THE ‘VAMPIRES IN THE SACRISTY’

Feminist body theory and (socio)biological reductionism into the 21st century

Lynn Roarty

BA, CCAE; GradDipEd, University of Canberra;
BA (Hons), Murdoch University

This thesis is presented for the degree of
Doctor of Philosophy of Murdoch University
2009
Her suspicion that there existed a bedrock residuum of sex distinction played second fiddle, nonetheless, to her exasperation with the social overemphasis on sexual distinction.

(Cynthia Eagle Russett, speaking of Charlotte Perkins Gilman, 1989)
I declare that this thesis is my own account of my research and contains as its main content work that has not previously been submitted for a degree at any tertiary institution.

.................................................. ..............

Lynn Roarty
Abstract

What happens when feminist body theory and reductionist theories of biological sex difference are brought together? In this work I take as my starting point the increasing ubiquity of appeals to biology as an explanation for ‘human’ and ‘woman’s’ nature on the one hand, and the reactive and reflexive distancing of biology within feminist body theory on the other, to begin to question the middle ground. I aim to constructively dissent from taking up either of these positions in order to confront the question: what if the reductionists prove to be, even partially, right? In acknowledging that possibility, I am interested in whether/where there is potential for feminist theory to be more relaxed about biologically sex differentiated attributes.

I position myself as a women’s studies scholar taking a walk across the campus to see what evidence is being produced by ‘the opposition’. To place my walk in context, I first briefly explore various feminist approaches to the problem of biological sex differences, and the continuing difficulties surrounding binaries and binary thinking. Next, in the main part of the thesis, I review the historical and contemporary reasoning and claims made within three areas of reductionist science that are aligning at this time, and which have been reproached for promoting a return to a more biologically determinist social environment. I then take a brief excursion off campus to demonstrate the dangerous aspects of these scientific enterprises when their interpretation into popular culture is not carefully monitored. Finally, I return again to my own side of the campus to look at some of the ways feminists have already begun the work of overturning
outworn and contested conventional theories about biology and human nature in conversation with reductionist theory.

Having done this, was it worth the walk? My assessment is that while, in some cases, feminism’s defensive antiessentialism is warranted, there is work being undertaken within these reductionist sciences that is less rigid and reactionary than some critical interpretation would suggest. I conclude that there is a certain futility in feminist body theory’s oppositional stance to biology, and that its utility is put at risk by a continued investment in one side of a binary. Further, my walk across the campus leads me to believe that, while perhaps not imminent, there is every reason to expect that the scientific pursuit of an unequivocal genetic basis for specific sex differentiated behaviours will succeed. That being so, there are spaces where the insights of both sides might be productively brought together so as to avoid the worst excesses of biological determinism and, at the same time, loosen the grip of binary thinking on approaches to biology and the body.
# Table of contents

Abstract................................................................................................................ iv

Acknowledgements.............................................................................................viii

Acronyms .............................................................................................................ix

Introduction..........................................................................................................1

Part one : Skirting difference ..........................................................................15

1. Embracing nurture : the biosocial politics of difference ..............................16
   Introduction.................................................................................................... 16
   Metatheoretical approaches .................................................................. 22
   Sexual difference feminism – the ‘corporeal turn’............................... 26
   Challenging the science ....................................................................... 36
   Conclusion .............................................................................................. 60

Part two : The vampires in the sacristy ..........................................................63

2. Genetics, genomes and control : biology’s Holy Grail .................................64
   Introduction............................................................................................... 64
   Background to modern genetics......................................................... 69
   The Human Genome Project and its objectives................................. 73
   Outcomes and benefits....................................................................... 76
   Controversies........................................................................................ 82
   Ethical, legal & social issues.............................................................. 85
   Conclusion ............................................................................................. 99

3. The bent twig : cognitive sex differences ..................................................102
   Introduction............................................................................................... 102
   From antiquity ...................................................................................... 104
   The Victorians ..................................................................................... 107
   Modern times ........................................................................................ 116
   Conclusion .............................................................................................. 134

4. ‘Just so’ stories? : sociobiology as persistent paradigm ..............................138
   Introduction............................................................................................... 138
   The Biogram .......................................................................................... 147
   Genetic determinism ........................................................................... 152
   Genotype/phenotype interaction ....................................................... 154
   What is a gene? .................................................................................... 157
   Selfish replicators .............................................................................. 159
   Mechanisms of experience – human universals................................. 168
   Mechanisms of difference – sex differentiated behaviour ............... 176
   Conclusion .............................................................................................. 183
Acknowledgements

There are many people who have contributed, directly or indirectly, to the completion of this thesis.

First and foremost I want to thank my exceptional supervisor, Bev Thiele, for so many things: for intellectual generosity, insightfulness, and patient guidance and support; for challenging me to achieve clarity of thought and expression; and for her utmost care in engaging with my ideas and written work. Bev, I’m grateful beyond words for your unflagging enthusiasm, encouragement and friendship, and for hanging in there with me through to the other side of some pretty rough times.

All the other remarkable women who have helped me along this journey:

Sherry Saggers, who employed me, gently (okay, the occasional firm shove) mentored me into other areas of research, and offered friendship, wise counsel, and personal and practical support towards the completion of this thesis - and often managed to conceal her impatience at its long gestation.

Colleagues – in particular Meredith Green, Judith Cockram, Jenny Dodd and Lauren Breen – who also gave generously of their wisdom and warm friendship.

And the Jarrahdale regulars – Bev Murfin, Judy Durey, Larissa Sexton-Finck, Sally Knowles and Tracey Jones – for the wonderful friendships, camaraderie and work-in-progress support over the years; special thanks to Sally who does such a great job of organising our seasonal retreats.

Dear friends: Bev Murfin, for the countless long discussions and occasional disagreements over the years that have helped me to refine my thoughts and arguments, but most of all for being there every step of the way, through all the ups and downs; and Jan and Glynn, for cheering me on and for regularly providing a comfy, happy space where little successes could be celebrated, or the miseries of life could be set aside for a while.

And my family – especially Mum, Dad, brother Ian, uncle Ray, and mother-in-law Caris – for love and support.

Finally, my amazingly patient and long-suffering husband, Dave, who has always so generously given emotional, intellectual, practical and loving support. There would be no thesis without your steadfast belief and encouragement. I’m sorry this particular phase has taken so long - and I promise I won’t start on anything else for at least six months 😊.

And to Alan and Caris – still children when I began, moving into adulthood as I finish – maybe a small brick in the wall for you two, with my love.
Acronyms

AAAS  American Association for the Advancement of Science
CEER  Center of Excellence in ELSI Research
CIGHT Center for the Integration of Genetic Healthcare Technology
CIRGE  Center for Integration of Research on Genetics and Ethics
DNA  deoxyribonucleic acid
DOE  Department of Energy
EEA  environment of evolutionary adaptiveness
EELS  ethical, economic, legal & social issues
ELSI  ethical, legal and social issues
ESS  evolutionarily stable strategy
HGP  Human Genome Project
JHKCC Johns Hopkins Kimmel Cancer Center
LPCB  The London Project to Cure Blindness
NCI  National Cancer Institute
NHGRI  National Human Genome Research Institute
NH&MRC  National Health & Medical Research Council
NICHD  National Institute of Child Health and Human Development
NIH  National Institutes of Health
NINDS  National Institute of Neurological Disorders and Stroke
OED  Oxford English Dictionary
OHER  Office of Health and Environmental Research
OMIM  Online Mendelian Inheritance in Man
ORNL  Oak Ridge National Laboratory
SAT  Scholastic Aptitude Test
Introduction

[C]ulture and hence the unique qualities of the human species will make complete sense only when linked in causal explanation to the natural sciences. Biology in particular is the most proximate and hence relevant of the scientific disciplines. I know that such reductionism is not popular outside the natural sciences. To many scholars in the social sciences and humanities it is a vampire in the sacristy.  


What are the virtualities, the potentialities, within biological existence that enable cultural, social and historical forces to work with and actively transform that existence? How does biology – the structure and organisation of living systems – facilitate and make possible cultural existence and social change?  

Elizabeth Grosz (1999, p. 32)
Introduction

That old lemon, that mystical dichotomy, the nature-nurture problem, needs to be reconsidered. It seems to have more reality in the minds of men (sic) than in the world they see around them.

Glen McBride (1971, p. 37)

Rather than accusing science of essentialism and rejecting the role of the biological outright, it may prove more fruitful for feminism to theorize the “interimplication” of the biological and the social in attempts to understand sex differences in behavior.

Celia Roberts (2000, p. 1)

Reconsiderations of the nature/nurture problem have continued sporadically over the last four decades, but increased attention to genetic and other new technologies has provoked, since the early 1990s, a more sustained interest in the debate. Arguments about whether humans are primarily framed and motivated by biology or by culture, or by some (generally unequal) intermingling of the two, regularly appear in scholarly books and articles, and in the popular media. These arguments are themselves intricately tied to theories and beliefs about sex difference that, in turn, exert a timeless fascination, and continue to fuel opposing theories about the human body.

Body theory is a vigorously contested site, and sex difference has been variously conceptualised – often at one and the same time – as self-evident or mysterious, confronting or comforting, complex or simple, accepted or disputed,
acknowledged or silenced. Sex difference is a problem; and the many theories about how and why the sexes differ in their character and behaviour pose particular dilemmas for feminist theorists, because ‘the problem’ is most often framed in terms of women’s difference from men. This, as has been pointed out so many times, immediately places woman in the subordinate position within a dualistic framework, situating her as ‘other’ to man’s ‘subject’.

