which might reveal the energetic and creative impulses of females at mid-cycle. As she has often argued, several dominant groups indeed have vested interests in maintaining deterministic theories of women's biology and behaviour. And this makes her contend that changes in scientific and commonsense ways of thinking about gender will not be achieved easily. Rather, this will need political struggles and stringent reforms (Birke 1980, 1982, 1986)[25].

This is also why, as a Radical Scientist, she has been a regular contributor to the magazine Science for People. She believes the popularising of biology would prepare the path to genuinely democratic solutions to the social and ethical issues involved in biological research. As a feminist, moreover, her commitment to a democratization of science would be aimed at defending the worth of women's knowledge, of themselves and of the world around them, whether introspective (as highlighted in consciousness-raising groups, and data produced on the basis of qualitative research and life stories) or practical (as that of midwifery).

To summarize, Birke's contributions of the early 1980s were an attempt to systematically criticize scientific statements about women's health (namely the effect of genes and hormones on female behaviour and the medical treatments of women developed thereof), and to demonstrate their mythical content and taboos. The domination of biological reductionism within biology, she would argue, and the authority of science on the lay public have contributed to maintain, for instance, the existence of patriarchal biases in the understanding of women's behaviour and health, especially in clinical diagnoses and in medical treatments. Also, the low participation of women in biological research may have contributed to maintaining theories about women's biology and behaviour which are spurious, or else incomplete. These political and theoretical critiques of biology constitute, I would argue, the greatest strength of Birke's feminist biology.

Indeed, Birke has, in her more recent reflections, argued that methodologically, biology is far from implementing a holistic and dialectical view of human biology: it still upholds an additive model in which, in the last instance, genes and hormones are considered the most important factors to study in biology. As she has implicitly admitted however, she has not convincingly developed the transformative model which would scientifically justify her integrative approach of developmental human biology. This leads us directly to the last sections of this chapter in which we shall discuss why Birke only partially succeeds in generating a fully-fledged feminist 'successor' biology.

Birke's Idea of a Feminist Science

> It is not, strictly speaking, a feminist science that is being addressed, but the question of how science might look like in a more 'feminist' and egalitarian society, a society in which notions of gender did not imply notions of hierarchy. I shall continue to refer to 'feminist science'... but it should be borne in mind that no such thing is possible within our present society. (Birke 1986, 143)

As Birke puts it, the concerns of feminist science are utopian, for the reason that it is not possible to remake science in the absence of more general social changes. She retains the principles of the Radical Science movement in the elaboration of a socialist <u>and</u> feminist science: to defend human needs rather than corporate greed; to be socially accountable rather than elitist and mystifying; to seek harmony with nature rather than its exploitation for military and industrial purposes. More specifically, however, a feminist science would not, in contrast to a socialist science, perpetuate in any way the subordination and oppression of women (Birke 1986). All these elements are congruent with a politics of science. But what is, on might ask, the relationship of these principles with the constitution of a new scientific platform for biological explanation <u>per se</u>; or of a new biological method?

In my view, and as suggested earlier, the tenets of a feminist biology as expounded by Birke are first and foremost <u>conceptual</u>, relating essentially to how biology ought to represent nature and human life. But she does not go as far as laying the grounds for a new biological method, let alone a new scientific epistemology. By and large, biologists seem to hold quite similar views on human biology, and, like Birke, accept the methodological tenets of biological reductionism but only with qualification. As far as a biology of human behaviour is concerned however, mainstream biologists seem to hold these views in principle but not in practice. Institutionally and normatively, biologists do not address the problem of human behaviour in a strict sense; they would rather leave that to psychologists. In their laboratory at any rate, biologists aim to 'control for' environmental factors, and to isolate biological causes per se. It is true, however, that some biologists are committed to finding 'ultimate' biological causes, even in cases of patterns of human phenotypical traits (e.g., of finding genes or chemical reactions which would determine 'everything else' in life forms); but these biologists seem to represent a minority among mainstream biologists.

At the methodological level, Birke appears to develop her approach and theories on the same rules of validation and justification as mainstream biologists. It might be suggested here that her 'integrative' approach to biology and her 'dialectical' viewpoint on the production of scientific knowledge could have lent themselves to a hard-core epistemological revolution of biological thought. Yet between traditional empiricism (or, more accurately, critical realism) and the sort of epistemological vacuum with which relativism and deconstructionism theories of knowledge have left natural scientists, Birke definitely opts for the former. Her rejection of biological determinism in favour of holism and dialectics does not transcend the conventional rules of scientific validation: these are still those of sense-data and experimental replication. Her critique of reductionism might have invited a challenge of strict empiricist epistemology (as in social studies) in favour of a more argumentative and discursive set of procedures of justification. Indeed this challenge seems to be the only 'scientifically acceptable' way in which her genuine effort to introduce a new body of concepts, hypotheses and explanations of human behaviour in biology, could be vindicated. In lieu of this, Birke appeared to believe, ultimately, that the only way a 'successor' feminist biology could be fully developed was on the basis of a politics of science.

What does it imply epistemologically, conceptually and organizationally (or institutionally) to 'remake' the science of biology on feminist tenets? Does it guarantee a continuity between traditional (or mainstream) biology and feminist biology or a complete epistemic rupture? Will it still be question of science; or else of another other form of knowledge altogether? What precisely would be different in a new feminist science and what would remain the same as in mainstream biology? To all those questions, Birke would fundamentally respond that what she has in mind is a 'successor science'.

The notion of a 'successor science' implies, as Birke puts it, that a given 'new' science "would build on what we have got, changing what needs changing and retaining what needs retaining". And she also argues that a feminist deconstructionist perspective on science is not incompatible with continuity: "the process of building a feminist science involves the process of deconstructioning if you like... but I don't believe [biology] will not be any longer science if we change it"[26]. A feminist successor science would more particularly, Birke adds, be socialist, less authoritarian and profit-oriented than current science, and last, but not least importantly, it would be:

> A science that does not denigrate and ignore women's experiences, but that emerges from what we now have while retaining its epistemological unity... It would probably ask different questions, addressing real human needs, and embody a different worldview. (Birke 1988, 308-9).

As we noted earlier, Birke mainly endeavours to simultaneously develop a political analysis of science (i.e. its incorporation into a larger social context, its process of decision-making, its research agenda) and an approach to biological explanations as the two-fold platform on which a successor feminist science would build. How is this actually achieved and with what degree of success?

In her book Women, Feminism and Biology, Birke launches a systematic comparison of the characteristics of current science and those of a potential successor feminist science. She sets this out on a discussion of three main issues of scientific practice:

the social role of expert knowledge in society, subject matters and research priorities in modern science, and critical evaluation of the scientific method.

On the first issue, it seems that her arguments in favour of a democratization of scientific policies are rather sketchy and do not explore thoroughly the practical difficulties of implementing such a political platform. Indeed, the democratic control of expert knowledge is problem-ridden, as a century of reflection and attempts to rule over technocrats and to share decision-making indicates[27]. But moreover, Birke's defense of the potential benefits of a democratization of biology sounds more dogmatic than supported by a sound sociological or political analysis of decision-making and negotiating processes: it seems merely motivated by the dogma of Radical Science.

With respect to the subject matters of science, Birke makes a strong challenge of the present research agenda of biology and medical research; but it mainly relates to the political agenda of the larger feminist movement and does not truly contribute original concepts in terms of a new cognitive approach to knowledge on reproduction. She discusses, for example, the issues of in vitro fertilization and reproduction (Birke and Vines 1988). The clinical, ethical, and social problems of 'in vitro' technologies she raises are commonplace among current controversies. Moreover, her position on these matters does not seem to invite political compromise. In another instance, her arguments in favour of womens' freedom of choice to have an abortion and her strong stand against potentially increasing medical and legal control of women's bodies on this issue relate to a politics of science, and is, here again, parallel to the political agenda of the feminist movement at large. Her rhetoric on 'real human needs' does not examine exhaustively, as she herself admits, the substantive issues related to reproduction and research on embryos (ibid.)

Finally, regarding the methods of science, Birke appears invariably to equivocate consistently between a discussion of method, ideology, organization, scientific policy, and social structure. This does not help to pin down what she understands by the term scientific methodology[28]. In spite of these weaknesses, her reflection on science methods in a feminist successor science has a prominent place in her argument. She links the epistemological issue of the biological 'method' with those more readily political, of fundings, research priorities, the dependence of scientific research on industry, the State and, more particularly, medical practice and public health care. As she explains, the social system which supports these research activities affects, in turn, the content of biological theories themselves: as such, politically powerful groups may have a clout on the subjects being researched and control the diffusion of results at their convenience; and doctors, psychiatrists and biologists, mostly males but also females, will recapitulate received ideas, cultural prejudices or even myths about women's biology and behaviour, without feeling constrained (from within the scientific community) to question the validity of biological judgments based on these ideas, prejudices and myths.

But the angle from which Birke examines the interplay between power groups, dominant ideology and science seems rather narrow. Even though she often admits that social relations are, in practice, more complex than how they are defined in theory, it is a fact that her views of the social relations giving rise to a feminist science are not only speculative, but also bear the same superficiality as her background analysis of the current social relations in scientific practice.

Birke's three-fold comparison between mainstream biology and feminist successor science is therefore relatively weak. She raises a great many issues which she does not fully investigate; moreover, her analysis is not always predicated with clearly defined concepts: this undercuts its viability as a blueprint for the implementation of a feminist successor science. It is not surprising that she tends to speak in terms of a feminist 'utopia' rather than of a fully-fledged 'project' of feminist biology. In the last instance, therefore, her greatest achievement as a feminist biologist is not the elaboration of a new method, but rather (and valuable) as it is, a condemnation of the scientific activities having direct adverse effects on the quality of life of women. Mainly, she urges feminists to enter science and to challenge it from within by setting up a new research agenda. But in the last analysis, ideological rhetoric appears to dominate her discussion at the expense of a more cogent analysis of the relations between society and scientific knowledge.

In fact, Birke urging feminists to enter science in greater numbers appears, although somewhat self-fulfilling, a more cogent argument for the idea of a feminist biology. Her views on this seem also relatively original within feminist literature, as she does not defend the notion of 'feminist biology' on the ground that the caring attitude of women will change biology; rather, she conceives of the project of a feminist biology as a political challenge for feminists. It is true, however, that feminists ought, according to her, to seek concrete action to "look for more cooperative, and generally non-invasive, ways of understanding nature" (Birke 1986: 150), which, in a sense, retains the gist of the 'feminine values' premise in the feminist theory of knowledge. But, here again, her understanding of this premise seems more political than epistemological; that is, in the elaboration of a new science, feminists would make a strong political defense of the ways women have traditionally related to nature, people, and human life. This indeed buttresses her claim that only feminist scientists can start building a successor science. For they are likely to be the only people who would think of -- and are committed to -- starting a reform from within current science and engaging in a challenge of the conventional biological approach to gender differences and woman's biology. This would mean to promote a view of biological organisms and 'events' as more complex and dialectically related to the environment -- natural and social. Thus, as such, Birke's idea of a feminist successor biology lies in the construction of institutional structures which would enable feminists to advance new descriptions of 'nature', new perspectives on human biology, and, ultimately, biological explanations which would be "more representative of the social and natural world" (Birke 1988, 311)[29].

Conclusion: Towards a Feminist Paradigm in Bio-behavioural Studies

> In one sense then, science does not have to change its commitments, as change the balance between them; commitment to the kind of relationship with nature that feminists have envisaged are already there in the history of science -- even if this is hard to see in the face of current scientific orthodoxy. But the question still remains; what does it mean for science to change... and still be considered science? (...) I assume, however, that we would still call [the kind of feminist commitments in a project of biology] 'science' in the sense that it would still be about the discovery of the ways in which the natural world works. (Birke 1986, 146)

Birke's project of a feminist biology, as the above quotation illustrates, primarily posits a contrast between patriarchal and feminist interpretations of human biology. But these feminist interpretations must always ultimately be justified on empirical grounds. Indeed, this quotation clearly highlights two separate levels in Birke's feminist project of biology. First, there is the level of the historical representations of 'nature' and 'life' that human beings make of their world by ways of deciding which sort of 'relationship with nature' and of communal life they wish to undertake. This involves, as Mary Hesse would suggest, a decision on the values entering theory-building and underlying the kind of model for knowledge of nature and humans best suited to our representations of the world. They partake of deep-rooted yet historically and culturally contingent ideologies. Secondly, when Birke refers to 'the discovery of the ways the natural world works', she speaks at the level of value-goals, and as a consequence refers to rules governing how theories should be justified. We have seen that in Birke's works as a zoologist, her discussion of the justification of biological theories is basically constrained by logical rules rather than by political ones. Birke's feminist 'zoology' is established on the grounds of the epistemology of critical realism and falsificationism. Within this epistemology, it is believed that scientific procedures of validation assist in separating facts from values -- though never completely, and that nature and animals may be observed objectively -- as objects existing independently from our subjective representations of them.

In Birke's biology, the idea that life forms change and evolve is precisely a discovery of mainstream biology and is grounded in critical realism: these changes are intrinsic to the object studied, to nature, but epistemologically independent of biologists' political will. I would therefore suggest that it is the subject matter of Birke's biological work, namely animals, which prevents her from more readily challenging the idea of empirical falsificationism as part of her biological epistemology. When she begins to address the specific question of human behaviour with her proposition of an integrative approach, she must engage more directly in the question of whether the epistemology, value-goals and theory-building of the natural sciences may apply to the social sciences. And this would have constituted, I contend, a great impetus to challenge the epistemology of falsificationism in human biology and questions about women, rather than stopping at the level of biological concepts and interpretations in her attempt to elaborate a fully-fledged new approach to biology based on feminist values and political aims.

Birke makes the claim that science should be accountable to society for the validation of its theories. There are indeed several levels at which 'society' could validate scientific theories, but these levels are primarily moral and political. These involve, for instance, decisions about research priorities, trials for and implementation of new technologies of reproduction, and debates about the importance of studying 'sex differences' rather than 'similarities', to repeat some of Birke's examples. At other levels however, like the activities of experimental testing and theoretical thinking, to name but two crucial facets of the scientific process, "the participation of a variety of people" and "of the community", as Birke suggests, does not make much sense. I would contend, therefore, that given these qualifications, the concept of 'social validation' of scientific theories is a gross simplification of the substantive processes involved in producing valid scientific knowledge. I would also add that this claim is simply not consistent with Birke's epistemological views. She seems to agree that biological explanations ought to be justified under the rules of falsificationism. Thence, 'social' validation cannot be an appropriate social channel for validation, unless the lay public is relatively well aware of the experimental and theoretical intricacies of the problem at hand, which is unlikely.

The social conditions of validation might be different for the human studies however. First, because there might not be any experimental data available (to either experts or lay public), and, secondly, because theory-building in human science is often grounded on immediate personal experience of, or familiarity with, the subject matter under study. The acceptance of the hermeneutic method for human and social studies precisely underlines, I would suggest, this possibility[30]. Indeed, and in my opinion, there may be cases for which 'social' validation would be welcome. These would have to be concerned with types of explanations which lend themselves precisely to the hermeneutic method. This implies that data relevant to the research problems under investigation are likely to be familiar to both the 'observer' and the subject being 'observed' (e.g. in biological research associated with clinical and health studies, research problems may stem from the symptoms reported by the subject and the environmental factors to which she/he has been exposed), both of whom are entitled to engage in a dialogue oriented towards an understanding of those very 'problems'. Having said that however, as far as human biology, health and behaviour are concerned, the hermeneutic method has its limitations. In human biology, one faces a hybrid type of epistemology rather than hermeneutics <u>stricto sensu</u>. That is, both experimental sense-data (regarding non-directly observable matter) <u>and</u> argumentative discourse about self-knowledge (regarding individuals' life conditions, intentions and actions) must be accounted for in the adoption of cogent epistemological rules justifying the explanations and theories developed thereof.

We saw in chapter 4 that teleology has been the object of constant controversy within biology. This is not to say that the notion of teleology is of any use in biological explanations; the reason for it being controversial is rather because it always directly raises debate in terms of social values and meanings -- and it does this more conspicuously in studies of animal and human behaviour, and in evolutionary theory. The meaning of natural selection in evolution, for example, is tainted with moral overtones, and the teleological systems underwritten by sociobiology seem tainted with political conservatism. The substantive content of the notion of teleology, without which no explanation exists, is likely to be laden with cultural meanings. Yet the notion of teleology, as philosophers have suggested, remains extremely useful as an epistemological category of explanation in biology (and, similarly, in the social sciences).

In the case of lower organisms, however, and also of animals, the specifications of the teleological systems do not directly relate to socially controversial issues and might, therefore, be accepted more readily. The value-assertions entering model-building are likely to be assessed on whether the model reflects reasonably (and ontologically) well our representations of lower organisms. The observational concepts these models would, in turn, predicate are likely to obey the validation rules relative to the pragmatic criterion. The gene-centred paradigm in evolution theory, for instance, is based on the idea of the individual selfishness of higher organisms which, it is argued, 'fits' the evolutionary dynamics of the ecological system[31].

In the case of higher organisms however, teleology tends to lose its predictive (or pragmatic) explanatory power because agreements on which specifications of the human (or social) system fit most adequately reality are far from being agreed upon. Indeed, goals and preferred states of social systems, which are the values that must precisely enter theory-building in disciplines dealing with human action and behaviour, are almost invariably the subjects of evaluative debates among scientific experts (and also non-experts). It is reasonable to think that on these matters, the content-value of teleological references will more directly reflect the power struggle about what social norms and order should ideally be. As the social norms are being challenged (by feminists or other social groups on the grounds that human beings are free agents and morally accountable to society), so are the teleological categories used in studies of human evolution, sociobiology, and the 'biology of behaviour'.

Thus, I would argue that, although all scientific judgements are conditioned by cultural representations, in animal biology (as this is mainly the case for Birke's work), theoretical divergences may more readily be resolved under the umbrella of the pragmatic criterion, whereas this might not be the case at all in studies of humans in which teleological values more directly reflect dominant social ideologies[32]. Indeed, this argument appeared implicitly in Birke's discourse:

> There is only a practical way to circumvent [ideology]: it is to minimise reductionism and its assumptions of objectivity as much as possible; for in a society based on reductionism, it is hard to imagine experiments done in a different way altogether: I cannot do without it, nor imagine using something else. It's a practical response to a problem we're aware of; it's a pragmatic approach. I think that this is exactly what it is. It's a practical response rather than saying, 'Well, there is no ideology'.[33]

In fact, right from the onset of her 'crusade' for a feminist successor biology, Birke's main goal has been to isolate the limitations of sociobiology as an explanation of behaviour, but without ever making any strong claim in favour of a revolutionary epistemology. In this regard, she wrote with her colleagues in the Brighton Women and Science Group that

> The claim that none of our singular causal statements about particular reasons and actions (including the true ones) is justified must be distinguished from the claim that the denial of each of them (again including the true ones) <u>is justified</u>. (Brighton Women and Science Group 1980, ix)

In other terms, Birke's representation of human life and biology is different from that of mainstream biologists. But although one might wish to consider this as <u>adumbrating</u> a conceptual shift (say, from biological reductionism to holism or dialectics), or a paradigmatic shift (say, from additive to integrative model of human biology), it is not convincingly implemented in a research practice. What is evident however, is that it does not predicate an epistemological shift (in terms of a new set of rules of justification for biological theories). In the end, Birke has always made sure she was following the same methodological and epistemological rules as mainstream biologists.

In closing this chapter, I should like to highlight one last aspect of Birke's project for a feminist biology: her argument in favour of the integration of other forms of knowledge in biological studies of women. As we shall see in our next case study, this claim is also being made by biologists in the feminist research group GRABIT. We noted earlier that Birke believes in the utility of alternative forms of knowledge such as individual introspection and collective consciousness-raising. I would suggest however, that her intention was mainly to defend these types of knowledge as being worthy of the attention of scientists, rather than to take them at their face-value and as valid explanations de facto. Birke would construe womens' partial knowledge (i.e. immediate knowledge of the senses) of their bodies and of their living experiences as biological facts to be explained at a deeper level. More precisely, she would see womens' partial knowledge of themselves as a 'bundle' of data to be reintroduced into a theoretical approach (the 'integrative approach') in which the interplay between both subjective experiences and biological processes would need to be explored, and about which only trained biologists are likely to understand, synthesise, and theorize. Unfortunately, having done most of her actual biological research in animal biology, Birke has not been able to put into practice these aspects of a feminist biology. But the research group GRABIT has made such an attempt, and rather successfully. We shall turn thus to an examination of its scientific work in the final chapter of this thesis.

Endnotes

1) Birke worked as a zoologist at the Open University from 1980 until 1989. She has since devoted her time to writing and teaching feminist studies in different establishments of higher education. It is a characteristic of the feminists advancing the idea of a feminist biology to be either non-scientists or else, to have abandoned scientific research per se. Until recently, Birke was still among the few feminist biologists attempting to introduce a feminist approach into biological research practice.

2) This review of Birke's scientific and parascientific works is selective rather than exhaustive. It has been difficult, in fact, to gather full details on her scientific and parascientific activities and publications. Apart from two long interviews conducted with the author (in July 1988 and March 1989), Birke seemed reluctant to give further information on her work.

3) Dr. Lynda Birke, interview by the author, tape recording, Milton Keynes, 30 March 1989.

4) As often mentioned by feminists, the terminology 'female' and 'male' hormones gives the impression of a deep gulf between the hormonal make-up of both sexes. This, it is argued, perpetuates segregation and the ideology of discrimination. Birke uses the term 'female' hormones with the same reservations.

5) As a Radical Scientist, Birke is aware of the authority she may, as an expert, have on the lay public. But she thinks her way of pursuing science is more likely to benefit than to hamper womens' interests or those of minority groups.

6) Herself a lesbian, Birke said she has never been the target of outright derogatory remarks from her colleagues.

7) See note 3.

8) Birke refers here to the divide between approaches within biology, but also to the lack of dialogue between biologists and non-biologists who do not hold the same views on life and nature.

9) As she indicated in an interview (see note 3). See also Birke (1988).

10) See note 3.

11) Birke and Archer also retain the categories of passive and active, intrinsic and extrinsic, and specific and diverse forms of exploration. Passive exploration and active exploration refer respectively to attentional changes and active investigation. Intrinsic exploration and extrinsic exploration refer respectively to motivations of curiosity and to some other external drives. They also retain the notions of 'absolute novelty', where the environment is totally unfamiliar, and 'relative novelty', where a mix of familiar and unfamiliar stimuli are present. The former type of novelty would induce 'diversive' exploration while the latter would entail 'specific' exploration.

12) I should refer here to the idea developed by Dilthey suggesting that hermeneutic understanding is the result of a constant re-interpretation (a 'hermeutical circle') of individual propositions ('parts') in view of the larger cultural context ('wholes') in which they take place. See Bauman (1978), Giddens (1976) and Outhwaite (1975).

13) As a matter of fact, for several ethologists and psychologists, and Birke and Archer alike, the problem of novelty (and for that matter, agency) lies at the heart of the question of animal exploration. This makes me conclude that Birke and Archer's criticisms of the old stimulus-response approach are fairly standard and that, rather unsurprisingly, they fall short of solving the difficulties which lie ahead in the elaboration of a new research programme (or paradigm).

14) See note 3.

15) I should refer to chapter 1, especially to the books of Fisher (1980) and Reiter (1975).

16) But there is no consensus about the rightness of these new interpretations. A thesis like that of Hrdy (1981), for instance, although it may have found legitimacy within mainstream biology, does not, however, foster a consensus on either its substantive content nor its underlying assumptions. In an interview with the author (Cambridge, Massachussets, 8 May 1989), Ruth Hubbard, for instance, rejected the entire enterprise of sociobiology. See also her views in chapters 1 and 2.

17) See note 3.

18) See note 3.

19) In this vein, Birke draws, for instance, a parallel between organic development and a snowball rolling down a hill:

> As development proceeds... its 'choices' become more and more determined until it lands up in one specific location at the bottom. The hills in between the valleys comprise constraints on the system, such as physical constraints... I would argue that the development of an organism represents processes of complex interaction, processes in which the interacting parts become changed. But, at the same time, there are constraints upon those processes; it is quite clear that not all outcomes are possible. (Birke 1986, 101-3)

20) For this reason, when Birke claims that "our biology, however, does not determine anything" (Birke 1986, 106 ; my emphasis), one must link this readily with her political opposition to biological determinism (and its connotations with patriarchal and racist ideologies) rather than a scientific rejection of the existence of biological determinations of human behaviours altogether.

21) Epigenesis refers to explanations and models of evolutionary development. Waddington has proposed new models of epigenesis, more environmentally-oriented than strictly genetically-oriented. See chapter 4. Also Waddington (1969, 1974) and Maynard-Smith (1986).

22) I should also like to stress, as in chapter 4, that process-oriented biology has always been an important theoretical stance within the epistemology of biology, but that it has never successfully replaced reductionism and experimentalism as a method. It is reasonable to think that the approaches of holism and interactionism have highlighted the specific problems associated with the truth-content and validation of theories in the life sciences.

23) But she is not saying that symptoms may not be directly related to changes in the body's functions occurring during the cycle, as she writes:

> Brain waves (as measured by the electro- encephalogram) are affected by the cycle, so that epileptic fits are least likely between ovulation and

premenstruum, and most likely just before a period. Various other functions change, such as carbohydrate metabolism, thyroid function, mineral and water balance, resting temperature and sensitivity to smells. (Birke and Best 1980b, 98).

24) Birke (1984a) gives a powerful argument in this connection, relating to psychiatric treatment of women. Birke has also addressed the issue of lesbianism in the BWSG collective. In her reflection 'From Zero to Infinity. Scientific Views of Lesbians' (1980c) Birke again makes an anti-reductionist and anti-determinist stance against current biological knowledge. She disputes the idea of the 'unnaturalness' of lesbian preferences, on similar grounds as the case of the 'natural inferiority of women'. She denounces the ill-founded, though widely prescribed in medical practice, hormonal treatment of homosexuality. She stresses that Lesbians are doubly oppressed: because they are women, and because they represent a political weapon against men's control and access to women's bodies. Finally she warns against future backlash and the alternative answers that science will find to maintain lesbians under ideological and technological control.

25) This is consistent with the three other tenets of her project of feminist science: it ought to embark on understanding the role of cooperation rather than competition in evolution, a socialist issue; it ought to substantiate the idea that biology is experienced differently by different peoples, an upshot of the biological paradigm of dialectics biology-social meanings; and it ought to reconsider the use of animals in biology and foster harmony with nature.

26) See note 3.

27) See Passmore (1978), Medawar (1985), Barnes (1985), and also the debate between the biologists S. Rose and J. Watson in Rose and Appignanesi (1986).

28) I am not sure that assuming that science and society are linked, or that ideology and scientific thought are dialectically intertwined, exonerates the analyst from the obligation of clarifying conceptual lines and examining thoroughly into the magma of reality!

29) In a brief conversation (7 April 1989), Birke expressed her disappointments as regards the dearth of feminist trained in science or who have dropped out of it. She also said unabashedly that she was -- until recently -- the only 'feminist biologist' left in Britain, whereas most feminists who criticize science are "definitely not scientists".

30) I would refer to chapter 3 for a discussion on this matter, especially to the works of Habermas (1970, 1979), Hesse (1980a), Bernstein (1983), and Hekman (1986).

31) See Maynard-Smith (1986) who has written extensively on the subject. It must be stressed, however, that a great many biologists do not even agree that gene-centred theories fit the data. I should like to thank Dr. J. Masters, evolutionist, for pointing this out. Interview by the author, London, 4 August 1989.

32) I should also like to add that the machine metaphor (or 'structure' metaphor)

when it comes to explain, determine, and predict human behaviour, always makes our theories harsh and inhumane in face of our ideal of human nature, as noted by Gellner (1974). The problem with biology, and for that matter the human and social sciences, is precisely that: the ontology of the objects being studied in the life and human sciences seems to be quite alien with respect to the ideal cognitive theories these very disciplines invite researchers to take. In the same manner, one may find that, in feminist writings, there is also, in the last analysis, this kind of utilitarian hard-core faith in modern science and reliance on the pragmatic standpoint on knowledge so well described by Gellner and the philosopher Hesse (1980a).

33) See note 3.