Whether dualistic thinking comes ‘naturally’, or is culturally imposed (a question which is itself an expression of dualistic thinking), it is intrinsic to the way we view the world. We tend to think, for example, that either it is dark, or it is light; either it is hot, or it is cold. For the most part, it seems unproblematic to think in this way, and it matters relatively little to our everyday lives that things are not usually so clearcut – that, for example, light seeps into the darkness, or a cool wind wafts through a hot day – since, individual preferences aside, neither pole of the (supposed) dualism is infused with any notion of a priori superiority over the other.

The biosocial politics of difference, however, is bedevilled by the seemingly intractable problem of the dualistic framework within which it is inevitably situated, because here we are most often confronted by two phenomena not, as it were, side by side, but in an hierarchical arrangement which eventually works to

---

1 As Simone de Beauvoir puts it: “She is defined and differentiated with reference to man and not he with reference to her; she is the incidental, the inessential as opposed to the essential. He is the Subject, he is the Absolute – she is the Other” (1988, first published 1949, p. 16). For an extended argument on the essentialism/social construction dualism, see Diana Fuss, Essentially Speaking (1989). Ecofeminist Val Plumwood also discusses the nature of dualisms at length in Feminism and the mastery of nature (1993), in particular chapters 2 & 5.
constrain any interrogation.\textsuperscript{2} Thus, where the everyday terms within a dualism are constructed as either male or female (as, for example, in Aristotle’s philosophy the capacity to generate greater heat is characterised as a superior, male, ability), then those terms associated with ‘female’ are invariably also constructed as inferior to those associated with ‘male’ (see Cixous, 1986; Gatens, 1996, p. 61), and this has a material effect on our lives. Simone de Beauvoir recognised this constraint when she noted that “The real problem is precisely to find out why woman should be defined with relation to man” (1988, first published 1949, p. 25). In turn, the positioning of women in this way, as ‘different’ to men, leads to the struggle to resolve the binary thinking inherent within the problem itself, since the concept of ‘difference’ necessarily invokes one or more ‘Others’.

As a result, feminism historically has had a very tense relationship with biology and with the disadvantageous implications of notions of biological difference for women. Despite the ‘post-feminist’ tenor of the times,\textsuperscript{3} and despite the many astute and creative ways in which feminists have theorised the body, the literature demonstrates how easily unpalatable (and at times unintended) political, social, and/or biological implications can attach to every approach. This has led, inevitably and understandably, to a feminist literature on biology and the body which, in general, tends to minimise or deny a role for biology in our

\textsuperscript{2} Especially, though not exclusively, of biology. Biology, of course, is not the only place where hierarchies within dualistic thinking are problematic; we need only think, for example, of the different values given to science/social science, empirical/theoretical, quantitative/qualitative work within the academy and in the wider society.

\textsuperscript{3} I place quotation marks around ‘post-feminist’ since, as much of the material in this thesis makes very clear, despite many gains – to borrow from and paraphrase a feminist slogan – we are not yet living in a post-patriarchy, and therefore the notion that a post-feminist society is possible is premature.
understanding of sex difference. In particular, feminist interaction with theories of the body that foreground the impact of biology on social and psychological manifestations of sex difference has been conflicted and often antagonistic.

In part, this is also an outcome of how, over time, when we look at the history of feminist engagement with questions of biology and sex difference, the same themes recur. Essentially the same arguments need to be brought to bear again and again in response to ‘new’ challenges. In her classic text *Biological Politics* (1982), Janet Sayers concludes by saying:

> Capitalism, despite its liberal protestations, has had to be fought every inch of the way to get it to concede equal rights to women … This fight has meant, among other things, that feminists have repeatedly had to address the biological arguments adduced by the ideologues of capitalism. They have repeatedly had to confront and criticize the many biologically phrased arguments designed to defend existing sexual inequalities in society. As I have demonstrated in this book, these arguments are still being advanced today. The struggle is by no means over: even the limited victories that have been secured in regard to women’s biological function in the area of abortion, for instance, are now under threat (1982, p. 201).

It is discouraging to realise that the ‘today’ to which Sayers refers was over 25 years ago, and yet her statement could, with equal truth, apply to ‘today’ as I write this, in early 2009. Four decades after Kate Millett (1971) alerted us to the invisible and naturalising assumptions and attitudes entrenched within traditionally patriarchal institutions, what is now at risk of becoming (some feminists would perhaps say has already become) so naturalised as to be accepted without question, is a notion of a universal human/female nature that is encoded in our genes. This reinscription of ‘our nature’ in our genes returns us back to the same old set of assumptions in which women’s biology is again recruited to put us back in our place. Clearly, there is something wrong here, not
only with the tenor and content of some of the biological arguments advanced by ‘the other side’, but with the ways in which we, as feminists, respond to them.

This thesis is about biological sex difference, about ‘human nature’ if you will, and the strained relationship between feminism and science on this issue. It takes as its starting point my unease with the ways in which feminist theory deals with the reality of biology – the whole corporeal package, the substance that is me. My interest in this question began some years ago when I read a comment made by Elizabeth Grosz in Volatile Bodies, to the effect that the project of correlating neurological function (biology) with ideas and mental processes (culture) seemed doomed to failure (1994, p. 7). This statement both puzzled and irritated me, and my attempts to think through just why it did so lent me a different perspective on feminist literature on the body. Again and again, I was returned to my perception of Grosz’s statement as an almost throw-away line that, nevertheless, works to shut down that particular field of inquiry. Why would she want to do this, if indeed this was her intention and not merely my interpretation?

As I became more deeply immersed in the wider literature on sex difference and the body, the ubiquity of appeals to biology as an explanation for human/woman’s ‘nature’, and the reflexive distancing of biology within feminist body theory, increased my exasperation. The more I read, the clearer it became that I was far from alone in struggling with these problems, and that the struggle to deal with the dialectic between feminism and biology was taking place on both sides of the campus; as long ago as 1986, for example, feminist biologist Lynda
Birke was calling for feminists to rethink biology. Although I was somewhat comforted by the insights to be found in the feminist literature, I was less than satisfied with the answers, or lack of answers, to my ‘dumb’ questions: If, as an outcome of being conceived with an XX combination of chromosomes, I am a particular sexed body (a female body), why do we draw the line at my also being/having a particular sexed brain? What do we make of that? Why don’t we make something of that? How can we overcome the mind/body and nature/nurture splits by theorising a biologically sexed body and not a biologically sexed mind? Why does it seem as though we have largely abdicated this theoretical space to the ‘other’ side?  

Edward O. Wilson’s description of the reductionist nature of biology as a vampire in the sacristy of the social sciences, resonates with my understanding of the way many feminists still ‘read’ and react to socio/biological research. Since I began thinking through this thesis, there have been calls for a more open relationship between feminism and Darwinism (see, for example, Grosz, 1999, 2004, 2005); feminist interventions into neurobiological readings of the body (Wilson, 1998, 2004); some optimistic approaches to new genetic research (Keller, 2000), and to neo-Darwinian sociobiological readings of the body (Richards, 1982, 2004; Vandermassen, 2005). Nevertheless, feminist body theory for the most part still fails to positively connect with the reductionist theories that, in one way or

---

4 Vicki Kirby talks about her own capacity for asking ‘improper’ questions, and of how certain ‘dumb questions’ asked of her, although irritating, lingered in the mind and forced her to think more carefully about them. She says the ‘dumb question’ is one which “seems naïve and quite ignorant about the discursive protocols that would rule it out of order … Yet inasmuch as a prohibition is also an enticement, the ‘dumb question’ remains irrepressible” (1997, pp. 1-2).

5 Darwinism, as distinct from the neo-Darwinism that is dealt with in this thesis, and that is most prevalent today, however.
another, support the notion of essential sex differences. How then do we positively reanimate what has been for too long a polarised, unproductive conversation?

In asking this, I am influenced by Celia Roberts’ call for “more specific attention” to be paid to “the positive theorization of the role of biology in the production of behavior” (2000, p. 1, my emphasis). I suggest that there has been very little positive theorisation of biology’s influence on behaviour from either side of the debate. That is to say, on the feminist side there is a tendency to defer or deny the question; while, in the reductionist literature, even where the intended aim is said to be the acquisition of neutral (or even positive) knowledge, the social and political effect of assigning behaviour to biology has been almost universally negative for women.

Banu Subramaniam comments that “most women’s studies scholars engage with the sciences through [feminist critique] rather than walking across campus to see what work happens in these fields today” (2003, p. 929). But why might we want to do that? For me, the answer is a simple one: I am not going to begin to find the answers to my ‘dumb questions’ by remaining on my own side of the campus or by simply reading others’ accounts of reductionist theory. In the 15 years since I first read *Volatile Bodies*, there has been a clear social shift away from an explanatory reliance on ‘nurture’ back towards explanations from ‘nature’. I believe that this shift has been facilitated by an evident alignment between the separate but inter-related fields of genetics, sociobiology, and sex difference.
research since the early 1990s. That is, a greater willingness to entertain biology as an explanation for everything from sexuality to susceptibility to faith can, in some measure, be attributed to the undertaking to map and sequence the human genome and, in so doing, to reveal “nature’s complete genetic blueprint for building a human being” (NHGRI, n.d.-a, para 1). In turn, I contend that the work of academic sociobiologists and sex difference researchers has been reinvigorated by the intellectual comfort to be found within this endeavour and in post-genome studies. Finally, there is a continuing proliferation of vulgar reductionist texts that purport to ‘explain’ the sexes to each other by recourse to the ‘naturalness’ of sexually differentiated roles and behaviours. The popularity of these texts – the appeal of their over-simplified and distorted re/presentation of the academic research, and the ease with which they are presumed both valid and authoritative – is the best evidence of an increasing return to the social acceptance of a more biologically determinist environment over the past two decades.

So, what if biology does have a greater influence on our behavioural phenotype than feminist body theory currently allows? I suggest that the need to reconceptualise the ways in which we theorise bodies has become ever more urgent as the science of biology embeds itself deeper into the social and cultural fabric of everyday life. With the flood of new information spilling out from

---

6 NHGRI is the National Human Genome Research Institute.
7 Richard Lewontin defines the distinction between the genotype and the phenotype as: “The genotype is the descriptor of the genome which is the set of physical DNA molecules inherited from the organism’s parents. The phenotype is the descriptor of the phenome, the manifest physical properties of the organism, its physiology, morphology and behavior” (2004, para 2).
genetic, neurological and other technologies (complicating existing notions of how our biological being-in-the-world interacts with and intervenes in our social being-in-the-world), biology is once again a very large ‘elephant in the room’. There is a need for feminist body theory to revisit the fact of difference.

In this thesis, then, I position myself as a women’s studies scholar taking a walk across the campus to see what is going on in the three fields of reductionist science that most often inform popular, vulgar reductionism. This research impacts on the lives of all of us, but most particularly on the lives of women. Although I take embodiment as my starting point, I do not attempt to resolve debate over the actual differences between men and women. Nor do I attempt to decide between the opposing positions of nature or nurture. Rather, my aim is to constructively dissent from taking up either position in order to ask: what if?

As far as possible, I undertake this journey with an open mind, not intent on critique but, as a non-scientist, on understanding. I take with me just two fundamental assumptions: first, that biology cannot be left out of any account of how we negotiate our way in the world as sexed beings; and second, that believing this need not inevitably (although quite clearly it too often does) lead us into determinism. In walking across the campus, I hope to find potentially useful synergies between the two ‘sides’ that might work to ameliorate the more usually adversarial relationship. The walk will be worth it if finding such synergies allows for the prospect of a more positive theorisation of biology and the body. The risk, in taking the walk, is the possibility of disappointment – of
not finding, in colleagues or the other side, the same willingness to ask improper, dumb questions.

The thesis is organised in three parts. Part One (Skirting Difference) contains one chapter providing an overview of the ways in which the problem of biology and sexual difference has been dealt with by different schools of thought within feminist body theory. The chapter briefly describes how feminism has negotiated the essentialist/constructionist and mind/body splits, how we theorise bodies, and how we respond to biology and to the science of sex differences. This chapter places my walk across the campus within the context of feminism’s historical and necessary concern with questions of biology, and the difficulties that continue to exist around shifting the binaries, escaping binary thinking, and countering damaging reductionist readings of the body.

In Part Two of the thesis (The Vampires in the Sacristy), I turn my attention to some of the ways in which reductionist science constructs and theorises bodies. Following feminist philosopher Janet Radcliffe Richards, I proceed from a belief that, if “we really want to make society as well suited to people’s natures as possible, we cannot afford to ignore any evidence; not even when it is produced by the opposition” (1982, p. 27). The four chapters in this section each represent a discrete, yet related, biological ‘vampire’ for feminism. They are linked together by their reliance on biological processes to support their hypotheses and conclusions. Perhaps more importantly for my purposes, they are also strongly connected by their focus on and differential treatment of bodies. That is to say,
there is a thread running through and connecting the three fields of scientific
endeavour with each other and with the way they have been popularly rendered.
I move from genetics, where the interest is in discovering what is ‘human’ nature;
to sex difference research, where the interest is very clearly upon women’s
difference from men; to sociobiology, which hypothesises about both ‘human’
nature and sex differences to draw conclusions about ‘women’s nature’ that are
often detrimental to women. Finally, vulgar reductionism draws upon all three of
these academic enterprises to present a story to the wider public about the
natures of women and men that I believe is, in many ways, profoundly
disrespectful of and damaging for both sexes, but which, because it so effectively
supports the status quo, is especially harmful for women.

The four chapters in this section of the thesis are, then, a look at the arguments
and evidence being produced in three areas of reductionist science that are most
closely tied to research into and beliefs about biological sex difference: genetics,
sex difference, and sociobiology. In Chapter Two (Genetics, genomes, and
control: biology’s Holy Grail), I look at the science of genetics through the lens of
the Human Genome Project, its search for the biological basis of ‘human nature’,
and its attempts to meet the significant ethical, legal, and social issues arising
from this new knowledge. In Chapter Three (The bent twig: cognitive sex
difference), my focus shifts to the field of sex difference research and reveals how
this research, historically and still today, foregrounds the ‘nature of’ ‘woman’
within its investigations of ‘difference’. Chapter Four (‘Just so’ stories?:
sociobiology as persistent paradigm) is concerned with how academic
sociobiology brings together hypotheses about ‘human’ and ‘woman’s’ (and ‘man’s’) natures and proposes evolutionary mechanisms for sexed behaviours. In each of these chapters, my aim is to explore the work being undertaken in these fields, and to review what practitioners working within them have said about human sex difference. It is not my purpose to subject them to critique (although some critique will inevitably occur), since this has been exhaustively undertaken by others. Rather, I am interested to see where/whether there are spaces in which the familiar social body of feminist theory might, without harm, take up and take on some of the biological attributes described by these sciences. Finally, in Chapter Five (‘Lunch-chasers and nest-defenders’: vulgar reductionism), I step outside of the academy, and down from the fence, to explicitly critique the vulgar reductionism so endemic in popular culture, so explicit in popularisations of these fields of research, and so secure because of the complacency with which the research is undertaken.

In Part Three (‘The organism is predecessor to our theories about it’), I complete the circle by returning to my ‘own’ side of the campus to outline some feminist work that attempts to move towards a more positive engagement with some elements of reductionist theory. These are those feminists who theorise the body through its various biological processes, and the sociobiologist feminists, who have shifted conventional malestream conversations about sex difference from within. As well, I look briefly at those feminists who weave their philosophies around a favourable view of evolutionary psychology, a more recent manifestation of sex difference and sociobiological research. I conclude this
chapter by expressing some views about a way forward for feminists on each side of the nature/nurture divide.

Finally, within a brief summary of my thesis journey, I revisit the potential synergies that exist between feminist theory and the reductionist sciences examined here, and offer some thoughts on the possibilities for a more productive conversation between them.
Part one : Skirting difference

No science or discipline can peel off layers of culture and learning and find an untouched core of biological *nature*. Rather than biology acting to constrain and limit our potentialities, it *is*, in fact, the supreme irony that our magnificent brains, with their nearly limitless structural and functional potentiality for learning, flexibility, and choice-making, have produced *cultures* that constrain and limit those potentialities.

Ruth Bleier (1984, p. 7)

We were the “cultural animal” all right, but stress was entirely on the cultural while the animal was relegated to a few odd things like blinking, sucking, feeling hungry, and copulating. Ninety-nine percent of our behavior, it was held, was “learned” and hence cultural. And what was more there was no limit to what could be learned.

Robin Fox (1971, p. 280)
One
Embracing nurture: the biosocial politics of difference

[T]he subject of women’s biology is profoundly political.
Ruth Hubbard (1990, p. 2)

[P]artisans of the Standard Social Science Model insist on the Cartesian distinction between the material world of anatomy and physiology and the mental world of psychology, vigorously resisting attempts to see them as different descriptions of the same integrated system, subject to the same organizing principles.
John Tooby & Leda Cosmides (1995, p. 57)

Introduction

Historically, both academic and grassroots feminists have focused on the fundamental importance of our bodies: the meanings given to them, the social roles prescribed for them, and the injustices visited upon them. Whatever else it may be and however variously we may define and describe it, by its very nature feminism is deeply invested in the physicality of women. This investment informs much of the resistance to and interpretation of sociobiological and genetic research as saying that our bodies are only ours in theory; that is, that in reality they belong to ‘the gene’, and the gene, in turn, now belongs to (predominantly male) science.
Feminist theorists occupy various positions on the continuum between essentialism and social constructionism in their approaches to such reductionist paradigms. I am principally interested in two groups of theorists who might be said to occupy the middle ground, and whose work most clearly struggles with the problem our biology poses. The first of these are feminist approaches that, despite acknowledging the body, work to defer the problem of sex difference by dealing with an ‘imaginary’, discursive, legislative, fragmented, or social body (for example, Butler, 1990; Diprose, 1994; Gallop, 1988; Gatens, 1996; Grosz, 1994; Kirby, 1997). The second approach are those feminists who, in one way or another, challenge the science that has the effect of, or explicitly seeks to, establish a biological foundation for an essential female (or male) behavioural pattern (including Bleier, 1984; Fausto-Sterling, 1985, 2000c; Hubbard, 1990; Keller, 1985; Keller & Longino, 1996; Oakley, 1972; Rogers, 2000; Sayers, 1982; Spanier, 1995). It is within these two approaches that, for my purposes, the struggle over biological sex difference is largely played out.