CHAPTER 8

A STUDY OF FEMINIST PRACTICE IN BIOLOGY II: THE GROUPE DE RECHERCHE-ACTION EN BIOLOGIE DU TRAVAIL

The Groupe de recherche-action en biologie du travail (Group for Research-Action on Human Biology and Work, referred to as GRABIT hereafter) is representative of a biological practice oriented by feminism, but this time in human rather than animal biology. In contrast to Lynda Birke, whose scientific research was about animal behaviours, GRABIT is concerned with the health problems of workers. It is a relatively unique mainly-female group of biologists whose research agenda and approach to biology are inspired by feminist critiques. In fact, its scientific production is extensive, involving research in genetics, neurophysiology and ergonomics in connection with risk factors for industrial diseases. GRABIT advances an approach to human biology inspired by both the feminist critiques of science and the research-action approach to occupational health. These two concerns are central to the scientific agenda of the group.

Based on an examination of the conceptual framework and method GRABIT has developed over the past ten years, and of its research results, this chapter will assess in what ways the scientific approach of the group constitutes an original contribution of feminism to biology, and why it has succeeded in gaining some legitimacy vis-à-vis the scientific milieu.

We shall compare the research agendas, methods, and theoretical standpoints of GRABIT and of mainstream biologists, in order to highlight the chasms and points of convergence between the two approaches. This comparison will be followed by a discussion of what needs to be modified, epistemologically and politically, in order to apply the feminist critiques of science into real scientific practice. Two questions will be surveyed more particularly: the relationship between being a woman and doing feminist biology, and the scientific roles of listening and subjectivity. These questions have always been relevant to the issue of developing 'feminist biology' as a full-fledged scientific approach, and have also been salient features in the construction of a feminist approach at GRABIT.

In this chapter we shall defend the idea that GRABIT's practice of biology is original with respect to the conventional norms of investigation in biology and occupational health. Certain aspects of GRABIT's method of investigation may appear rather conventional if one examines them from the perspective of the sociology of labour. Yet GRABIT deserves the great credit of having successfully bridged the gaps between the research agendas and methods of two areas separated institutionally: the biological and the social sciences.

At the conceptual level, GRABIT has gained an understanding of the risk factors specific to women workers in the context of the sexual division of labour, rarely attained in the field of occupational health. The body of theories GRABIT works with is not entirely alien to the sociology and economy of labour of the past decade. Feminists had already made several renowned contributions to those two fields. But such a body of theories remains rather new in the biological sciences.

Finally, in contrast to mainstream scientists, GRABIT's directors consider as part of their tasks the extension of the benefits of their knowledge to the workers directly. They organize training programmes for the unions and are involved directly in the promotion of changes in working conditions.

Having said that however, the approach of GRABIT is not outstandingly revolutionary from the point of view of epistemology. It relies heavily on the usual methods of scientific proof in the empirical sciences -- including both the social and the natural sciences -- to gain its scientific legitimacy. Thus, in comparison to the extensive feminist literature about scientific knowledge, feminist biologists have made their contribution from within the confines of current epistemological canons rather than on entirely new cognitive bases.

Yet GRABIT deserves credit for having entrenched certain social concerns (about political minorities like women and workers) and concepts of social theory within a discipline which has, paradoxically, striven hard to become a 'hard science' producing results the truth-content of which is defended by is 'mainstream practitioners' as being free from social values. In this sense, GRABIT is certainly contributing -- but perhaps only half-wittingly -- to the development of a science more self-conscious of the function of values in human cognition in general, and in biological science in particular. In brief, GRABIT may be participating in the reassessment of the epistemological differences between the social and the natural sciences, doing so from inside the empirico-analytical sciences themselves, rather than from outside, in the usual manner of feminist social critics and hermeneuticians. Indeed, GRABIT has given evidence that an approach to biology and work acknowledging publicly its feminist goals and woman-centred standpoint -- i.e., acknowledging openly its value-ladenness -- is able to secure its legitimacy on the same epistemological canons of proof as any other empirical science.

This case study is based on one month's worth of direct observation among the members of GRABIT at UQAM (Université du Québec à Montréal) in May 1989[1]. In addition, individual interviews were set up with five women staff of the group: the two directors, the geneticist Karen Messing and the neurophysiologist Donna Mergler, and three principal researchers[2]. Several research students (all females) were also interviewed on an individual basis. Finally, a survey of the publications of the group completed the enquiry, including articles published in trade union periodicals, in feminist and other scientific journals, research reports, and published communications.

The Group and its Members: Social, Political and Institutional Affiliations

In a parallel with the case study of Lynda Birke, we shall introduce briefly the directors and other members of GRABIT. By doing so, it will be evidenced that the five women staff of the group may be considered as individuals who are, in general, politically and socially active. Even before they formed or joined the group, these women were militating in social groups. This gives weight to the argument that feminist biology is, first and foremost, the practice of scientists who are militant, of 'feminist biologists', rather than of women in general, the mere product of a 'feminine' style of practice and of 'feminine values'.

Without rejecting the argument that the spreading of feminist biology is benefitting from the increasing participation of women biologists -- an idea that feminists have always propounded --, the case study of GRABIT strongly suggests that feminist biology is historically rooted in the participation of politically-oriented women biologists. True, most women are practising biology along conventional norms. Some are even opposed vehemently to feminist biology, as shown in a previous chapter. In contrast,there are some men who are feminists and tend to practice biology along the same principles as feminists. All these aspects have been acknowledged in the feminist literature. Yet I shall suggest that it would be an error to dismiss the strong sociological and cognitive relationship between the gender of biologists and their contributions to feminist biology and the development of new scientific knowledge.

The case for being a woman and pursuing 'feminist biology' still holds strong. For instance, according to Messing, women have a more direct interest in promoting a better understanding of women's health problems; moreover, they have an edge on men because they have an immediate empirical understanding of women's bodies and living conditions.

The case of GRABIT tends to show that women biologists are more responsive than men to the problems and hypotheses addressed by feminist biology. Let us thus present the principal members of GRABIT and analyze their attitudes towards women's occupational health and feminist issues with a view to the question of the relationship between being a woman and engaging in feminist biology.

Karen Messing was born in Massachussets in 1943, in a middle-class family[3]. In the 1960s she went to Harvard and earned a bachelor degree in psychology. During that period she became uneasy with theories about woman's psychology and was influenced by new feminist writings. In 1969 she became a member of the Front de libération des femmes (Front of Liberation of Women). She decided to challenge the idea that women were scared of doing science by shifting from psychology to biology. She completed her graduate studies at McGill University in Montreal, specialising in genetics, and was awarded a Ph.D. in 1975. She returned to the United States for a year, pursuing post-doctoral research in plant genetics. Since 1976, she has held the position of professor[4] in the department of biological sciences at UQAM, to which she was attracted by her close friend, Donna Mergler. Her position in the professors' union of UQAM led her to involve herself as a member of the committee on women's working conditions in one of the major trade unions in Québec. With Dr. Mergler, she also set up (in the late 1970s) a new course in the department, Biologie et condition féminine (biology and woman's condition), which was well attended by women biologists and social scientists. In 1982, with Dr. Mergler, she founded GRABIT. Since the late 1960s Karen Messing has served as a resource person for women's groups, and also as a 'token feminist' on committees in trade unions, on public boards, and at UQAM.

Donna Mergler was born in Montreal in 1944. She studied physiology as an undergraduate and earned a Ph.D. in neuro-physiology from McGill University in 1973[5]. She taught biology and physiology in different colleges in Montreal until she was offered a post of physiologist in the department of biological sciences at UQAM in 1970, which she has held since. From 1982 to 1984 she was head of her department. In the late 1970s, she was asked to set up training courses for workers in occupational health. Since then, she has committed herself to gear her science to the condition of the working class. Donna Mergler has, from an early age, followed the example of her father who was a leading lawyer for trade unions. She has always been engaged in trade-unionism. She is a militant socialist herself.

The two directors of GRABIT lead "two careers" simultaneously: as social activists and as scientists. Politically and scientifically they have influenced each other over the years. As social militants and scientists, they are both very much involved in popularising biology and explaining its applications to women and workers. They have lobbied (successfully) for the entrance of more women in the department of biology. Also, with other women colleagues at UQAM, they founded, in 1976, the Groupe interdisciplinaire d'enseignement et de recherche féministes (Interdisciplinary Group for Feminist Education and Research). Their research projects evidence their strong commitment to the welfare of women and workers. Finally, they publish extensively in scientific journals and participate in international and national conferences.

Dr. Nicole Vézina was born in Montreal in 1953. She grew up in a working class family. Early on, she decided she wanted to do research on the occupational diseases and working conditions of women[6]. She studied ecology as an undergraduate at UQAM. She then embarked on a Masters' degree in physiology under the supervision of Dr. Mergler, which she completed in 1982. Her research on workers in a slaughterhouse was to launch a long and successful series of studies by GRABIT. She went to France in the mid-eighties and worked on a Doctorate in ergonomics at the Laboratoire de physiologie du travail et ergonomie, affiliated to the Université de Paris Nord. In 1987 she came back to Montreal where she has been working as a senior researcher for GRABIT since. Dr. Vézina has played a particularly important role in GRABIT. As an ergonomist closely related to the trade unions milieu, she has contributed to the elaboration of several research projects on trade unions' requests.

Suzanne Bélanger and Ana-Maria Seifert are the other two senior researchers at

GRABIT[7]. Ms. Bélanger was trained as a nurse in the early sixties. As a practising nurse, she has always been an active member of her trade union, organizing training sessions and sitting on committees of women's conditions. She met Dr. Messing while working as a union officer on a project for screening genetic mutations in women hospital workers. She joined GRABIT in the mid-eighties, and took the opportunity to undertake a Master's degree in physiology under the supervision of Dr. Mergler. Since 1987, she has worked closely with Dr. Mergler on her projects on the effects of solvent exposure in factories.

Ms. Seifert was born in Bolivia. As a student she was very active in social movements and politics. She entered medical school, but never finished her course. When she was still in her early twenties, she was imprisoned, then forced to leave her country. She arrived in Montreal in 1973 as a political refugee. She enrolled in biology at UQAM in the second half of the 1970s. There, she met Nicole Vézina. They participated in attempts to reform the biology curriculum. With other students, they initiated "La semaine de biologie" as an attempt to entrench societal concerns within the biological program. At about the same time, she started working with Dr. Messing and earned a Masters' degree in genetics under her supervision. Since 1987, she has worked closely with Dr. Messing on her human genetics projects, particularly on effects of exposure to ionizing radiation in hospitals.

In comparison with the five women staff, the graduate students at GRABIT do not show as strong a commitment to feminism or trade unionism. However, they share a scientific interest for workers' occupational health, and they also recognize the value of the biological approach developed within GRABIT.

Carole Brabant is in her late twenties. She is one of the two doctoral students at GRABIT[8] -- along with a young medical doctor who specialises in epidemiology. Brabant did her undergraduate studies and Masters's degree at UQAM, where she became interested in the courses on women and biology given by Drs. Messing and Mergler. Brabant considers herself committed to doing research on the working conditions of women. She has a good grasp of the underlying rationale of the approach developed at GRABIT. She is, according to many members of the group, very imaginative, articulate and single-minded. She has initiated, with her friend and colleague Sylvie Bédard, a study of the cardiac and physiological strains in women laundry workers. She has also collaborated on publications about the theoretical aspects of GRABIT's approach to women's biology and work. She appears to have been directly inspired by the approach of her directors at GRABIT.

Sylvie Bédard completed her Masters' degree in 1988 on the subject of thermal discomfort among women laundry workers[9]. She then enrolled in a course in occupational health at McGill University. But, as she recalls, the course was taught in the traditional way of doing biology: it was not connected enough to the reality of working conditions. Bédard has, like Brabant and Julie Courville also, a more feminist orientation than the other students at GRABIT.

As an undergraduate student, Julie Courville participated in a study of the working conditions in a clothing shop where there was only one female for ten male workers[10]. This study, under the supervision of Drs. Vézina and Messing, aimed at adapting the tasks of leather cutter, traditionally done by men, to the musculo-skeletal characteristics of the average woman. For her Master's degree, Courville has been doing similar kinds of field work in a garage of mechanical engineers. Courville has been very much influenced by the social and scientific concerns of Dr. Vézina. Yet when she was asked if, as a member of GRABIT, she also considered herself a feminist, Courville answered, after several hesitations, that she was not. Nevertheless, her director Messing believes that she understands very well the rationale of GRABIT's approach, and that she sees clearly the 'political games' of science that GRABIT is forced to confront head-on.

Ginette Plouffe is a mature student and was doing her Masters' degree on laboratory workers and backaches[11]. Her decision to join GRABIT in the early 1980s was in direct relation to her work experience. In the 1970s she was involved in the unionization of a laboratory where she was working as a technician. Like the majority of members at GRABIT, she recognizes the important social role that biologists can play.

Isabel Fortier is among the youngest students at GRABIT and was also working on a Masters' degree at the time of the study. She did not really considered herself feminist[12]. Nonetheless, her research work undeniably made her realize the valuable applications of biology to the conditions of workers in industry. She was convinced that the results produced by GRABIT's approach were not only as valid, but even better than other studies in the same area. Fortier recalled vividly a particular episode of her field work in a factory. She said she was totally shattered when she saw how rapid was the deterioration of the health of workers exposed to solvents in industry. This gave her a new perspective on the social problems her laboratory work was directly tackling. She did not fully acknowledge the political dimension of her research work however, mainly because she never had the chance to familiarise herself with the harsh negotiations between employers, trade unions, and researchers. Like Plouffe, her work did not put her in contact with the specific problem of women's biology and work.

There are several other students and associate members at GRABIT. Messing and Mergler suggested that several of them did not fully share their social commitment and social goals[13]. For instance, Messing said that one of her students who was inclined towards feminism before she joined the group, has not been able to integrate her feminist views with her scientific work. In contrast, she maintained that another of her students, reserved about her opinions on feminist issues, had nevertheless a much stronger view on both her scientific and social commitments within GRABIT.

Mergler for her part doubted that her students would be combative outside the group and without its immediate support. She wondered whether they would keep challenging traditional biology, occupational health, or epidemiology once they move on to other professional settings. Yet she believed that their passage will not have been worthless, giving them the tools and critical thinking of a research-action approach to occupational health.