In general, I dislike taxonomies for their tendency to attach a singular label to individuals who may, and usually do, belong to disparate groups, espouse complex and nuanced positions, and subscribe simultaneously to a range of views that may well cut across taxonomic groups, dependent upon circumstances. Nevertheless, in an attempt to negotiate a conversation with the “difficult child” that is the study of sex differences, and with its “thoroughly

---

8  As distinct from those feminists (for example, Sandra Harding) whose focus is on the position of women in science.
mixed-up” feminist parents (Foreword, Oakley, 1985),9 in this chapter, I will position the two groups I am most interested in within a loose taxonomy of five primary approaches to the problem of biology, adopted by feminist writers on sex difference and the body.10 A sixth position, where I see feminist and reductionist approaches beginning to come together, is dealt with in the final chapter of the thesis. This taxonomy also allows me to discern the directions of the conversations that are already in play around these questions between the various parties: Who is talking to whom? Which groups are not talking to each other? On what terms are conversations being conducted? Who is walking across the campus and who is not?

Two poles

The first two of these positions, the essentialist and the explicitly postmodernist/poststructuralist positions, will be dealt with only briefly since their shortcomings and difficulties are well understood. The third position, the metatheoretical approach, is dealt with in slightly more depth, since it really sits to the side of, and is in conversation with, the range of other approaches. Theorists within this group deal not with biology per se, but with an interrogation of dualistic thought and how this has circumscribed the routes feminists are able to take through this difficult theoretical terrain. Nonetheless, they set a broader context for the final two positions – sexual difference feminism, and feminist engagements with science – which I take up in much more detail because they

9 As, indeed, in later chapters, I somewhat artificially segregate its thoroughly mixed-up reductionist parents within particular paradigms when in fact many of those working and writing in these fields cross and re-cross taxonomic borders.

10 While making no claim that this is an exhaustive list of feminists writing in any of these areas.
are both more closely aligned to debates around biology and the body. I begin
with the two positions at either end of my taxonomy, since these very polarised
positions have, in their different ways, always already effectively eliminated the
questions with which I am concerned in this thesis.

In the strong essentialist position, often associated with eco-feminism and with
some cultural feminist work, differences carry implications not only for physical
characteristics but also for gendered behaviours (for example, masculine
characteristics of aggression, competitiveness, emotional disconnectedness;
feminine characteristics of consultation, caring and peacemaking). This model,
sometimes explicitly but more often implicitly, accepts the notion that there are
important and influential sex differences in the brain, but redefines and reverses
the judgments attached to sex-linked traits so that the (usually) negatively-
valued, so-called female traits are instead valorised (see, for example, Gilligan,
1982; Griffin, 1984; Kreiger, 1987; Merchant, 1982; Plumwood, 1993; Warren &
Erkal, 1997).

While we may applaud any emphasis placed upon a positive valuation of so-
called women’s traits, this manoeuvre effectively, and simply, inverts one of the
least acceptable outcomes of malestream thinking – the preferential valuing of
behavioural traits associated with men. It thereby ultimately risks more firmly
embedding the same essentialist notions that have worked so effectively against

11 Although this is an approach most closely associated with ecofeminism, it is important to note
that many ecofeminists argue that women are best fitted for certain tasks not by nature, but as a
result of sex differentiated nurturing. Chris Cuomo (1998) and Janet Biehl (1991) both argue against
the ‘essentialist’ frame of much eco-feminist thought.
the best interests of women. Indeed, this is an approach that has been actively pursued by some sociobiologists, and writers of vulgar popularisations of sex difference science, who appear to suppose that writing ‘woman’ and some (still biologically derived) ‘feminine’ traits as superior and/or beneficial to women will somehow make the underlying determinism more palatable.\textsuperscript{12}

As I make clear throughout the thesis, I am not averse to the notion of biology as a player in the formation of sex-differentiated psychological characteristics \textit{per se}. On the contrary, my openness to this \textit{as a probability} was a major precipitating factor for undertaking this research. Yet, accepting this probability is \textit{not} the same as accepting that either sex is best fitted \textit{by nature} for anything (with the obvious exceptions of those few elements of reproductive process biologically tied to one sex or the other; that is, insemination, gestation, parturition, and lactation). If we are to reject the idea that women are fitted \textit{by nature} for a particular sexually differentiated (subordinate) social niche, then I suggest we must also reject the notion that we are better fitted by nature than are men for the (superior) niche that they have historically occupied.

At the other end of the continuum lies the explicitly postmodernist/poststructuralist turn to an approach that questions the very existence of ‘woman’ outside of social discursive practice (a shift that itself raises a whole new set of ‘improper’ or ‘dumb’ questions for me that cannot be dealt with here). Denise Riley, for example, states that ‘‘The body’ is not, for all its

\begin{flushright}
\textsuperscript{12} See, for example, Helen Fisher’s \textit{The first sex} (1999), and any number of popular texts of the ‘Why men don’t iron’ variety.
\end{flushright}
corporeality, an originating point nor yet a terminus; it is a result or an effect” (1988, p. 102), and that she:

would not seek the freshly conceived creature, the revelatory Woman we have not yet heard. She is an old enough project, whose repeated failures testify to the impossibility of carving out a truly radical space; the damage flows from the very categorisation ‘woman’ which is and has always been circumscribed in advance from some quarter or other (1988, p. 107).

Other feminists writing in this vein see questions of biology as theoretically irrelevant. Christine Delphy comments that, “the vast majority [of feminists] continue to think that ‘we mustn’t ignore biology’. But why not exactly?” (1984, p. 23). This most extreme version of the social constructionist model of sex difference allows only that women and men (may) have differing physical characteristics as a result of their biological sex, maintaining that all other differences, and the significance accorded to any difference, are produced discursively by/within our historical and social location. Where the body is a true tabula rasa, an undifferentiated lump of human clay within and upon which cultural constructions of ‘woman’ are inscribed, the whole issue of biological sex difference becomes redundant.

These two approaches do not walk across the campus and they are, effectively, in conversation only with each other. In seeking to deal with the dualism by either adopting or denying it, they are not particularly useful to the questions I seek to interrogate. At best, they allow us to clearly see the difficulties associated with taking a very polarised approach to the problem of biology, and in this sense,

13 Again, Delphy argues that gender “created anatomical sex” (1984, p. 144).
they have been useful to my third category: what I have called the metatheory on feminist approaches to biology. These feminists focus on the ramifications of thinking about the body dualistically. They explore the various ways in which essentialist thought and attributions of sameness and equality have played off against notions of difference, the effects of this on women historically, and how this way of approaching the problem of biology has constrained feminist theorists in their responses to it. Theorists engaged in untangling and making clear the problem of binary thinking are also largely involved in conversations with other feminists, and not with the reductionist science that underwrites theories of sameness/difference and essentialist/social constructionist thought.

**Metatheoretical approaches**

In this section, I discuss the work of two feminist theorists, to illustrate the kind of metatheoretical approach that comments upon the tensions within feminism struggling with dualistic thinking. Carol Bacchi, in conversation with essentialist/social constructionist feminists, allows us to see very clearly the problem of essentialism for feminism; and Diana Fuss, primarily in conversation with the postmodern/poststructural and sexual difference feminists, reveals the problems with postmodernism. Bacchi and Fuss are less concerned with the dualisms they use to illustrate their work than with the more fundamental problem of dualistic thinking itself.

In *Same Difference* (1990), Carol Bacchi explores the ways in which feminism has engaged with questions of sexual sameness/difference within the context of our
temporal and cultural locations. In comparing and contrasting historical debates and contemporary Australian, British and American approaches to sexual difference framed within positions on equal rights, special treatment, protective legislation, the sexuality debates (pornography and sexuality), and the construction of ‘woman’, Bacchi allows us to see why the question of biology has been confronted and denied in the ways that it has:

‘Sameness’ and ‘difference’ became catch-cries, precluding more thoughtful discussion of the social structures which made it necessary for women to advocate one of these alternatives (1990, p. 49).

Concluding that American feminism is predominantly equal rights (which she roughly approximates to socialisation theory); British feminism is predominantly welfare-oriented; and Australian feminism is a combination of both (which she calls ‘reform’ feminism), Bacchi makes it clear that each approach carries with it the potential for inequity for some group of women. ‘Equal treatment’ (sameness) theorists, for example:

... will wait and fight for laws that ‘accommodate the needs of all workers’, even if this results in ‘immediate losses for some women’ who might have taken maternity leave (1990, p. 119),

while ‘special treatment’ (difference) theorists,

... portray equal treatment as part of classic liberalism which ‘dissociates the individual person from any context of family, religion or class’. In contrast to an analysis of abstracted individuals, they say that they are willing to identify the needs of women as a group, and to suggest that the goal ought to be ‘equality of effect’ rather than ‘equal treatment’ (1990, p. 119).

Interestingly – and in a reflection of and response to a primary sameness/difference concern within reductionist theory, when discussing ‘equal’ versus ‘special’ treatment – the only biological sex difference that is canvassed in any detail is pregnancy (that is, female reproductive function). Bacchi’s
fundamental point, however, is that the argument between the sameness/difference alternatives has become a focus because of social realities and structures. Noting that “the sameness/difference framework is a limited way of conceptualising social problems” (1990, p. 107), she concludes that framing the argument in these ways averts “necessary social change” (1990, p. 157). She says:

The ‘sameness’ alternative is insufficiently critical of the status quo. The ‘difference’ option is critical of the status quo, but seems to conjecture that women can exist in some sort of separate world. Seeing women as the ‘same’ as men prevents us challenging the model against which women are being compared; seeing women as ‘different’ prevents us changing it. Talking in terms of ‘sameness’ or ‘difference’ skirts the issue, rather than confronting it (1990, p. 262).

Like Simone de Beauvoir, Bacchi locates the central issue of the sameness/difference question in the identification of women in relation to men, proposing instead that we need to get beyond this formulation to one where political change allows “people to live fully human lives” (1990, p. 265, my emphasis). Her focus is, then, not on the fact of difference itself, but rather on the social and political implications that have followed from situating (each individual) woman as ‘different from’ men, rather than as having a “differential social location” (1990, p. 167) to men.

Diana Fuss also confronts the problem of thinking dualistically about the body in her important text, Essentially Speaking (1989), in which she argues that “essentialism is essential to social constructionism”, and aims to “break or in some way to weaken the hold which the essentialist/constructionist binarism has on feminist theory” (1989, p. 1). As Fuss notes, the weaknesses of essentialism

---

14 It is interesting to note how the feminist location of essentialism appears to slip from the classic definition of ‘essence’ – “that which is most irreducible, unchanging, and therefore constitutive of a
are well known; perhaps not so well recognised are the problems of the social constructionist reliance upon concepts and categories which are still essentially implicated in essentialism. Thus, for example, the constructionist manoeuvre of pluralising to mark difference itself relies on a ‘linguistic unity’ – women – which “semantically marks a collectivity” (1989, p. 4).

Most relevantly, Fuss elaborates on the essentialist basis of the deconstructionist phrase ‘always already’, so broadly in use within postmodernist circles. The danger, she says, lies in “the temptation to rely upon the ‘always already’ self-evident ‘nature’ of ‘always already’”:

The danger (and the usefulness) of “always already” is that it implies essence, it hints at an irreducible core that requires no further investigation. In so doing, it frequently puts a stop to analysis, often at an argument’s most crucial point (1989, p. 17).

In short, while social construction and essentialism are organised dualistically and are necessarily implicated in each other, Fuss argues that social constructionism constantly defers its engagement with essentialism rather than confront its dependence upon it. Recognising this allows Fuss to question whether (and how) we might begin to think of essentialism in terms of flux and change and, in this way, to trouble the diametric opposition between the two terms. If essence is indeed a “slippery and elusive category” (1989, p. 20), then logically it is not fixed.

given person or thing” – to an “‘original femininity’, women’s oppression, and women’s unity” (Fuss, 1989, p. 2, my emphasis), as though men are not also embodied and therefore also potentially have an ‘essence’.
These metatheories make clear that it is not easy to escape dualistic thinking, even where one is concerned to ‘trouble’ a specific dualism. Further, they provide a background against which to read those feminist theorists, writing within and across diverse disciplinary boundaries, who acknowledge the fundamental importance of biology and have taken up the challenge of dealing with the problems it poses in their work. This struggle is most evident in the final two approaches to be discussed here: those I described as occupying what can be called loosely the ‘middle ground’, although they are in fact both much closer to the social construction end of the continuum.

**Sexual difference feminism – the ‘corporeal turn’**

I have borrowed the designation “sexual difference” feminism from Elizabeth Grosz (1994, p. 17). She used it to categorise a diverse group of feminists historically situated within the 1980s and 1990s, who employed various approaches to grapple with ‘the problem of’ the body. Situated within this broader field, is the body of theory labelled the ‘corporeal turn’. Produced primarily in the 1990s by Grosz and other Australian feminist philosophers Moira Gatens and Rosalyn Diprose, and social scientist Vicki Kirby, this work provides a useful site to explore sex difference feminism, because these theorists specifically attempt to confront and include biological process in their meditations on the body. In the end, however, I would argue that despite their best efforts,

---

15 In her list of ‘sexual difference’ feminists, Grosz includes “Luce Irigaray, Helene Cixous, Gayatri Spivak, Jane Gallop, Judith Butler, Naomi Schor, Monique Wittig and many others” (1994, p. 17).
these theorists deal with the question of sex differences and the biological body by *not* dealing with it.\(^\text{16}\)

Elizabeth Grosz, internationally recognised for her work in philosophies of the body, is perhaps the most prominent of the corporeal feminists. Early in the ‘Introduction’ to her most well-known text, *Volatile Bodies* (1994), she says that feminist theory has generally remained “uninterested or unconvinced about focusing on bodies” (1994, p. vii).\(^\text{17}\) For someone interested in finding synergies specifically between biological and feminist theories of the body, Grosz’s identification and portrayal of ‘sexual difference’ feminists initially strikes some encouraging notes. In cataloguing the several beliefs and concerns that this group of feminist theorists have in common, she includes a “refusal or transgression of the mind/body dualism”, a suspicion of the sex/gender distinction, and “a commitment to a notion of the fundamental, irreducible differences between the sexes” (1994, p. 18). There are early indications, however, that these statements are not to be read in any straightforward or uncomplicated way. Grosz attempts, in this book, to redress the wider disinterest in bodies by “using corporeality as the framework for explaining effects of subjectivity”, which, she then says, “has the added bonus of inevitably raising the question of sexual difference *in a way that mind does not*” (1994, p. vii, my emphasis). While this appears to promote the body over the mind – ‘mind’ is here placed in the curious position of being

---

\(^{16}\) Although the arguments deployed by these theorists are, of course, considerably more intricate and extensive than is reflected here, and I am conscious that I am far from doing justice to their complexity within this restricted frame.

\(^{17}\) In her later texts, Grosz herself has shifted away from a specific focus on the body to an interest in the philosophies of time, which necessarily includes some engagement with evolutionary and Darwinian theory (see Grosz, 2004, 2005).
treated both as inconsequential to questions of sex difference, and as extracorporeal – this is not her intent. She says:

The body cannot be understood as a neutral screen, a biological *tabula rasa* onto which masculine or feminine could be indifferently projected. Instead of seeing sex as an essentialist and gender as a constructionist category, [sexual difference feminists] are concerned to undermine the dichotomy (1994, p. 18).

Grosz utilises concepts of corporeality and body image to frame her resistance to the body/mind and sex/gender dualisms, and deploys Lacan’s metaphor of the Mobius strip\(^\text{18}\) to sustain an argument that mind and body are neither one, nor two, but something in between:

Bodies and minds are not two distinct substances or two kinds of attributes of a single substance but somewhere in between these two alternatives. The Mobius strip has the advantage of showing the inflection of mind into body and body into mind, the ways in which, through a kind of twisting or inversion, one side becomes another (1994, p. xii).

This model is said to circumvent both dualist and reductionist models of mind/body biology by positing a theory of continual flow between the inside and the outside, the mind and the body. Grosz’s arguments, and the book itself, are organised around her concern to show the need to “work both surfaces [that is] from the outside in and inside out … the way in which the outside of our bodies is lived on the interior … [and] how the psyche itself is a development or resolution of the body’s outer surface” (Best, 1994, para 6).

Our physical and psychological materialities become, then, an outcome of experience, experience that is mediated by our distinctive internalised body images, which are in turn mediated by our social and discursive inscriptions as

---

\(^{18}\) An inverted three-dimensional figure eight.
sexed bodies. Where is the biological body here? How do we get from a position that says:

I am reluctant to claim that sexual difference is purely a matter of the inscription and codification of somehow uncoded, absolutely raw material, as if these materials exert no resistance or recalcitrance to the processes of cultural inscription ... On the other hand, the opposite extreme also seems untenable. Bodies are not fixed, inert, purely genetically or biologically programmed entities that function in their particular ways and in their determinate forms independent of their cultural milieu and value (Grosz, 1994, p. 190),

to one that can claim, about the same text:

Volatile Bodies demonstrates that the sexually specific body is socially constructed: biology or nature is inherently social and has no pure or natural “origin” outside culture. Being the raw material of social and cultural organization, it is subject to the endless rewriting and inscription that constitute all sign systems (Publisher’s blurb, 1994, back cover).

This shift from a view of sex difference as potentially prior to, or at least outside of, cultural inscription, to a view of biology as ‘inherently social’, demonstrates the slipperiness of the task of resisting the biology/culture split and, further, that engaging with the dualism does not necessarily allow one to escape dualistic thinking. For example, and as others have noted, the Mobius strip is itself binary in its two-sidedness at any particular location, as is Grosz’s framework of a “psychical interiority and a corporeal exteriority” (Richardson, 2003, p. 99).

In any event, in this text, for the most part, Grosz concentrates her attention not on sexed bodies, but rather, on “apparently ‘neutral’ presentations of the ‘human’ body” (Grosz, 1994, p. 188). She is “concerned with the ways in which a corporeal ‘universal’ has in fact functioned as a veiled representation and projection of a masculine which takes itself as the unquestioned norm”, and with the ways in

---

19 I make the assumption that, if Grosz has not written the blurb herself, she has at the very least approved this description of her overall meaning in the text.
which the specificities of the female body have been subsumed, “reduced to the role of modifications or variations of the (implicitly white, male, youthful, heterosexual, middle-class) human body” (1994, p. 188).

Constrained, however, by the agenda within which she is working – the interrogation of male theorists’ theorising about the ‘human’ body – Grosz herself never really gets to the specificities of the female body in this work. There are no biological sex differences to be found here – only in the final section do we find ‘sexed bodies’, and even here, the only detailed discussion of a biological function specific to women is of menstruation, which is placed in opposition, as it were, to semen, the only specifically male flow. Together with the fact that she is in dialogue with male theorists, within whose work she argues there simply is no female body, the need to set up these kinds of oppositions between interior/exterior and male/female bodies means that Grosz is herself forced back into (or never escapes from) a dualistic framework, in which the exterior constructs the interior; the interior is cast as subjectivity not biology; and male bodies are presumed normative (if not by Grosz, then by her sources).