One last point is worth mentioning before closing this section: the relative absence of men at GRABIT. At the time of the study, GRABIT numbered fifteen women, including research students; but only four men: an epidemiologist (the only male senior researcher of the group), two post-graduate students, and a contractual worker doing statistical work. There has been only one male student, Daniel Tierney, the only one at GRABIT to our knowledge, involved in a study of women's health problems. He studied the effects on low birth weight of hospital workers during pregnancy, and completed his Master's degree in 1988[14].

As Bélanger mentioned, it is not necessarily because of a conscious discrimination against males that there are so few men at GRABIT. She suggested that the strong feminist stance of the directors might have dissuaded men from joining the group in the first place, especially the young male students.

All the above remarks give weight to the thesis that women are more prone than men to get involved in projects about women's health occupation. However, the participation of some women -- and men -- biologists in GRABIT can be explained by the fact that the directors of the group have always had strong scientific credentials within their professional milieu, rather than in terms of the personal combativity of these individuals as feminists.

A Comparison Between Traditional Models and Research-Action Models in Biology and Occupational Health

The research-action model tries to fill the gaps in the medical model of occupational health. In the history of GRABIT, the research-action model has adumbrated the development of a feminist approach specific to the problems of women's biology and work. This approach challenges the traditional model of investigation in occupational health, the 'male-centred' model. In its request to become a WHO (World Health Organisation) collaborating centre for women's occupational health, the group wrote:

> The name GRABIT is derived from the group's research and training activities in occupational health: BIOLOGY and WORK refers to our research priorities, which center around biological alterations resulting from workplace situations. This emphasis on early biological changes stems from a preventive approach to workplace risks, focusing on indicators which reflect diminished well-being and constitute potential warning signs of developing illness. The RESEARCH-ACTION component refers to that aspect of our methodology whose objective is to respond to workers' needs and concerns by involving them in the entire research process, from problem formulation to workplace recommendations. This is facilitated through agreements between our university and the two major Quebec unions and womens' groups.[15]

This section will highlight the differences between, on the one hand, the research-action models developed at GRABIT and, on the other hand, the traditional models the former aim to replace.

Institutional background

As a research group affiliated to UQAM, a young french-speaking university totally supported by public funds, GRABIT subscribes to the special mandate (or "protocol") of the university to serve the people, set up programmes of teaching and research in community services, and assist social groups in a very direct way. A first cooperative programme was established with the two umbrella associations of labour organizations in Québec in 1976, the Protocole d'entente UQAM-CSN-FTQ. This programme gave the unions access to the human and physical resources of the university. It also enabled them to submit requests to the university involving training courses and research about health at work. Finally, it gave them some control over the elaboration and implementation of research projects. The university has more recently established programmes in conjunction with a federation of women's organizations. GRABIT contributes directly to those two programmes.

The association between scientists, workers, and women's groups forms the concrete infrastructure for the implementation of the research-action model developed at GRABIT. GRABIT has also had an important financial aid from the IRSST (Institut de Recherche en Santé et Sécurité du Travail), a research division of the Worker's Compensation Board where representatives of employers, workers and the government are sitting in equal numbers. These two alliances helped GRABIT to anchor more firmly within the institutional structure its research programme about health and security problems of women and workers.

One of the first requests the trade unions made to biologists at UQAM was for them to participate as resource persons in union-organized workshops. As Mergler put it, "It was through these workshops that we began learning to combine our academic knowledge with workers' concrete knowledge of their workplace and their health. It is here that we are becoming familiar with the working milieu". (1987a, 154)

The research-action model versus the medical model in occupational health

The research-action model was first considered in the early 1980s by Drs. Mergler and Messing, and their colleague, Dr. Luc Desnoyers. The model had already been developed in different forms by "progressive scientists" in different countries, most notably in Italy at the turn of the 1980s (Mergler 1987a). In 1982, Mergler and her colleagues were granted $75,000 for one year in order to develop a "biological approach adapted to the workplace" and to create a research team[16]. Mergler has since published several papers in trade union periodicals and in scientific journals expounding the research-action model[17].

The research-action model in occupational health is based on three main assumptions. First, research should aim at social change rather than being strictly academic; secondly, the workers should be integrated as active participants in all stages of research rather than being the passive objects of study; finally, the scientists should be conscious of, and acknowledge fully, their partiality in the process (Mergler 1987a)[18].

The underpinnings of the research-action model appear congenial to the WHO's definition of health -- released in 1975. This definition stresses that health is not only related to the innate abilities of individuals but also depends on their environment. Moreover, health should not only refer to the absence of illness but first and foremost to the well-being of individuals and to the conditions favourable to it. In those two respects, the medical model presents some flaws as a preventive approach to occupational health (1987b).

Mergler isolates several shortcomings of the medical model which the research-action may correct. She maintains that its fundamental shortcomings reside in the fact that it takes as its starting point individuals rather than the collectivity, and the notion of illness rather than that of well-being. Thereby, the medical model is only able to monitor the evolution of a disease ex post facto. That is to say, it only applies when a worker shows clear signs and symptoms of illness or invalidity, or when the onset of an ailment is obviously related to a specific cause or event. As a result, Mergler notes, for several cases of occupational diseases, the medical model will neglect prevention and interventions will come too late.

When risk factors are defined ex post facto, the probability that an individual can be cured of his or her disease is low. But more importantly, the investigation of the problem at its source is undermined. Mergler contends that the medical model rather than pointing to the work environment as the source of biological defects, tends to focus on and blame the victim (i.e. his or her genetic, physiological, or psycho-social defects). Mergler acknowledges that illnesses with a late onset are hard to monitor at an early stage and, therefore, to prevent -- especially when the factors that cause them are diffuse. But she argues that her research-action approach to occupational health, since it emphasizes prevention, could at least minimize some of the costs that the workers are paying for with their health.

The scientific rationale of her approach is the following: from the point of view of physiology, the organism of a worker in industry who is exposed daily to various stressors and substances, progressively lowers his level of resistance to further aggressors, even though there are not as yet any clear clinical signs of illness -- detected empirically by the observing analyst. It is the early symptoms (expressed subjectively by the worker) of a deterioration of the well-being of the workers that a research-action approach will be examining. It will work on developing experimental tests (with workers or in laboratory) relating biological signs (physiological, genetic, neurological) to the symptoms of a deterioration in the worker's health. These signs may not be linked to any specific pathologies in the first place and yet they are the likely precursors of more serious damage or illness.

A third shortcoming of the medical model is that it relies on expert knowledge alone. It does not attempt to integrate the knowledge of workers and to make good use of it. Workers are often the best judges of their own state of health and well-being, as Mergler says. They also are in a very good position to recognize the hazards in the plant or the factory. In this sense, they may contribute fruitfully to the screening of risk factors for their health. The research-action approach, in contrast to the medical model, makes a valuable use of the workers' knowledge in the screening of precursor symptoms and signs of illness.

Another disadvantage of the medical model is that it relies on expensive methods and measurement techniques, and on laboratory set-ups that bear no resemblance to the real setting of a factory. Even when measurements are taken in the factory, the analysis is usually done in a laboratory. It is reasonable to think, as the researchers of GRABIT argue, that the environmental samples collected on the shopfloor do not necessarily give a good indication of the variations in temperature, noise, or dust to which workers are exposed during a whole day shift. They also point out that experiments carried out in the laboratory are often based on 'normal' healthy subjects who have not suffered the stress of several hours on a conveyor belt, in an uncomfortable position, or under abnormal temperatures.

The research-action approach can make valuable use of several sophisticated measurement techniques: neuro-physiological tests which lasts hours and HPRT (a genetic test synthesizing the protein HPRT in cells to detect mutants from normal genes) are two examples. But it also complements laboratory tests with simple techniques of data collection, such as questionnaires and interviews with workers. Mergler, with the assistance of her students, has, for instance, developed tests and samples to measure the levels of deterioration in the vision and olfaction of workers exposed to neuro-toxic agents like solvents (de Grosbois and Mergler 1985; Fortier 1989; Mergler and de Grosbois 1984; Mergler, Bélanger, Blain, Vachon 1988). The information thus collected conveys a rich body of evidence on the real working conditions of these people.

The widespread use of questionnaires and interview protocols has enabled GRABIT to adjust certain of its research designs in accordance with the workers' better knowledge of the workplace. It has also permitted the introduction of variables concerning risk factors that would have been neglected if workers had not participated fully in the elaboration of the research design[19].

Finally, Mergler maintains that the medical model has missed out on the specific problems of women at work. She argues that the medical model has focused solely on the reproductive risks during pregnancy, which constitute a very small part of all the problems which women suffer at work. It has not taken into account the fact, shown in a great many studies, that a great number of women work in specific jobs which involve particular risks. For a research-action model, this fact implies that a new set of working conditions needs to be investigated, and that a new series of potential ailments ought to be identified.

This leads us directly to the presentation of the feminist approach to work and biology developed at GRABIT.

The feminist approach versus the male-centred model to biology and work

In several articles and communications aimed at feminist and women's groups, trade union committees, and left-wing groupings, Messing propounds the theoretical basis of the feminist approach to biology and work (Bédard, Brabant, Mergler, Messing 1986; Messing 1983b, 1984, 1986, 1990a, 1990b; Vézina and Messing 1985). In those papers she points out the dearth of studies dealing directly with women's health problems at work. She writes:

> When problems are not studied, they can't be documented. When they aren't documented, they have no official existence. Thus, workers, legislators and the public may think that women's jobs are safe, because no study has shown otherwise. (1990b, 3).

Messing also sheds light on the ill-founded assumptions and biased orientations of the traditional male-centred model of occupational health, exposing its limited applications as far as the working conditions of women are concerned. Let us now turn to her arguments.

The field of occupational health is generally based on the assumption that men have jobs and women are housewives or mothers. Hence, model-building in the field of biology at work rests essentially on the image of a male worker and on the description of health hazards in men's jobs (Messing 1990b). The appearance of studies about women at work goes back to the turn of the century, but it is only recently that a significant body of research has begun to emerge. According to Messing, this is due mainly to the efforts of women researchers; for only a few men have been interested in this type of problem thus far[20]. She also explains that, in the present social and scientific context, researchers must confront two important obstacles in order to introduce a feminist approach.

The first kind of difficulties encountered by feminist researchers is mainly scientific. They must convincingly argue their case for the replacement of certain analytical and empirical categories inferred from male-centred models, and which have been applied without qualification to women's jobs. These categories and models are well entrenched in the field of occupational health, mainly because they are based on the strong belief that female jobs are not as damaging or dangerous for health as men's jobs. In this sense, the introduction of feminist model-building meets the usual difficulties of having to challenge theoretical traditions and established paradigms.

Challenging traditional models in science also implies in the case of GRABIT to justify its methodological tenets. GRABIT argues that the usual experimental designs and techniques of investigation of occupational health are not the most appropriate for a sound screening of the health problems specific to women's jobs. For instance, health hazards in men's jobs are more often very obvious and related to a very precise job characteristic: manipulation of heavy loads, exposure to extreme temperatures or to large amounts of dangerous chemical substances, manoeuvring with dangerous machinery. These job characteristics also fulfil the methodological conditions for testing causal models in epidemiological studies.

In the case of women's jobs however, the health hazards are more heterogeneous (i.e., the source of an ailment or of an illness is usually manifold). For instance, in jobs like tellers and sewers, two "job ghettos" for women, the worker must make thousands of rapid and repetitive movements, in a noisy environment, and must deal with the clients or meet the quotas of production. These various sources of stress may not be life-threatening individually; but incrementally, they constitute risk factors for the mental and physical health of the workers. And one must also take into account the additional stress these women perhaps have at home caring for their respective households.

For instance, several of the grocery tellers interviewed by GRABIT manifested symptoms of backaches and headaches (Courville 1989; Vézina and Courville 1989); while sewers complained of acute pains in their shoulders, and also in their fingers, wrists and upper backs (Vézina, Courville, Tissot 1988). The persistence of these symptoms led to belief that an inflammation in the tissues of the muscles and the joints was present, thus indicating that the jobs of these women involved risk factors -- including factors of mental stress -- for their health. Health hazards in a typical woman's job are not as often immediately life-threatening as in men's jobs. But this should not minimize the importance of the risk (for both their mental and physical health) that women may expose themselves to in the short or long run.

The second type of difficulty encountered by the approach to woman's biology at work is related to the study of women entering traditional male jobs. Researchers have, in this case, to make the proper methodological and theoretical adjustments, and no less importantly, to justify their research programme ideologically, politically, and economically, to employers, employees, and fellow scientists.

For instance, one of the prime sources of resistance faced by researchers of GRABIT is the belief that women might never be able to occupy some of the traditional male jobs because those jobs require physical strength or other biological traits or capabilities 'normally' attributed to men. Messing and her colleagues believe that a good number of these differences can be minimized if one adapts the tasks, tools, equipment, and machines to the average body and musculo-skeletal capacities of women (Al-Aidroos 1981; Vézina and Messing 1985; Messing, Courville, Vézina 1989)[21]. They have done several ergonomic studies suggesting specific modifications of the tasks involved in men's jobs so as to suit the musculo-skeletal make-up of women and, by the same token, to prevent health problems among men. Their study of a clothing shop for instance, demonstrated that using the right tools and introducing adjustable equipment not only helped the female cutter to improve her efficiency, but also alleviated the backaches the men had developed.

As Messing often points out, a notable percentage of Canadian women are taller and heavier than a good number of Canadian men. Differences in strength between the sexes are not absolute; there is some overlap. As a consequence, if employers could bear in mind that they are often misled by sexual stereotypes, they might become less prone to exclude indiscriminately all the women from certain jobs. This might also encourage them to pay more attention to the specific problems of weaker and shorter workers, females and males. But, as Messing also emphasizes, employers usually have several other reasons, other than strictly ergonomic, not to hire women for certain jobs. She also stresses that, in comparison to women, men will often prefer not to complain about their health problems and difficulties rather than to be perceived as 'weak', thus making it more difficult to assess their real health problems.

One must therefore realize that the insertion of women into male jobs commands more than ergonomic examination of the size and shape of equipment, and of the movements and postures of workers undertaking certain tasks. The success of this type of project also depends on coping with the social and psychological effects induced by the breaking of traditional male attitudes in the workplace. As GRABIT's researchers recognize, it is because of their research-action approach that they are now aware of the social problems related to occupational health. For it is a principle of this approach to ask and allow workers to discuss all the potential problems at work.

Moreover, and as Messing frequently stresses, the fact that researchers at GRABIT are mainly female makes them more sensitive than male researchers to the problems women face at work.

Finally, researchers at GRABIT have become conscious that important managerial interests are at stake by virtue of their research programme. They know that employers might be most reluctant to support the type of research they are doing for they would fear additional requests and safety measures from the unions, or that productivity will decrease if a woman takes up a man's job -- and vice-versa[22].

Interestingly, the experience at GRABIT seems to demonstrate that employers are usually reluctant to let GRABIT's researchers enter their firm primarily because they are known to be supported by trade unions, rather than for anti-feminist reasons. Thus, GRABIT's feminist orientations might appear less threatening than its union affiliations. Having said that however, the predicament of GRABIT as a feminist research group still exists: the arrogance of funding bodies, the hostility of fellow scientists, and last but not least, the impotence of the male-dominated trade unions are there to prove that point. But the hostility and arrogance of the scientific milieu seems to have decreased over the years[23]. This is correlated with the quality of GRABIT's work, participation in conferences, and publications in national and international journals. We shall return to this issue later in the chapter.

Let us now examine more exhaustively the shortcomings of the 'male-centred' model as a framework for the examination of the safety and health problems of women at work.

Messing pinpoints several basic flaws. Firstly, the male-centred model is disconnected from the reality of a gendered division of labour and oblivious of the fact that a majority of women have jobs that bear little resemblance to male jobs. She contradicts the proponents of the traditional male-centred approach who hold that women's jobs are safe and physically not demanding, and therefore do not involve any potentially acute health hazards. She strongly opposes the assumption that women's frail biology is the explanation of their ailments (Al-Aidroos 1981; Messing 1982).

The assumptions of the male-centred model have indeed been refuted by ergonomic and physiological studies done by GRABIT and research groups (Messing 1990b). Those studies have shown that, in the industrial sector, "female jobs" generally involve constrained postures, sitting or standing without moving, and fast pace movements of the upper limbs without any time to rest. In the service sector, studies have indicated that jobs like waitressing, nursing, teaching or that of receptionist, require a response to the need of the public, and thus involve a significant mental load. In other types of traditionally female jobs like those of typists, tellers, and assembly-line workers, rapid hand movements are characteristic. These also involve specific musculo-skeletal demands that bear no resemblance to male jobs.

A feminist research-action model would take into account all the traits typical of women's jobs in an investigation of women's occupational health. In addition, it would take into account all the 'environmental' aspects (such as mental stress of fast pace work, client pressure, routine work; in addition to specific conditions such as low salary, temporary employment, and sexual harassment) affecting the well-being of women at work. Finally, it would draw attention to household responsibilities that might add their adverse effects on health to those of the paid tasks. The feminist approach thus gives rise to a new array of issues for empirical investigation, hitherto unexplored in surveys based on the male-centred model.

On the methodological level however, there is some trouble with the woman-centred model. Messing fully acknowledges this kind of predicament. She admits that it is not easy to assess a "decrease" in the "well-being" of female workers by means of conventional diagnostic tools.

For example, to this day, studies of "collective" symptoms of discomfort, ailments and depression, observed more frequently among women workers in women-dominated milieu, have usually been explained in terms of a mass hysteria (Brabant, Mergler and Messing 1990). In general, diagnosis of ill-health starts with the symptoms, which are then tested against signs observed on the patient and may also be followed up by laboratory tests. But diagnosis rarely relies on the subjective reports of workers alone. It was pointed out earlier that in several cases of ailments associated with typical women's jobs, individual signs cannot be detected as such. The symptoms reported by the workers as a group thus become the best indications of a (latent) reduction in the well-being of the individuals. Moreover, these reports happen to be most fruitful in the screening of aggressors, sources of stress and risk factors. Indeed, Messing believes that to listen to these workers is fundamental both methodologically and heuristically.

In a recent essay (1990a), Messing defended her woman-centred approach by arguing that as a scientist and feminist committed to research-action, she has benefitted scientifically not only from listening to women workers, but also from listening to them 'with empathy'. Her study of the effects on health of ionizing radiation in hospital work has evidenced that listening to the women technicians made her pay attention to certain aspects of their work that she would have neglected otherwise (e.g., taking into account their workload, the descriptions of their tasks, and the specific times when additional exposure to ionizing radiation occurred, instead of taking dosimeter measurements at certain times only), to sensibly modify her observation design, and to interpret some of her data. She wrote,

> We think a place should be found in occupational health research for documentation and for statistical description of workers' perceptions. So we find ourselves making such obvious statements as, 'Workers report that they work too quickly, and that the more quickly they work, the more they feel exhausted' ... We think that when a large number of women are exhausted, scientists should listen to them. We also know that it's obvious that women shouldn't work so hard because they'll get tired has no influence on work schedules in factories or hospitals. When scientists say the same thing, it may have a greater chance of resulting in a change in working conditions. (Messing 1990a)

As Messing stresses, in general, scientists opt for a conventional clinical model of detection of ill-health, rather than for controversial methods bearing controversial interpretation and yet more appropriate to pinpoint the complexity of the problem under study. She says,

> Researchers are often asked to examine health symptoms. Scientific problems arise from the fact that symptoms are so often detected first by the worker herself, and the most reliable, inexpensive and efficient measures are subjective. The science of doing research on subjectively-perceived symptoms is not yet well-developed. Many people do not have confidence that the worker with symptoms will not, for example, over-report previous exposures to dangerous working conditions. (1990b, 10)

She also contends that symptoms like the mental workload reported by women workers are often neglected by researchers on the grounds of prejudice: "it's all in the mind", as their male-centred model would assume. And this is where the male-centred model is basically flawed. For, as Messing explains, most women occupy jobs where various sources of mental and physical stress are at work, thus suggesting that a holistic view on the risk factors should be favoured at the expense of simple causal models, looking at single risk factors, and using the accepted, conventional methods of validation using laboratory tests and focusing on observable signs. She writes,

> Most women work in situations where they are more likely to suffer from combinations of conditions than any one aggressor. They work where there are small numbers of workers exposed to any one set of conditions. Relating working conditions to health effects may be complex and require the use of many techniques. It is probably reasonable to say that if many women in

> a workplace suffer from a symptom, there is likely to be a problem in the work environment. (1989, 11)

Mental stress is more common in women's descriptions of their jobs than men's. In typical women's jobs, the reaction of the body to the aggressors in the workplace is "global". This makes it "difficult to relate working conditions to illness, unless there is a single, overwhelming aggressor with a well-defined effect which is not normally encountered [outside these jobs]" (Messing 1990b, 9). For instance, lifting light weights repetitively when the workplace is hot might increase the strains on the heart in laundry workers. In the clothing industry, the effect of repetitive work at fast speed when at piecework rates of pay raises the psychological and physiological stress already associated with the task.

Not content to give up in face of 'methodologically controversial' research designs, Messing stresses that several studies have perfected the tools and concepts measuring perceptions of mental aggressors in the workplace. Moreover, she maintains that the most reliable and reproducible existing measurements of mental workload actually are subjective and based on the worker's report of their degrees of difficulty with diverse aspects of the job[24].

To summarize, in contrast to a traditional male-centred model, a feminist approach would counterpoise a woman-centred model, since typical women's jobs bear specific health risks that bear little resemblance to typical men's jobs. These risks are associated with, for instance, speed, client pressure, and mental workload. They are not due ultimately to women's alleged frail biology. As a result, a feminist approach would assume that the ailments reported by women in these jobs are real. Also, in contrast to a conventional cause-effect model of explanation, a woman-centred model would make good use of interview and questionnaire techniques. Finally, and not least importantly, the feminist approach would examine women's occupational health from a holistic perspective rather than in terms of linear cause-effect relationship.

The feminist approach of GRABIT does not neglect the fact that individual traits may contribute to a person's susceptibility to diseases. Personal characteristics such as age, smoking habit, socio-economic status and genetic make-up are all taken into account to predict the likelihood of an individual becoming ill. Taking this into account, one should however realize that the diffuse demands (and negative effects) of the workplace and of the tasks may combine, in a "synergic" way, with the personal traits of individuals already at risk, and, thus, increase the likelihood of diseases and health damages. Domestic duties, menses, pregnancy, smoking habits, are intervening variables which must be considered when discussing health risks and the evolution of symptoms.

Let us now look at three of the studies done by GRABIT in order to illustrate the originality of its contribution to occupational health. This should afford pragmatic validity to the argument that GRABIT has provided 'revolutionary' results to the field of occupational health by means of its feminist research-action approach.

Three Studies of GRABIT on Women's Biology and Work

The three studies presented in this section concern three topical issues in which GRABIT is interested. The first study concerns workers in abattoirs and compares the health problems of men and women. The second study is about laundry workers, and points to the typical health risks of women's jobs which pass unnoticed when traditional models of occupational health are employed. The last study investigates the musculo-skeletal and ergonomic features of a traditionally perceived male job in order to adapt it for women workers.

The study in abattoirs was the first generated by the UQAM and trade unions' agreement (Vézina, Mergler, Beauvais, Everell 1980). The paucity of data on the work conditions in slaughterhouses and their effects on health problems prompted the trade union to request that such a study be carried out[25].

The bulk of the study was conducted by means of self-administered questionnaires distributed in ten abattoirs over a period of twelve months in 1979-1980. 55.3 percent of the workers (276 women and 385 men) answered the questionnaire, giving a reasonably representative sample of the population. In spite of the fact that direct observation on the shopfloor was not allowed by the employer, the researchers had the opportunity to visit the shops a few times and to discuss and revise the research design with the workers and union officers at other times. The final version of the questionnaire was the result of five pre-tests. It was used to measure work conditions such as levels of noise and temperature, quality of the air and level of ammonia (in the refrigeration rooms), speed of the conveyor belt, worker's movements, the levels of reported ailments, aches, sore limbs, and accidents, and finally the levels of problems reported by men compared to women workers, especially those allegedly related to menses. In the latter case, the responses of women workers were compared with those of the spouses of male workers who did not work in slaughterhouses.

The data of the study was computerized and analyzed. Three series of results have been published successively since the first general report was presented to the workers in 1980. The first series surveyed the comparative problems reported by male and female workers (Mergler and Vézina 1981, 1982; Mergler, Everell, Desbiens, Geoffroy 1984; Mergler, Vézina, Brabant 1985; Mergler, Brabant, Vézina, Messing 1987). The second series concerned warts on the hands of workers manipulating poultry meat (Mergler, Vézina, Beauvais 1982; Vézina and Mergler 1983). The third one dealt specifically with dysmenorrhea and cold exposure (Mergler and Vézina 1985; Vézina, Mergler, Beauvais and Everell 1980).

The first series of results is particularly interesting for it aimed precisely at disproving the "myth that women's occupations are not harmful to health" (Mergler, Vézina, Brabant 1985, 19). The analysis showed that of the eighty-three symptoms surveyed, women workers reported a significantly higher preponderance for 35.5 percent of these, compared to only 3.9 percent by the men. However, when the results were adjusted to the types of jobs done by the individual (i.e., comparing men and women reporting similar sets of working conditions, such as very cold and humid exposure, standing and immobile position, repetitive work with knife or scissors), the health symptoms reported in those groups revealed no significant -- or else greatly diminished -- differences between women and men (Mergler, Vézina, Brabant 1985; Mergler, Brabant, Vézina, Messing 1987). The study showed that the sharp sexual division of labour in the food-processing industry was a crucial factor in the explanation of the differences in symptoms reported by men and women. Hence, by using a woman-centred model of investigation, GRABIT's researchers were led first to verify if women and men were doing similar jobs in similar working conditions, and secondly, to control the symptoms reported by the task descriptions and environmental variables. Their results showed that men and women doing similar jobs reported significantly similar symptoms of stress, and of pains in the upper back and in the upper and lower limbs. In brief, GRABIT's feminist approach and woman-centred model contributed to a full assessment of the role of working conditions on the differences in symptoms reported by men and women. It also seriously undermined the male-centred model and its assumption of a 'biology of the weaker sex'[26].

The second series of results was highlighted thanks to the active participation of workers in the study. Meat cutters reported mild abrasions on the skin of their hands due to non-fitting steel-mesh protective gloves in conjunction with using blunt bladed knives in uncomfortable positions[27]. This data helped reveal one of the explanations why workers developed warts. Researchers were aware that the Papilloma virus responsible for warts could only propagate via the traces of meat-juice and fat to which workers are constantly exposed on the shopfloor, and that it could only develop in the sub-cutaneous layers of the skin. Drawing their attention to the flaws in the equipment and to superficial cuts and abrasions on the hands of workers was the key to a sound explanation of the problems of warts. Without the workers' knowledge of their working conditions, the link between biological knowledge about Papilloma and clinical signs (warts) would not have been made (Mergler, Vézina, Beauvais 1982; Vézina and Mergler 1983).

The third series of results concerning dysmenorrhea (i.e., irregularity and abnormal pains during menstruations) and cold exposure in the workplace responded to a lack of research on the subject. GRABIT's results disproved the idea that women are unusually sensitive to pain when they have their periods. The results showed, in the first place, that 10 percent of the women exposed to cold temperatures were incapacitated by dysmenorrhea and took sick leave during their prior menstrual cycle; and that this proportion increased in correlation with increasingly cold temperature (Mergler and Vézina 1985). Secondly, the factors commonly known to reduce dysmenorrhea (e.g., oral contraceptive, age, regular cycle) did not apply in the case of the workers exposed to cold temperatures. Pain in the regions of the lower limbs, the stomach, the kidneys, and also headaches were widely reported by all the female workers exposed to cold work conditions (Vézina, Mergler, Beauvais, Everell 1980). Finally, it was shown that the pain was not as acute among the control group formed by the spouses of the male workers who did not work in the slaughterhouses.

In the second study, the problems of women mangle workers in the laundry industry, a typically female job, were investigated. As the researchers of GRABIT emphasize, despite the well-known fact that the relationship between ambient temperature and thermal discomfort varies greatly according to the situation at hand, the majority of studies on the subject are still done in laboratories because measurements of metabolic heat load are easier to make in this setting (Bédard, Brabant, Mergler 1987). GRABIT's study therefore aimed at mustering information on the effects of heat exposure in the workplace. More specifically, it tried to provide more data on the real effects of high temperature levels otherwise considered normal for a type of work designated as "light' as is the case for mangle workers who happened to report physical discomfort in their jobs.