In common with Grosz, Moira Gatens is concerned to subvert dualisms. In a series of essays written over a span of 11 years, framed by a preoccupation “with the question of how to theorize human embodiment without losing the sexual, political or ethical particularity of different bodies” (1996, p. vii), she deploys the notion of the body image (the ‘imaginary’) to challenge familiar constructions of the body. As she explains, in an interview with Emile Sherman:
I use the notion of the imaginary body to posit an understanding of sexual difference that isn’t caught up in a series of dualisms which have dominated Western thought … The imaginary body cannot be reduced to either gender identity or biology. Rather it is a term that straddles both those conceptions and is linked to a range of social and political practices and institutions which generate specific social imaginaries (Sherman, 1996, p. 11).

In her critique of the sex/gender distinction Gatens challenges – rightly, in my view – the usefulness of the concept of gender for feminism, noting its ‘questionable’ theoretical ground and its tendency to treat the body “as sex-neutral and consciousness as a passive *tabula rasa*” (Gatens, 1996, p. 16). She says:

In addition to the neutralization of sexual difference, the sex/gender distinction lends itself to those groups or individuals whose analyses reveal a desire to ignore sexual difference and prioritize ‘class’, ‘discourse’, ‘power’ or some other ‘hobby-horse’. Their accounts attempt to co-opt or trivialize feminist struggles and feminist theory, reducing sexual politics to gender difference … as if women’s bodies and the representation and control of women’s bodies were not a crucial stake in these struggles (1996, p. 17).

It is important to note here, though, that while the bodies Gatens is talking about are ‘biological’ bodies, the significance accorded their sexual differentiation is accomplished in/by the social imaginary:

The very same behaviours (whether they be masculine or feminine) have quite different personal and social significances when acted out by the male subject on the one hand and the female subject on the other. Identical social ‘training’, attitudes or, if you will, conditioning acquire different significances when applied to male or female subjects (1996, p. 9).

Arguing against an “*arbitrary* connection between femininity and the female body, masculinity and the male body” (1996, p. 4, my emphasis), then, she concludes that feminine behaviours “are manifestations of and reactions to the (conscious and unconscious) ideas which we share about our biology” (1996, p. 14). Significantly, although the connection between sex and gender is drawn tight here (as indeed Grosz does in her discussion of the inability of male transsexuals to truly experience what it is like to live as a woman (1994, pp. 207-208)),
gendered behaviours are still not grounded in biology as such, but are necessarily mediated by our social being-in-the-world. That is to say, in attempting to overcome the sex/gender distinction, Gatens lapses back into the mind/body split. It is how we think about our bodies, not our body’s biology, which produces sexual culture.

The last of my three Australian feminist philosophers, Rosalyn Diprose, also refuses the idea of any ‘natural’ pre-social identity, although, unlike Grosz and Gatens, she explicitly attempts to work outside the frame of the nature/culture distinction. Diprose interrogates and attempts to overcome the operation of the mind/body dualism through a focus on contractual responses to pregnant bodies, and asks why it is that contractarian ethics “cannot fairly accommodate women’s specific modes of embodiment” (1994, p. viii). That it cannot is made clear by her careful analysis of its application in surrogacy contracts. Diprose concludes that the difficulty lies within the Cartesian approach, which leads to an objectification of the body and an inability to allow for sexual difference, resulting in the exclusion of the female body from the social contract. She contends that the very foundation of ethics rests on an acceptance of the mind/body split, where the mind is understood to be distinct from, uninfluenced by, and in control of the object that is the body. This understanding of the body does not hold in the case of a pregnant body, where the notion of a self-present individual, unaffected by corporeal change, and with a mind that is distinct from and emotionally uninvolved with the labour of the body, cannot be sustained.
Thus, Diprose rejects the liberal/contractarian assumption of an autonomous, self-present individual and, following Maurice Merleau-Ponty (1962), argues that the body is the “self-expressed” (1994, p. 109). That is, we do not have a body, we are a body, and that body is the sum of the socially constructed and differentiated self.Individual identity and difference, then, come from, not before, social relationships:

Merleau-Ponty ... claim[s] that the lived body is constituted by its dwelling in the world. That is, the capacities and habits, and therefore the interests, of any body do not arise separately from its engagement with others nor from the discourses and practices which make up the world in which it dwells. For Merleau-Ponty, your corporeal schema is never individual: it is fundamentally intersubjective and specific to your social and familial situation (1994, p. 119).

Diprose continues:

... the limits to your potential modes of being will vary depending on with whom you associate and under what circumstances. Presumably then, insofar as social discourses expect and encourage differences in male and female comportment, these differences will be incorporated into the corporeal schemas of children (1994, p. 121).

For Diprose, there simply is no ‘natural’ (sexed, biological) body, only different bodies that take up positions as subject or object within the (social) construction of the self. Her effort to ‘work outside’ the nature/culture split leaves the differentiated body’s biology ambiguously poised and, for the most part, meaningful only in an act of social construction.

Within each of these accounts, although the biological body is present and acknowledged, it is nevertheless inexorably overshadowed by the social. It remains a construct of our ideas, our experiences, our situatedness within the social world. There is a kind of double-movement going on here that

---

Diprose quotes Merleau-Ponty: “This body is not something I have, it is what I am and its motility is how I have a world” (1994, p. 106).
simultaneously entertains and rejects any notion of agency on the part of a biology operating independent of culture, which means that the particularities of sexed bodies that are foregrounded are still those that are necessarily constructed within the social and temporal realms within which they exist and interact. And so, despite an avowed interest in engaging with the body, the conversation within corporeal theory is here confined to one side of the campus, and the question of *biological* differentiation remains unapproachable, untheorised and thus unable to deal with contrary claims from the other side of the campus.

Moira Gatens has said:

> We live in a Cartesian present. Our ‘common sense’ conceptions of human being are Cartesian (quoted in Sherman, 1996, p. 14).

The flow from the outside/in - from culture to nature – is evident within these philosophies of corporeality, but I suggest that the flow from the inside/out, the flow from body (biology) to mind, remains obscured. And in the end, we still live in a Cartesian present.

Social scientist Vicki Kirby is also interested in interrogating the operation of the nature/culture dualism and its effect on the meanings ascribed to bodies. Kirby, however, situates her inquiry somewhat differently: while acknowledging the interventions of many of the male theorists with whom Grosz, Gatens and Diprose are in conversation, her scrutiny is primarily directed to and in conversation with the work of other feminists, in particular with postmodernist and poststructuralist approaches to biology and the body. Although still confined to one side of the campus in her conversations, Kirby is thus simultaneously positioned as an ‘insider’ – a corporeal feminist who is seeking a way out of the
nature/culture dualism – and an ‘outsider’, a metatheorist casting a critical eye over the problematic effects of dualistic thinking within feminism itself.

Kirby’s careful critique of the postmodern turn to metaphors of inscription illustrates both its failure to recognise the possibility that “nature scribbles or that flesh reads” (1997, p. 127), and the inherently dualistic nature of its own thinking about biological agency. Her challenge to the postmodern approach is, although complex in its full account, also elegantly simple: if culture writes, upon what does it write? As she so evocatively demonstrates in her recount of the conference speaker who, pinching herself, declares that she is not referring to “this [biological] body”: if not ‘this body’, which body are we talking about, and where is it located (1997, p. 70)? She says:

And is our reluctance to address the question of ‘flesh and bone’ a silent acknowledgment that the sense of ‘risk’ is warranted, that the substance of biology will inevitably thwart our puny efforts with the force of its reality? (1997, p. 76).

The questions Kirby poses, the flaws she exposes in postmodernist approaches, and most particularly her cogent insight into the problem of the terms upon which we can come to grips with ‘the substance of biology’, all bring us closer to engaging meaningfully with ‘this’ biological body, and with its representation within reductionist theory. In her work, “anatomy’s dermal veil” (1997, p. 77) has begun to lift, revealing tantalising glimpses of the flesh and bone behind it. As valuable as her challenge is, though, seeing the problem is not the same as solving the problem, and Kirby’s own efforts to find a way out of the dilemma circle back into it. In struggling with the complexities involved in coming to grips with the divide between notions of embodiment and the “site of self” (1997, p.
Kirby begins to approach the question of biology by positioning anatomy as embracing both essentialism and non-essentialism, and biology as the “mutable intertexture” that rewrites itself (1997, p. 78). However, in then subsuming both within morphology, Kirby returns to a concept of inscription, where “‘becoming woman’ is written in the spacing of a corporeography from which nothing is exempted” (1997, p. 80). The question of biology as process is once again deferred. In the end, even though she so clearly delineates the problem, Kirby, like her philosopher colleagues, is still unable to solve it.

Challenging the science

For the final group of feminist theorists I wish to discuss in this chapter, I turn to a selection of works undertaken within the field of feminist science studies, which originated before and extends throughout and after the work of the corporeal feminists. Feminist science studies attracts a large and disparate group of contributors from both sides of the campus and is thus comprised of scientific ‘insiders’ and ‘outsiders’.

Some of the ‘outsiders’, from fields such as philosophy, history and sociology, are chiefly concerned to critique the practice of ‘science’ as an enterprise that is inherently masculinist, to encourage greater participation in science by women, and/or to develop a feminist approach to science (for example, Harding, 1991; Hird, 2004; Keller & Longino, 1996; Laslett, Kohlstedt, Longino, & Hammonds, 1996; Rosser, 1992; Schiebinger, 1993; Tuana, 1989, 1993). This body of work, vital as it is in other contexts, is not germane to my specific concerns here. Still others
(although they are relatively few) engage with particular scientific disciplines to explain and to some extent defend them (see, for example, Patai, 2000; Richards, 2004; Vandermassen, 2005), and this latter group will be discussed later in the thesis.