The results of the women mangle workers study indicated that detailed measurements of all the movements, postures, and cardiac strains during a full day's work suggest that mangle work ought to be classified as 'moderate' rather than 'light' work, and that the regulations governing the levels of normal temperature be lowered accordingly

(Bédard and Brabant 1989; Brabant, Bédard, Mergler 1986a, 1986b, 1987, 1988a, 1988b). The data for this study was collected on eleven women manglers over two periods of three days during the summer of 1985 and the winter of 1986, totalling 66 worker/days. (Access to the shopfloor was gained in spite of reluctance on the part of the management.) Feelings of discomfort were measured by systematic questioning of the workers and were "indicative of a coherent pattern of physiological responses" (Bédard, Brabant, Mergler 1987). Other individual items of information (e.g. age, smoking habits) were also collected by means of a questionnaire. Descriptions of the job's tasks were gathered by means of direct observation. Finally, portable sensors connected to a small computerized data storage apparatus were attached to the mangle workers during their whole seven hour day shift in order to measure their energetic output and cardiac rhythm.

The results showed that, although each item of laundry manipulated individually did not amount to lifting a heavy load, the accumulated weight of linen manipulated per day (taking into account different types of manoeuvre) amounted to 700 kilograms. The other characteristics of the jobs, such as a static posture with use of the upper limbs, torsions of the whole body, and speed of movement required to feed the mangles contributed to heighten the energetic output of the workers. Finally, the combination of this physical -- and mental -- workload and the thermal ambience on the shopfloor increased the workers' global energetic output even more. Those results were cast into a new light when readings of the cardiac strains were done at diverse times during the day, and in different seasons. The cardiac rhythm was much higher in the second half of the day shift, and in the summer season, even though the physical workload itself did not vary (Bédard, Brabant, Mergler 1987; Brabant, Bédard, Mergler 1988a, 1988b).

Interestingly, early on during their study, the researchers had already pointed out that the observed heart rates for these women laundry workers resembled those reported for persons, usually males, working in heavy industries (Brabant, Bédard, Mergler 1986a). After several analyses, they finally concluded that the working conditions were actually damaging the health of these women workers in a specific way. They hoped therefore that their results would help in ameliorating the working conditions accordingly, and stimulate intervention in the workplace:

> Identification of the most strenuous phases of the work activity will provide the basis for recommendations in this workplace. Long term consideration of the findings of this study should initiate rethinking of heat exposure standards in order to prevent more adequately excessive cardiac strain, which affects well-being, possibly accounts for discomfort, and may have long term chronic effects. (Brabant, Bédard, Mergler 1988b, 240).

The third type of study is an investigation of the ergonomic aspects of a typical male job in order to adapt it for a woman: the cases of a cutter in a clothing shop (Courville 1987) and of mechanics in a diesel engines shop (Courville, Vézina, Messing 1989). The first project was developed in response to the request of the trade union's "committee on woman's working conditions" (Comité de la condition féminine). There was only one woman occupying this job in the company; in fact, she was the only woman in the whole union. This woman, as was stressed in GRABIT's report, wished to get the job in order to increase her salary[28]. The second project took place in a company of 1500 workers where only three women were employed. The actual study was conducted in a shop where one woman and ten men were doing the same job.

The study in the clothing shop was conducted in the fall of 1987 and the first report presented to the union in December of the same year. The methodology included direct observations (including the taking of photographs for the study of the bio-mechanical aspects of the job's tasks), interviews, and measurements. The results suggested that given a few modifications in the equipment and the positioning of pieces of equipment, the female worker was able to accomplish her job satisfactorily. For instance, using the scissors instead of a small saw increased control over and efficiency in her work. Also, the introduction of a platform placed in front of the table she worked at and on which she could stand gave her an optimum posture and rendered her work easier. It also reduced the strains on her upper back. Finally, a most interesting finding revealed that minor modifications in the equipment, such as, for example, the adjusting of the height of the working tables, or the introduction of adjustable platforms, could also greatly decrease the levels of backaches reported by the male workers.

The study conducted in the engine shop was similar in design. The work activities of the woman were directly compared to those of her team mates, including the "operational modes" (i.e., postures, manoeuvres, muscular effort). The woman accomplished the same manoeuvres as her team mate, in spite of being much shorter (168 centimetres v 185) and weighing half as much (57 kilograms v 104).

The results showed that the woman was among those workers who found the tasks difficult. But as researchers learned that the laying out of the working post was adjusted to the average height of the two teammates, they deduced that it was too high for the woman, and that, as a result, she was compelled to work her arms in abduction, an operational mode which is rather taxing. GRABIT's researchers are now preparing to investigate which kind of adjustments in the tasks, the laying-out of the work post, and the tools would minimize the difficulties encountered by the woman mechanic and some of her male colleagues.

Feminist Biology at GRABIT: Convergence with, and Resistance from, Mainstream Biologists

In the previous sections, it was shown that GRABIT has contributed a new conceptual framework to the field of occupational health. This has, in turn, made GRABIT opt for a set of methodological tools in order to pinpoint a number of important variables explaining health problems at work which conventional models and methods did not apprehend.

A discussion of three other types of scientific issues will help clarify the reasons why GRABIT's research programme has incurred some resistance, or alternatively, earned the approval of some of its peers in the scientific milieu. Some of these issues are plainly institutional, relating to the politics of funding research. The others deal more directly with methodological norms of practice in biology and the epistemological canons of validation in the empirico-analytical sciences, and the points of rupture between 'mainstream biology' and 'feminist biology'.

The financial and institutional restraints

The financial situation of GRABIT has been healthy for the past five years, and grant money has always come in regularly. However, it is noteworthy that, within its research agenda, the field studies concerned more directly with the workers has almost always been underfinanced by funding bodies.

Messing has compiled statistics on the rate of success of GRABIT's grant applications (1988). She has pinned down two major tendencies in the policy of public funding towards its research-action projects. The first tendency shows that public funds systematically finance the laboratory aspects of GRABIT's projects, but turn down its requests for sponsorship for the 'worker aspects'. She demonstrates that on a total of twenty-eight applications made by GRABIT since its creation, 100% of all fifteen proposals for laboratory studies were financed, compared to only 25% -- or three out of the twelve proposals -- for field studies with workers. The second tendency shows that when Mergler or Messing applied for a grant in collaboration with a male colleague, it was usually sponsored (i.e., eight successful applications out of nine). In contrast, when they did not put in the names of any male collaborators on their demands and were the sole supervisors of the projects being submitted, their applications were successful at a rate of only 42% (i.e., eight successful applications out of nineteen).

It could be argued that, because they have always openly shown their feminist orientations, GRABIT's directors have had to suffer political discrimination from financing bodies. But this conclusion would need to be qualified by the fact that GRABIT's 'worker-oriented' research-action is much more threatening to employers and the financial establishment than its feminist orientation. As mentioned earlier, several senior members of the group have suggested that it is primarily its link with the trade unions that hampers GRABIT from getting research money from financing agencies and other sources of sponsorship.

Especially in the fields of occupational health and ergonomics where medical doctors and engineers are closely associated with the employers' point of view, the industrial and medical establishments clearly are antagonistic to GRABIT. As Vézina mentioned, it has often happened to her to be sent in to court as a counter-expert having to confront the engineers sent in to defend management's policy against the grievances of the trade union. Researchers at GRABIT have also often raised the problems of gaining access to shopfloors for their field studies; while several also maintained that once they gained access, they had to suffer systematic obstruction on the part of the management[29].

It seems therefore that engaging in feminist research is, in the particular case of GRABIT, less damaging politically than doing worker-oriented or worse, 'socialist research'. As Seifert contended, the antagonism to the worker-oriented approach is especially conspicuous on the part of funding bodies or plant management, and less so against the woman-centred orientation.

Yet within the scientific community itself, the feminist label seems to be highly resented, especially by men. Bélanger pointed out that male students are not at all attracted to the seminars given by Drs. Messing and Mergler about women and biology. Messing and Mergler also stressed that their male -- and some of their female -- colleagues have either been oblivious to, or vehemently opposed to, any rigorous feminist research agenda in the biology department. In addition, Messing indicated that she has always had colleagues denigrating indiscriminately all the projects of GRABIT and attacking its scientific credentials. Thus, if one takes these events into account, the feminist commitment of GRABIT's directors seems to create a climate of resentment and obstruction within the scientific milieu. But it also seems reasonable to think that their scientific credentials have not necessarily suffered a systematic rejection. On the contrary, GRABIT seems to have gained respect relatively quickly, within very few years.

The methodological issues

In the arena of methodology, three issues might be raised successively in connection with the points of rupture and convergence between mainstream biology and GRABIT's feminist biology. First, the epistemological canons of scientific proof in the field of epidemiology and the methodological norms of scientificity in the biological sciences more generally tend to undermine the value of the qualitative approach used at GRABIT. Secondly, the epistemological status of "listening to the subjects" of study, known to be the nexus of the new method propounded in feminist biology, is not so much 'revolutionary' in epistemology in general as controversial in clinical research, and in the life sciences in particular. Having said that, it seems that the credibility of feminist social theories of labour in economics and sociology have given a non-negligible legitimacy to feminist occupational health and biology at work.

As mentioned previously, there are certain problems with the methodological tenets of a research-action model. Firstly, the results are often based on a small sample of data, usually the workers in one factory. In epidemiology, the results of a study are considered controversial if they rely on too small a sample, for in a small sample, control for factors that might potentially be responsible for the actual disease is not always possible (Fletcher 1986; Messing 1990b).

The second difficulty with the research-action model is that it often relies on interviews and the subjective reports of individuals. The advantages of using this type of data collection were shown in previous sections. But it also bears disadvantages of which researchers at GRABIT are fully aware. Subjective reports of workers may, for instance, comprise unduly biases induced by anxiety and imperfect memory; by a lack of interest;

by ignorance of the possible dangers. These might distort the facts, thus concurring with scientific misinterpretation of the results. The likelihood of self-selection of respondents in health-related studies is another well-known predicament menacing accurate diagnosis. Finally, the omission of crucial events by those subjects who have little to lose by reporting incompletely their personal health history is likely to distort the results of a study.

Considering that some aspects of GRABIT's approach are genuinely controversial, it is not uninteresting to note that GRABIT's students are less at ease than the senior staff to accept or defend the methodological tenets of a research-action model at the expense of the more conventional methodological norms followed by epidemiologists. Several students indeed admitted having problems justifying their results in the eyes of other biologists. At a conference attended by professionals in health research (where the author was a witness) for instance, a student was asked how she could justify the causal links between data based on two sets of subjective reports[30]. The student was unable to answer satisfactorily. But Seifert, who was present at the conference, defended the validity of the study by arguing that there is a coherence in the patterns of responses obtained with current medical knowledge.

In brief, the logic of justification that underwrites GRABIT's research methodology runs as follows. First, symptoms of discomfort and health problems should be investigated at their source, that is in the plant or the factory. Some of these sources may be obvious and easily identified. But others are manifold, constitutive of a synergic ensemble of causes. They are only identifiable via a holistic and contextual understanding of the work situation. Difficulties arising out of studies involving symptoms of mental stress should not be avoided because there are no obvious signs that can help orient the investigation. Reliance on the subjective reports of workers are even more important and worthy in these cases. All these arguments should justify the choice of working on a small sample rather than on a large-scale sample as is usually the case in epidemiology. The usage of interview techniques, finally, should be justified on the following grounds. This technique offers the possibility of detecting important information otherwise neglected by the researcher who is not familiar with the work conditions specific to the shopfloor under study. The reliability of the data collected by this technique is tested against the presence of a pattern of coherence in the responses obtained with accepted medical knowledge[31].

I should like to suggest, at this stage, that the methodological arguments raised by GRABIT in favour of its techniques of investigation might sound familiar to any social scientists engaged in empirical research. Indeed, the interview techniques and the use of questionnaires are widely accepted in the social sciences and human studies. The former is used as a most efficient device in the exploration of new fields of research, such as in GRABIT's research, the study of a new industry or a new workplace. The latter offers the grid for a quantifiable analysis, generally accepted in the social sciences as a reasonable basis for the validation of results. It seems therefore that the techniques of investigation used at GRABIT are not fundamentally new from the point of view of epistemology. That is to say, it is nothing but an accepted norm of scientific practice in the 'soft sciences', although it still provokes controversies in clinical and 'hard' sciences. It might be 'shocking' to the community of biologists to have to resort to a method borrowed from the 'soft' social sciences, because biology and epidemiology are two fields which are now considered more or less on a par with other 'hard sciences' precisely because they have finally managed to integrate, after so many years, the 'best' of the 'scientific method' to their inquiry of human beings[32].

It has often been argued by the directors of GRABIT that the method of listening constitutes the foundation of a new kind of research that would herald a biology more favourable to women (Mergler 1983; Messing 1986, 1990a). The notion of 'listening', as shown previously, is not only at the nexus of the research-action model as such. It lies at the core of a feminist approach to knowledge that has been suggested to entrench the feminine -- rather than the masculine -- values within the scientific method and to renew, on this basis, scientific epistemology in the natural sciences. Although this view has now been revised, even by those who advanced it some years ago (see Jaggar and Bordo 1989; Keller 1987b; Kirkup 1986; Mura 1989; H. Rose 1987; Stacey 1988), the importance of 'listening' still looms large in the arguments supporting the idea of a feminist biology.

For instance, one might ask to what extent do researchers listen to people and workers? Are their opinions taken into consideration at all stages of the research process? Or else, are they only considered as input data and not really taken into account in the design of research and in the interpretation of results? Is the 'art of listening' to living organisms only the prerogative of women biologists? Is it epistemologically reasonable to argue for an approach of 'listening' when one is dealing with genes, viruses, and toxines rather than with human beings? Although it is not possible to answer all these questions in the context of this thesis, some qualifications may be made concerning the strengths and limitations of the method of listening for the development of feminist biology.