Of more direct interest here are those who are intent on deconstructing and exposing the ways in which specific scientific enterprises are interpreted, the social effects that follow, and the alternative readings that might be made (among many others, Diprose, 1991; Kerr & Shakespeare, 2002; Mahowald et al., 1996; Mahowald, 2000; Martin, 1991; Morse, 1998; Nelkin, 1993b, 2001; Nelkin & Lindee, 1995a; Rose & Rose, 2000; Rosner & Johnson, 1995; Segal, 2001; Tavris & Offir, 1977; Travis, 2003).

Similarly, ‘insiders’ – feminist biologists, ethologists, neuroscientists, primatologists, physicists and molecular geneticists – engage with the reductionist science that is the focus of this thesis from different perspectives, depending on whether their primary identity is as a feminist or a scientist. This, of course, is a subtle and fluid distinction, since locations can shift and none of these groups are mutually exclusive: all are wanting to ask a different set of scientific questions shaped by their feminism in order to do better science, but in pursuit of this end, they adopt differing approaches. The majority – the ‘feminist scientists’ – primarily employ their specialised scientific skills through a feminist lens to critique androcentric bias in the questions, methodologies, and products of the science of others (see Birke, 1986, 2000; Bleier, 1984; Fausto-Sterling, 1985,
2000a, 2000c; Hubbard, 1990, 2003; Kaplan & Rogers, 2003; Keller, 1985, 2000; Keller & Longino, 1996; Rogers, 2000; Ross, 1992; Spanier, 1995). Others – the ‘scientist feminists’ – utilise the tools and methodologies of their particular disciplines alongside the ‘tools’ provided by feminism, working for change from within largely through the insights of their own research (in sociobiology, for example, see Hrdy, 1999, original publication 1981; Smuts, Cheney, Seyfarth, Wrangham, & Struhsaker, 1987; Smuts, 1999; Zuk, 2002). 21

Any general discussion of this enterprise must begin with British sociologist Ann Oakley who, in 1972, opened the door to feminist examination and critique of scientific and sociobiological views of sex difference with her now classic text, Sex, gender and society (1972). The first paragraph of Oakley’s original ‘Introduction’ poses the central questions in the debate over sex difference, and in so doing, delineates the disciplinary and philosophical split that still exists within the field:

Everybody knows that men and women are different. But behind this knowledge lies a certain uneasiness: how different are they? What is the extent of the difference? What significance does it have for the way male and female behave and are treated in society? (1972, p. 9).

Her questions foreshadow the difference in approach and emphasis that I am considering: between, on the one hand scientists who, in general, emphasise the importance of gathering empirical data, grounded in biology, in an attempt to answer the first two questions; and, on the other hand, feminists among others,

21 As I indicate in chapter 4, I am conscious that not all those whose work includes analysis of the biology of social behaviour – whether in humans or in other animals – would choose to identify themselves as a ‘sociobiologist’. Sarah Hrdy, Barbara Smuts, and Marlene Zuk, however, do so.
who are more concerned with the social significance of any findings of sex difference, and with proposing alternatives to biological explanations for them.

To put some order into my discussion of feminist engagement with the science of sex differences, I am going to rely heavily upon Oakley’s progression through the various debates around sex differences. This is because *Sex, gender & society* is, in very many ways, the paradigmatic example of feminist response to sex difference research. In its orderly marshalling of the research across various domains and its posing of social realities as alternatives to reductionist conclusions, it foreshadows the particular concerns of much of the work that has followed in the intervening decades up to today. As well, in its overarching concern to shift the focus from innate male/female difference to a focus on the ‘lived body’ and individual difference, it is a precursor to the theoretical concerns of the corporeal feminists discussed earlier in this chapter.

Nonetheless, it is important to note that in the nearly four decades since first publication, some of the science that Oakley cites has been superseded, and thus certain statements that she makes, based on that science, would today be disputed. Her acceptance of a characterisation of the female as the ‘basic’ human form and the male as the result of something ‘added’, for example, has been challenged by the literature discussed later in this thesis, as a mistaken exemplar of the naturalised hierarchy of the sexes (see 1972, p. 22). In what follows, I leave these inevitable inaccuracies and outdated research cited in the text aside,

---

22 See, for example, Lynda Birke (2000, p. 37), and Anne Fausto-Sterling’s discussion of this issue (2000c, in chapter 8, in particular pp. 203-205).
and instead use her text as a valuable case study of common feminist responses to claims of biological sex difference, a methodical approach to sex difference theory as it applies across specific domains of human and social attributes, and a useful organisational model for my precis of this literature. I will demonstrate this throughout by drawing upon the more recent feminist work in this field, before moving on to deal with feminist responses to science not addressed by Oakley.

Oakley begins her examination of ‘the biology of sex’ with a discussion of the role played by hormones in physical development through the life cycle from conception to old age in both sexes. She then notes:

What other sex differences are determined by biology? Genetic maleness is correlated not only with the possession of penis and testicles, but also with greater size, weight and strength. These are, in fact, the sum total of the advantages produced by the Y chromosome (1972, p. 27).

This comment is mirrored by early reactions to the discovery of the small size of the Y chromosome (<23 million base pairs) (ORNL, n.d.-a), and its relatively small number of genes (~ 78 protein-coding genes) (Skaletsky, Kurodo-Kawaguchi, Minx, Cordum, & et al, 2003; and see, for example, Maureen Dowd, 2003). It is interesting to note how quickly the Y chromosome was re-positioned, however, as a “goldmine for understanding chromosome evolution” (Whitfield, 2001, final para), as “providing the answers to some very big cultural questions”, and as “the wild frontier of the genome, where strange and important things happen” (Burne, 2001, para 1, para 3).

23 Although the text was ‘revised’ and reissued in 1985, the two versions appear to be identical (the 1985 version even including typographical errors as they appear in the 1972 version). The only revision seems to be the inclusion of a new Introduction to the later text, in which Oakley notes that “nothing that has happened since 1972 would lead me to alter the basic themes” (1985, p. 6).
However that may be, Oakley goes on to elaborate on her claim, citing the many (still existing) disadvantages of being born male: among them a greater vulnerability to genetic disorders, a higher risk of foetal and infant mortality, and a lower life expectancy. She does not (as many of her successors have) ask why women should have been constructed by society as the weaker sex, but goes on to deal with hormonal effects on behaviour in fairly short order. Without denying that hormonal change occurs, in what has become the standard manoeuvre for dealing with this argument for biological influence, Oakley proposes that the pressure to adopt specified societal roles is just as likely to explain behavioural change as would the impacts of hormonal changes in either sex during adolescence (1972, pp. 27-48).

From this starting point, Oakley moves through a succession of theories about sex differences across the six domains of personality, intellect, sexuality, social roles, sex and gender, and gender roles. Foregrounding her discussion of sex and personality by noting somewhat wryly that only “the everyday observation of men and women in society is needed to ‘prove’ that differences of personality follow the biological differences of sex” (1972, p. 49), she proceeds to agree that personality tests do show, on average, that men and women differ temperamentally. She challenges only the conclusion that these differences can be reliably shown to be an outcome of either biology or culture. Noting in passing that research into sex-differentiated toy preference in young children would tend to support a finding of either biological or very early cultural

---

influence in shaping behaviour, Oakley turns to evidence from other cultures, comparing the ways in which ‘masculine’ and ‘feminine’ behaviours and personality differences are defined there. Drawing on anthropological studies across a number of tribal groups, she demonstrates a wide variability in beliefs, expectations, and outcomes.25

Turning then to more specific research, Oakley first looks at sex differences in aggression, as a “key word in the literature on sex differences” (1972, p. 64). She discusses a number of studies that tend to show greater aggressiveness in males than in females. In keeping with her general approach of presenting both sides of the argument, she says that one interpretation “might reasonably be that the biological drives responsible for this sex difference account for the remarkable consistency in female-passive and male-aggressive behaviour” (1972, pp. 64-65). Other possibilities, however, lie in differences in parental treatment, and socialisation from peers, the school system, and society generally. As important, she notes, are the different ways in which males and females display aggression, and how the links between social role and expectation and aggression play out in differing patterns of behaviour.

Writing soon after the publication of Oakley’s revised text, Alice Eagly & Valerie Steffen conclude from their meta review of the literature on aggression and gender that “aggression sex differences are a function of perceived consequences

25 Oakley relies for much of her discussion on the work of anthropologist Margaret Mead, whose work in Samoa was later discredited (after Mead’s death; see Freeman, 1984, 1999), igniting a fierce debate in anthropological circles (see, for example, Feinberg, 1988; Leacock, 1988; Levy, 1984).
of aggression that are learned as aspects of gender roles and other social roles” (1986, p. 309). More recent research on aggression does indeed show that females are far from being passive, but that aggressive behaviour is usually expressed differently by females. Nicki Crick and Jennifer Grotpeter (1995), for example, have found that girls are more relationally aggressive than are boys. Kaj Bjorkqvist and colleagues distinguish between direct and indirect aggression as a “definite phenomenon, at least during adolescence” (1992, p. 127) with the latter being more often practised by girls. Further research confirms these findings, but extends them to investigate the strategies of intrasexual competition between women, including indirect aggression (Campbell, 2002), and other studies indicate that while women are more likely to deploy indirect aggression, they are also more likely to inhibit and “divert their aggression” through the use of defusing strategies (Campbell & Muncer, 2008, p. 282). Without drawing any conclusions as to whether the gender roles adopted by females and males are the result of socialisation or evolution, Anne Campbell and Steven Muncer do conclude that gender roles, including differing beliefs about aggression, are associated with sex-differentiated acts of, and responses to, aggressive behavior.