The idea that a "method of listening" is central to, and specific of, feminist science is being contended within GRABIT itself. Especially among the senior researchers, there have been discussions about the scope and real impact of such a method. The utility of a "method of listening", it is true, seems obvious in the case of studies where interviews are used extensively to assess the health problems and malaise of workers. However, even in this case, the "subjective reports" of workers are bound to be validated on other "more objective" grounds, such as measurements done with instruments or a "pattern of coherence of responses" with accepted medical knowledge. In greater contrast, as in the case of the investigation of the effects of the working conditions on some genetic defects, it becomes even more obvious that the use of "objective measurements" is more important than the "subjective reports" of workers, even though the latter may be, in some contexts, the only yardstick for "measurements" of the working conditions under study. The limitations of the (interactive) "method of listening" thus become clearer. In general, its utility, which is genuine, does not trespass the first step of the research process, at the stage of exploring -- or measuring -- an ensemble of working conditions.

Yet, in spite of its limited utility in certain areas of biological study, the interactive method and the technique of 'listening' cannot be questioned as to its utility in the process of discovery. Its relevancy in the logic of discovery is not being challenged; what is being questioned is its relevancy at the stages of formalizing and validating results in biological studies. For biology is not an arena where the ontological and epistemological conditions of the application of the hermeneutical method are present in the first place. These are rather superseded by the pragmatic criterion governing the empirico-analytical sciences.

In a nutshell, the potency of the 'method' of 'listening' may be conceived of in terms of a renewed attempt to use various techniques of data collection in clinical studies. If it is still likely to be considered as a controversial yet rational methodological stance in human biology, it can hardly be considered as an epistemological basis for the validation of biological knowledge, and feminist biologist would agree to this.

In fact, it seems that the combination of technique of listening and sociological concepts borrowed from feminist theories of labour is truly congenial to the development of new research designs and hypotheses in occupational health. Moreover, this combination seems even more congenial to a sound understanding of women workers' health problems. However, the data mustered via these technique and concepts remain, in the end, subject to the traditional canons of validation of the emiprico-analytical sciences; and it is the convergence towards these scientific rules, or 'mega-norms' of practice that might have helped GRABIT to justify and legitimize its feminist research programme.

I should like to suggest that the strength and originality of feminist biology at GRABIT lies precisely in its relatively successful attempt to establish a bridgehead between 'methods' and concepts in the sociology of labour and the biological sciences with a view to the elaboration of a woman-centred research-action programme in occupational health. As an instance of feminist biology, GRABIT has contributed to biology in a substantive way. It has introduced the theoretical assumptions about gendered representations of human nature, behaviour, and labour in a discipline hitherto oblivious to these; and it has implemented new concepts, research agenda and findings to the field of occupational health.

Feminist scientists, women scientists, and feminist biology

The main proponent of a feminist biology at GRABIT is Dr. Messing. As the co-director of the group, she has the opportunity to orient its research agenda according to feminist concerns. She also contributes greatly to shaping the image of GRABIT within and outside the scientific community. This is not to say that feminist biology is just a matter of discourse. On the contrary, this chapter has shown clearly that GRABIT's research practice really consists in an original contribution of feminism to the field of biology and work.

Thus, to capture in which ways women, feminists and non-feminists, might have contributed to the development of feminist biology, one has to examine the motivations underlying why the biologists at GRABIT, staff and students, have opted to follow in Messing and Mergler's steps.

In this last section I should like to make some clarifications about the relationship between being a woman and doing feminist research, and stress that this relationship should not be conceived of as a synchronic causal chain but rather as a diachronic unfolding of scientific thinking and social interests.

Let us first try to disengage from the idea -- very controversial within feminism itself -- that the caring attitude and the method of listening is the causal link between being a woman and engaging in feminist science. This argument is not only flawed epistemologically; it is also sociologically shallow: for it is spurious to categorize men and women within another rigid dichotomy that does not even represent the milieu of biological practitioners accurately[33]. Finally, as women climb higher in the scientific hierarchy, they tend to use their authority in no 'healthier' a manner than the men. In this connection, several young female students interviewed for this research indeed commented on having very unpleasant relationships with their female supervisors[34].

It was pointed out in the first section of this chapter that only a few members of GRABIT were directly involved in feminist movements. Moreover, younger students at GRABIT tended to avoid the feminist label, and did not necessarily consider themselves feminists. Nevertheless, they were all <u>interested in doing research ABOUT women</u>. That was their main motivation in coming to GRABIT. At GRABIT, these young researchers became familiar with feminist theories of labour and the feminist-oriented research-action model. Their 'socialization' as scientists was thereby built on the basis of feminist concerns. They worked with a framework of feminist theoretical concepts. They utilized methods that, although rather conventional from the point of view of the social sciences, are not generally accepted within biology and epidemiology. But they were as yet unexposed to the politics of the institution.

What will these students do after they leave GRABIT for other more conventional research centres or laboratories? The answer to this question is crucial for the development of feminist biology. But at the moment one can only answer this question by looking at tendencies. In this respect, one must take into account that these young people will have internalized a research approach and research 'interests' that are the basis of the future development of feminist biology and work. But it must be stressed also that a political awareness of the ideological and economic rationale of the biological institution is still, in the present social context, necessary for those who wish to follow the pioneers.

At present, the agenda of a feminist research-action is likely to be overshadowed by other research priorities of the biological, financial, and industrial establishments. If it seems reasonable to think that feminist research in biology and work is not the greatest threat to conventional biological research, although it may be menacing for the economic interests of industry. Certainly research on women's health is taken into more serious consideration than one or two generations ago; or compared to a century ago when it was almost totally dismissed. Having said that, even though some important scientific battles have been successfully fought by feminist biologists in the past, there are others that still need to be fought in order to put questions of concern to women and feminists higher on the biological research agenda.

As the case study of GRABIT has shown, Dr. Messing has been the main proponent of a feminist approach on behalf of the group. She has had the total support of Dr. Mergler in her undertaking. Together, they have worked on the integration of their feminist critique into a research-action approach to biology and work. The latter approach gave the former research priority a relatively legitimate scientific basis.

Nevertheless, through the years, Messing and Mergler have had to deflect the hostility of some of their peers and, most importantly, of the industrial establishment. As Messing explained, "listening" to the workers in occupational health is dangerous to the class system; and for this reason it may have been easier to reach a political consensus on the necessity of a "woman-centred" model than on a "research-action" approach.

All the above arguments suggest that the relationship between feminist biology and women biologists should be understood in terms of an historical unfolding rather than in terms of synchronic causal links. That is to say, the evidence mustered in this thesis suggests that a feminist biology is a full-fledged scientific practice informed by feminist social critiques, woman-centred analytical categories, and 'empathy' with women's life experiences. This historical unfolding suggests, in turn, that other biologists will participate in feminist biology inasmuch as its scientific values converge minimally with the canons of validation of empirical knowledge, but also on the condition that practising biologists develop and maintain a minimum of commitment to feminist political goals. And throughout this historical process, the social interests and research concerns of women will certainly make them participate more than men to the development of feminist biology.

Endnotes

1) The collaboration of the members of GRABIT was outstanding, given the circumstances: the group was busy preparing papers and organizing a special colloquium on women and occupational health as part of the annual Symposium of the ACFAS, the French Canadian association for the advancement of science. The two directors were, as always, very busy and could not always spare the time necessary to discuss the questions relevant to the present research. (A second trip to Montreal was made in January 1990 during which several discussions with Karen Messing completed the information on this case study.) On the other hand, Suzanne Bélanger, Ana-Maria Seifert, and Carole Brabant, were very reliable informants and gave steady and valuable assistance. Overall, their attitude was very forthcoming.

2) GRABIT also collaborates on special projects with other staff in the department of biological sciences at UQAM, and a few other biologists outside the university. If we include these people, nearly thirty researchers (over 80% female) -- including students -- form the 'extended GRABIT'. In the present study, only the core of the group was taken in consideration, that is, the staff paid from a grant of the Institut de Recherche en Santé et Sécurité au Travail (IRSST) and the research students under the direct supervision of Messing or Mergler. This accounted for eighteen researchers and one full-time secretary, whose competence was often praised by GRABIT's members, and whose involvement in the group extended beyond secretarial work.

3) For a succinct portrait of Karen Messing, see Y. Villedieu. 1988. 'Karen Messing: Faire de la science pour changer des choses'. In Interface, 9, 1 (janvier): 8-10.

4) In the academic hierarchy in the province of Québec, there are four categories of teaching staff, but each bears the title of professor.

5) For a short biography of Donna Mergler, see A. Gotheil. 1988. Les Juifs Progressistes du Québec. Montréal: Ed. Par Ailleurs.

6) Interview with Dr. Nicole Vézina, Montreal, 2 June 1989.

7) Interview with Suzanne L.-Bélanger, Longueil, 19 June 1989; interview with Ana-Maria Seifert, Montreal, 30-31 May 1989.

8) Interview with Carole Brabant, Montreal, 30 May 1989. The Ph.D. programme in environmental sciences was only recently instituted at UQAM, in 1988.

9) Interview with Sylvie Bédard, Montreal, 18 May 1989.

10) Interview with Julie Courville, Montreal, 2 June 1989.

11) Interview with Ginette Plouffe, Montreal, 25 May 1989.

12) Interview with Isabel Fortier, Montreal, 25 May 1989.

13) Interview with Dr. Karen Messing, Montreal, 1 June 1989, and 4, 12 and 16 January 1990; interview of Dr. Mergler, Montreal 16 May 1989.

14) Daniel Tierney, Karen Messing and Donna Mergler were the only three anglophones of the group, but were all fluent in French, which is the language of communication within GRABIT.

15) Application to the WHO/PAHO, April 1989. Still being negotiated in January 1990.

16) In 1985, after the departure of Dr. Desnoyers, Mergler and Messing initiated the GRABIT. The team was sponsored for two years by the IRSST and UQAM. In 1987 the IRSST gave another substantial grant covering all the functioning expenses of the group for a period of three years. At the time of the study, the grant had not been renewed, which worried several researchers.

17) D. Mergler and Q. Samak. 1987. La santé au travail: une approche médicale ou approche préventive? In *Colloque international sur la santé du monde*, Confédération des Syndicats Nationaux, 35p.; D. Mergler. 1981. *Recherche épidémiologique en milieu de travail*. Colloque sur la recherche et l'action communautaire. Québec.; D. Mergler. 1987. Workers' Participation in occupational health research: Theory and practice. In *Journal of health services*, 17: 151-67.

18) Mergler emphasises that scientists are not necessarily altruistic professionals. They too gain their fair share of benefits in the success of a research project on workers.

19) I refer here to the study of Vézina et al. presented further in this chapter about the development of warts on the hands of workers in poultry slaughterhouses. It was the workers who drew the attention of researchers to the possible source of infection. The study of Messing et al. on hospital workers is also quoted very often. See K. Messing et al. 1987. Union-initiated research in genetic effects of workplace agents. In *Alternatives: Perspectives on technology, environment and society*, 15: 14-18.

20) Messing points out that it is mainly women who engaged in research on women's health at work. Like several feminists, Messing believes that women have a fundamental role to play in the promotion of feminist occupational health and biology.

21) Interestingly, Messing used to think that the problem of women not being able to enter traditionally male jobs was merely ideological. But she was led to think otherwise subsequent to several discussions with her colleague ergonomists and physiologists at GRABIT, and she was prompted to examine such problems in terms of the real biological limitations of women undertaking certain traditionally male jobs.

22) The fact that trade unions are male dominated also implies similar impediments for the implementation of feminist research in occupational health. The possibility that salaries will decrease if women are allowed in traditionally male jobs is not a small hurdle

for feminists and women who have to face the inertia of officers in the trade union movements. See: H. David 1986. Inégalité en emploi. Rapport de recherche. Montreal: IRAT.

23) Messing recalled several episodes where her colleagues or those of Drs. Mergler and Vézina demonstrated blatant hostility or annoyance vis-à-vis their feminist commitment. In contrast, I witnessed genuine appreciation of Dr. Mergler's approach from members of the ABQ, Québec's association of biologists, during the Symposium of ACFAS in May 1989.

24) See for instance Moray, N. 1988. Mental workload since 1979. In International reviews of ergonomics, 2: 123-50. See also Fletcher (1986) who examines methodological problems linked to the utilisation of small samples and subjective reports of patients in epidemiological studies.

25) That Dr. Mergler had contacted officers in the trade unions direct also played a role in this 'choice' of research topic.

26) In this example, the hypothesis that women are usually less reluctant than men to report the same level of symptom was qualified on the grounds that women's excess reporting was also correlated with their reporting of a faster work speed in the working conditions.

27) This example is certainly the piece of evidence most frequently quoted by the members of GRABIT to vindicate the utility of their research-action model.

28) Lower salaries are one of the many characteristics of female jobs 'ghettos'. At GRABIT this aspect of women's jobs is often mentioned as affecting the well-being of workers.

29) This is without consideration of the problems dealing with the trade unions themselves which are sometimes unable -- or unwilling -- to support GRABIT. See Mergler 1987a; Vézina et al. 1980.

30) The study concerned hospital workers who complained of abnormal pains during their periods. The dissension came with reference to the causal link drawn by the researcher between hardship at work and levels of pains reported. The contention pertained to the circularity of the logic: is it not reasonable to think that the women who complained the most actually exaggerated the level of hardship in their work, thus inverting the actual causal link? The counter-argument to this would be the existence of a specific logic of pain associated with specific working conditions.

31) GRABIT is now developing statistical tools such as factorial and regression analysis (see Bédard, Brabant, Ferraris, Mergler 1989) which bear resemblance to the packages used by social scientists doing quantitative analysis.

32) Interestingly, we suggested to the members of GRABIT that they were actually using the 'soft' methods of social science in human biology. Those to whom this was

suggested seemed however reluctant to accept the idea. They preferred to say that they were using a method which had been used in biology for a long time already. We tend to believe that both positions are legitimate, and prove that the so-called 'new' method of 'listening', for example, is not so 'new'.

33) Some might retort that it is mainly the men who do dissections and 'awful' experiments on animals. But this does not seem at all confirmed by empirical evidence.

34) This is based on the interviews carried out with biologists in London, but also according to some students at GRABIT.

CONCLUSION

THE ORIGINALITY OF FEMINISTS' CONTRIBUTION TO BIOLOGY

This research has analyzed the Anglo-Saxon feminist critiques of natural science with a view to the creation and implementation of projects of 'feminist biological practice'. It has assessed the sociological and epistemological arguments upon which feminists have justified their criticisms of biology and natural science. It has suggested that, from the perspective of a sociology of knowledge, the argument that biology has built on male-centred and patriarchal theoretical assumptions and institutionalized gender-segregatory norms of practice is justified. Several cases of historical evidence may be cited to support this, especially those relating to evolution theory, gendered behaviours and women's health on the one hand, and the organization of laboratory work on the other.

From the outset, feminist critics set out to explore the natural sciences, and biological knowledge in particular, as representing the last (and certainly least obvious) bastions of patriarchy. Alternately, any of the feminist attempts to construct a new feminist science had to appeal to several sociological arguments. The most effective argument in that sense might come from an analysis of science as a system of knowledge vested with interests: the science system is assumed to orient the research agenda (i.e. to decide which research projects will be funded and realized) and to balance the methodological norms of testing theories in favour of certain social interests.

In this perspective, several feminists have argued that the establishment of a project of feminist biology would occur following political and institutional struggles for recognition, rather than strictly legitimized on 'scientific' credentials. We suspected, however, that this logic of argument, though strategically seminal, was self-deceptive. A research programme for biology which, like that defended by feminists, finds its primordial justification on a political basis, might be said to ultimately vindicate the feminist practice of science on the same grounds as patriarchal science. We suggested, therefore, that, to be consistent, the creation of a fully-fledged project of feminist biology must either find totally new grounds for legitimacy, or reappropriate the conventional epistemological rules of validation spelled out in critical realism. Only then would it be acceptable to isolate and discard patriarchal theories about womens' biology on the grounds that they are 'incorrect' and 'untrue'. Having said that, however, criticisms of biological theories may be justified on the basis of a theory which posits that scientific knowledge is purely political, discursive, and institutionally controlled.

As I tried to show in this thesis, as far as the norms of practice of science are concerned, the ideology of patriarchy and the feminist ideology are not totally discordant. It seems that feminist biologists rely, in the last analysis, on a number of methodological and explanatory categories (or normative values) that were generated within the empirico-analytical sciences themselves taking place within the patriarchal system. Consequently, the 'grand programme' -- of feminist critics of science -- of sustaining projects of feminist biology on a new epistemological basis has not been fulfilled. Instead, projects of content-oriented theory-building in some research areas of biology have been realized successfully by some feminist biologists as one of our case studies showed.

This thesis contended that there has been a genuine contribution of feminists to the development of biological research in certain areas of the discipline. Why has it been so that feminist biologists have so far only had a limited impact on the biological institution? What is the extent to which epistemological, institutional (i.e. the structure of the discipline) and political factors have affected the establishment of a fully-fledged feminist biology?

We suggested, first, that the project for a feminist epistemology of the natural sciences has relatively failed in comparison to a feminist theory-building in biology partly because of the special epistemological status of biology. Biological explanations may sometimes resemble models of explanation in social science, other times, models in physical science, without necessarily abandoning rational procedures of logical inference and canons of validation in the empirico-analytical sciences. In the same vein, debates regarding the role played by social values in biological theories have been more or less politicized depending on the research areas (for instance evolutionary theory in contrast to microbiology), that is to say whether theories comprise value-assertions that bear more resemblance with those of social science or, in contrast, with physics and chemistry. It is well-known that social values entering theory-building in the social sciences have spurred more internal controversies and public debates than the social values entering theory-building in the physical sciences; while the biological sciences seem to have been afflicted with 'normative' disputes more acutely than physics or chemistry, but less so than sociology or psychology, for instance.

Feminists have advanced their critique of the scientific norms of biology on the basis of a sociology of scientific knowledge. This standpoint assumes that both the epistemological canons and theoretical assumptions of science are ultimately entrenched within social values and norms of practice. The central argument of feminist theories of knowledge is that, in spite of their spurious universality and of their adverse impact on women, biological theories about sex and gender differences remain largely uncriticized. A change in the social norms of life, and in the scientific norms of practice according to feminist ideology and epistemology would therefore be amenable to more accurate theories about women, and, as a result, to the emancipation of women within society in general. A new feminist biology built on these premises would, however, become legitimate only as feminist ideology would become accepted more widely both in the public at large and by practising biologists.

In this thesis we have argued that the feminist criticisms of biology did not necessarily need a new epistemological or methodological basis to be vindicated, even though some of these criticisms logically invited to a redefinition of disciplinary barriers and methodological tenets in biology. As we have observed, 'mainstream biologists' do not generally find it incongruous to have to reconcile a conceptual framework of holism and a methodologically reductionist perspective. Alternatively, we noted that feminist biological theories could be scientifically legitimized, without having recourse to entirely new types of 'truth-claims', nor using radically different categories of explanation -- such as teleology, interactions between environment and organisms, holism, and determinism. Inasmuch as the new concepts and theories advanced by feminist biologists follow the epistemological rules for scientific proof of the empirico-analytical sciences (i.e., the criteria that demarcate science from pseudo-science, and the pragmatic criterion of validation), an important barrier of resistance to the idea of a 'feminist biology' is being eschewed. Our comparison of feminist practice of biology, and conventional discourse about biology has revealed these aspects of the actual implantation of projects of feminist biology, highlighting also some of the types of resistance mainstream biologists have manifested towards it.

This has led to the second argument of this thesis. Feminist biologists have had problems defending their new approach within the community of biologists for three main reasons. First, at the institutional level, they have had to break the disciplinary boundaries of human biology in order to integrate concepts and methods of investigation borrowed from the social sciences. Secondly, at the sociological level, they have had to resist the anti-feminist attitudes of the majority of fellow biologists, usually male, but also female. Finally, at the political level, they have had, as militants and politically motivated, to struggle against funding bodies in order to pursue their research programme and maintain their credibility within the institution of biology.

In this connection, we have tried to document the mode of participation of women biologists, and to explore their potential or actual role in a redefinition of the norms of practice and disciplinary structure of biological work. Although the role of women in scientific changes is problematic for feminists, both in practice and in theory, our results did not disprove the argument that a 'feminist biology' should be first and foremost grounded on the implication of women biologists and of certain forms of 'feminine values'. This must, however, be interpreted with three qualifications. First, the link between a feminist biology and feminine 'values' ought to be appreciated as a historical unfolding rather than as a strong causal relationship. That is to say, feminists may be the pioneers of a feminist biology, but it is mainly women who are expected to follow in their steps, and actually are the workers in 'gender-oriented' sectors of biological and medical research. Secondly, we must bear in mind that the arguments advanced in favour of a feminist critique of biology are primarily addressed at gender-related problems, and observational research with a high theoretical content, rather than at experimentally-based theories. This thus leaves room for the possibility that gender-based socialization might supersede professional-based socialization relating to the development of certain research problems, designs, and hypotheses. As shown in our empirical studies, the potential for women to develop new 'woman-centred' ways of conceptualising problems and interpreting results is justified both epistemologically and sociologically. Thirdly, if we accept the notion that a feminist biology is a 'successor' science (i.e. it is assumed to be in continuity rather than total rupture with current science), then it seems appropriate to appraise the project of a 'feminist-feminine' biology on the argument that biology and clinical studies may employ the hermeneutical method for its research purposes, and, in the last analysis, lend themselves to feminist reconceptualizations on the basis of women's (social and biological) life experiences and intersubjective understanding.

In brief, we shall argue that the contribution of feminist biologists to the field of biological research is genuine, but needs to be qualified as follows. First, feminist biology does not ultimately reject scientific realism. It remains committed, in the last instance, to the traditional epistemology of empirico-analytical sciences, the rules of scientific validation, and the pragmatic criterion in model-building. Yet it does challenge successfully mainstream biology, primarily via new operational concepts and research designs in bio-behavioural studies and clinically-oriented research about women. One must stress, however, that these research areas are closer to the social sciences, theoretically, methodologically and epistemologically, than other types of biological research which are, in contrast, closer to the physical sciences. Granted this, the limited impact of some projects of feminist biology, such as that of GRABIT, might more readily be explained by the constraints of an institutionalized disciplinary structure, and political resistance to feminist militants, than by epistemological inconsistencies. Feminist biologists, because they are forcefully committed to the welfare of women and an approach to biology largely inspired by social theories, concepts, explanatory categories, and techniques of investigation in human biology, seem to have suffered unduly the burden of political and institutional restraints, and frequently also, the verbal animosity of colleague scientists.

So we shall close this thesis by suggesting, with a view to the above arguments, some avenues for future research in the sociology of feminist biology. The case of GRABIT will, again, serve as an illustration.

The principal researchers at GRABIT have suffered very negative reactions on the part of their colleagues at UQAM and of peer scientists. Reactions of hostility, sometimes accompanied by intellectual detraction, have been commonplace. Indifference seems, however, to have been more widely spread. In spite of this, the group has benefitted from the knowledge and political support of trade unions and feminist groups particularly, and this has been very important for a continuity of its research undertakings. What is the articulation between the political and the scientific viewpoints of practising biologists towards feminists in biology; and in which way may this feature of the biological 'norms of practice' jeopardize the position of women scientists who might lean towards a 'feminist' practice of human biology? These questions should help to orient further research on the subject of feminism in biology.

Funding bodies and private industry shared a systematic distrust of GRABIT's research team: they perceived it as 'socialist' and biased in favour of the workers. GRABIT has nonetheless received several important grants since its foundation in 1982. Perhaps this ought to be put in light of the fact that GRABIT has always been persistent in its requests to gain access to companies' data and to the shopfloors, has many contacts in trade unions, and has constantly applied for research grants. Who are the 'allies' of feminist biologists within the institution of biology; and what kind of strategy ought feminists to develop in order to promote their research projects? These questions may also serve to orient fruitfully further research.

As regards the scientific disagreements about GRABIT's methodology, there have been debates both within and outside the group. Medical doctors and epidemiologists, for instance, seriously doubt the validity of a methodology focusing on the subjective reports of workers. In addition, young members of GRABIT do not necessarily understand the rationale of such a non-conventional method for biology. What kind of 'professional' orientations and career strategies the young biologists who are now being inspired by feminist biologists will take in the future? This question might inspire feminist students in the development of new research insights into knowledge systems in general, and biological practice in particular.

Finally, although the politics of biological research emerges as rather antagonistic to a feminist-oriented, soft-method based, 'women-centred' (and 'workers-friendly') research programme like that of GRABIT, yet the group has been funded regularly and, on some of its studies, obtained the entire collaboration of employers. It seems therefore that GRABIT has gained scientific credits relatively quickly for a group of feminist scientists often portrayed as 'revolutionary'. A leading question in future feminist research might, in this regard, be: How should we interpret this evidence from the perspective of changes in 'systems of knowledge'?

In our opinion, the current social and scientific context offers, with qualifications, the space for a feminist biology to exist. As the case study of GRABIT strongly suggested, projects of feminist biology may be decisively non-conventional (conceptually, methodologically and institutionally) and develop a substantial and genuinely original 'women-centred' research programme in human biology, even upon a re-appropriation of the 'conventional' epistemological canons of validation of the empirico-analytical sciences.

APPENDIX

DETAILS ON RESPONDENTS IN PILOT AND MAIN STUDIES

Respondents were selected and contacted along the following procedures. A list of the members of staff of all biology departments (and medical schools) at the University of London was prepared, using the directory of the Association of Commonwealth Universities, the Commonwealth Universities Yearbook 1987.

Seven units were selected, and the heads of departments, contacted direct or by mail. Access was given in six departments. Updating of lists of members of staff was done, and lists of research students was requested (four departments provided such lists). Selection of respondents was operated with a view to a reasonable representation of disciplines (i.e., zoology, genetics, medical research orientated) and age-groups (taking the status of respondents as an indicator); and to equal numbers of men and women.

Personalized letters were, finally, sent off -- roughly to twice as many respondents as our study's target number -- explaining the aims and scope of the research and describing the organization of the project. The percentage of positive responses was 57%.

Female Respondents in Pilot Study

No.	Status of Interviewee	Age	Civil Status/ n. of Children	Discipline
#1	Student	30	married/2	bio/neurochemistry
#2	Student	23	single	parasitology
#3	Lecturer	50	married	biochemistry/neurology
#4	Sr. Lecturer	48	single	physiology
#5	Sr. Lecturer	37	married/2	molecular genetics
#6	Sr. Lecturer	50	married/2	bio/neurochemistry
#7	Sr. Lecturer	48	married/2	pathology/cell biology
#8	Student	28	married	immunology
#9	Student	26	single	biochemistry
#10	Professor	58	married/3	parasitology
#11	Teacher	32	married/2	paleontology
#12	Teacher	31	single	microbiology
#13	Teacher	30	divorced/1	marine genetics
#14	Teacher	37	married	plant physiology

APPENDIX (end)

Female Respondents in Main Study

No.	Status of Interviewee	Age	Civil Status/ n. of children	Discipline
1	Post-Doc	35	married/1	molecular genetics
2	Res. Fellow	45	divorced/1	physio/neurobiology
3	Res. Fellow	61	married/2	zoology
4	Lecturer	32	single	plant genetics
5	Lecturer	34	married/1	molecular genetics
6	Student	32	married	mycology
7	Lecturer	44	single	microbiology/genetics
8	Post-Doc	29	married	pathology
9	Student	26	single	medical genetics
10	Student	23	single	molecular genetics
11	Post-Doc	34	married	neuroanatomy/mol. gen.
12	Lecturer	34	married/2	plant pathology
13	Res. Fellow	61	married/3	zoology/neurology
14	Res. Fellow	46	single	medical genetics
15	Res. Fellow	46	married	human genetics
16	Lecturer	50	single	zoology/genetics
17	Lecturer	45	married/2	zoology/physiology
18	Lecturer	35	single	zoology/ecology

Male Respondents in Main Study

No.	Status of Interviewee	Age	Civil Status/ n. of Children	Discipline
101	Sr. Lecturer	49	married/2	microbiology
102	Student	45	married	plant physio./ecology
103	Post-Doc	34	married	insect physiology
104	Lecturer	35	married	biochemistry/botany
105	Lecturer	33	single	population genetics
106	Lecturer	41	married	zoology/paleontology
107	Reader	44	single	population genetics
108	Lecturer	40	married/2	zoology
109	Student	32	single	insect physiology
110	Post-Doc	25	single	molecular genetics
111	Lecturer	36	married/2	biochemistry/genetics
112	Sr. Lecturer	57	single	animal behaviour
113	Student	27	single	molecular genetics

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