Moving then to a discussion of sex differences in intelligence, Oakley begins with the comment that:

'It used to be a tradition among anti-feminists that the smaller size of the female brain was proof of woman’s inferior intelligence (1972, p. 79).

This is an aspect of the debate over intelligence that Oakley feels safe to dismiss in 1972, but, as I discuss in chapter 3, it is one that has returned with some force in recent years and must, once more, be confronted. Other findings in sex
differences in intelligence not disputed by Oakley in 1972 are also consistent with those being researched and reported today: differential skill is found in verbal ability (favouring females); number ability (favouring males in numerical/arithmetic reasoning and females on tests of computation); spatial ability (males); and creativity – defined as the ability to “restructure a problem in a new way” (1972, p. 80) – including analytic ability (males are less field dependent than are females).

Oakley discusses the way in which these disparities in spatial and creative ability are demonstrated by Erik Erikson’s observations of the use made of space by girls and boys when playing. He concludes, she says, that girls focus on inner space, simpler structures, lower enclosures; whereas boys construct more elaborate, higher, outer space structures, with a correspondingly greater potential for accident and ruin (1972, pp. 83-84). Observing that “not surprisingly, Erikson, as a psychoanalyst, considers that this different use of space by males and females is a reflection of their anatomy” (1972, p. 84), Oakley herself finds that these differences in spatial orientation ‘echo’ differences found in field dependence studies, but draws no further conclusions.

This question of sex differences in the use of space is, however, later taken up by Iris Marion Young in her classic essay, ‘Throwing like a girl’ (1990). Young argues, contra Erikson’s linking of it to analytic ability, that the different ways in which girls use space is an outcome of the experience of female existence, and of our socialisation. Girls are told we are more fragile, to stay ‘clean and tidy’, to
play ‘nicely’, and, unlike boys, girls are not encouraged to engage in physical contact sports or dangerous pursuits. As well, female body existence, according to Young, is characterised by our experience of our bodies as both subject and object. That is, we experience our body both in its subjectivity and as a thing which is looked at and acted upon by others. The knowledge of the other’s gaze becomes part of our own body image. Because women are, in effect, physically handicapped by the social construction of our bodies under patriarchy, the use of enclosed space forms a barrier within which we can move freely and which also provides a kind of defence against invasion.26

Reflecting further on the differences that have been found in IQ testing, Oakley makes the point that “demonstrated intellectual achievement” tells another story:

Throughout school life girls achieve better results than boys, and generally do better in examinations. If this intellectual achievement is compared with aptitudes for specific subjects, it is clear that girls do better even in subjects where boys tend to have the greater aptitude (1972, p. 85).

However, the subsequent achievement of women in the workplace reflects a considerable disparity between measured IQ and achievement. Noting that the ‘drop-off’ in achievement for girls begins around puberty, Oakley proposes two possible causes: accelerated hormonal activity, and inculcation of adult social roles. While much of her following discussion around the differing expectations for career and marriage is, to a large extent, now outdated, her conclusion about parental and societal sex role modelling and expectations as strongly implicated as explanation for these disparities are still valid today, and similar conclusions

26 See also Janice Haaken’s paper, ‘Field dependence research: a historical analysis of a psychological construct’ (1996).
have been drawn in a variety of recent feminist treatments (see, for example, Halpern, 2000; Hines, 2004; Hird, 2004; Kaplan & Rogers, 2003; Rogers, 2000, among many others).

Oakley does allow for the possibility of biologically based causes for differences in intellectual ability, including the different developmental timetables of boys and girls, but maintains that this does not explain differences in spatial ability, nor the disparities between measured IQ and achievement. Conceding that it could be held that “the intellect is to some extent genetically controlled by the sex chromosomes” (1972, p. 95), Oakley makes the further point that the mechanism for control, if it exists, is unknown. That being so, she recommends cross-cultural studies into patterns of intelligence, making the “intelligent guess” (1972, p. 98) that such a study would not find universal sex differences. Instead – and in line with similar arguments made later by Carol Bacchi (1990), and by corporeal theorists among others – she proposes that any such variations would more likely be an outcome of “the tremendous variation in individual endowment among human beings generally” (Oakley, 1972, p. 98).

Oakley next considers the question of whether there are innate differences governing the expression of sexuality in men and women, that is, that “male sexuality arises spontaneously and is specifically genital while the female’s is not” (1972, p. 107). Again, she draws on anthropological research to illustrate cross-cultural differences in the meanings given to physiological events such as puberty, menstruation, sexual play, intercourse, and orgasm, demonstrating that
sex-differences in sexual behaviour are not universal and are therefore unlikely to be genetically determined. Instead, she points to (Western) society’s stereotypical expectations of sexual behaviour and the greater restrictions on, and control over, female sexuality – circumscribing the physical movement of girls and women through parental controls; fear (for example, of pregnancy, assault or rape), or via the psychological deterrent of the sexual double standard (‘nice girls don’t’). As Sarah Blaffer Hrdy notes, “Almost universally, sexual sanctions are stricter for women than for men” (1999, original publication 1981, p. 177). Hrdy goes on:

> Even societies which appear to esteem women for their sexual purity and passivity nevertheless take extensive precautions to prevent them from breaching their chastity. On one point there is an extraordinary consensus: woman’s readiness to engage in sexual activity is great enough that the majority of the world’s cultures ... have made some effort to control it (1999, original publication 1981, p. 177).

Hrdy’s recognition that women’s attitudes to and physiological responses to sexual matters are “profoundly influenced” by social expectations – even to the extent of cultural variance in the enabling or inhibiting of orgasm (1999, original publication 1981, pp. 162-163) – supports Oakley’s earlier proposal that the “long conditioning of the female to a particular kind of sexual response (or lack of response)” leads to the conclusion that differences in sexual behaviour can more certainly be attributed to conditioning and learning than to any innate factors (Oakley, 1972, p. 126). Rather than speculate on such matters as the origins of the female orgasm, Oakley says, it is “surely much more important to study the

---

27 Again, she relies in part on research conducted by Margaret Mead.
28 Although pregnancy is not as much a deterrent in these days of readily available contraception, it still figures in the control of (particularly teenage) girls, especially those for whom, for one reason or another, access to adequate ‘safe sex’ information or medical advice and treatment is still difficult.
whole moulding of personality and sexual response by culture, and in different forms of human society” (1972, p. 127), and in a clear illustration of the need to revisit the same arguments over time, as we will see later in the thesis, Marlene Zuk sees the need to make a very similar point nearly three decades later (2002, p. 152). Sarah Hrdy also notes the changes in attitudes to the female orgasm within Western society over the three decades between 1948 and 1980, observing that “Few generalisations about sexuality apply cross-culturally” (1999, original publication 1981, p. 163).29 Ruth Hubbard, in her discussion of the social construction of sexuality, also explicitly states that “There is no ‘natural’ human sexuality” (1990, p. 130). Other contemporary discussions can be found in Meredith Small’s work on female choice (1993), Anne Fausto-Sterling’s extended treatment (2000c), and in Elisabeth Lloyd’s essay on bias in evolutionary science (2008).

Similarly, in her discussion of social and gender roles, Oakley reveals the ways in which ascriptions of a ‘natural’ division of social labour rest on assumptions made, and links drawn, between economic and reproductive tasks. These revolve around the characterisation of the work of motherhood and domestic tasks as time-consuming and all-encompassing – but at the same time, “relatively sedentary”, and “marginal” to the economy – and a further positioning of women as physically weaker than men (1972, pp. 130-131). In the following pages, Oakley convincingly dismantles each of these assumptions.

29 Also see Hrdy’s bibliographical update to chapter 8, regarding more recent research than was available at the time of original publication of The woman that never evolved (1999, original publication 1981, pp. 249-251).
She first demonstrates that, cross-culturally, parenthood is enacted in various ways, often with child care undertaken by both parents, and concludes that “putting child care in the hands of women alone is not necessarily the best way of doing things, nor the most natural, nor should we think of it as a rule which holds in all but a few insignificant and peculiar cultures” (1972, p. 136). As well, claims that child care and housework are largely sedentary occupations, and that women (because they are physically weaker than men) are best suited to these tasks, are rendered nonsensical by an appeal to what those of us (of both sexes) who perform domestic tasks and have children all know:

… child care is physically exhausting and mentally demanding work: it needs tireless vigilance, a great deal of energy, and a kind of protectiveness and responsibility that is aggressive rather than passive, implying as it does the ability to stand up for the child and fight for its rights in a world that is not always sympathetic. In particular, the physical strength needed in child care is by no means negligible … Traditionally domestic work has required – and in unmechanised cultures continues to require – considerable physical stamina and ability to carry loads … the idea that the males do the ‘heavy’ work while the females are occupied with ‘light’ domestic tasks seems to be a merely fictional opposition between two forms of work which both demand energy and strength (1972, pp. 137-138).

The sexual division of labour is, Oakley concludes, a result of convenience and precedent, based on beliefs about feminine, masculine and parental roles. These are “matters of culture, evolving without any necessary reference to biology” (1972, p. 146), and varying between cultures, based on culturally determined beliefs about reproductive function. Such beliefs then spill over into the world outside, and affect the roles that women and men take up (or are enabled to take up) in education and in the paid workforce. In her work on motherhood, although based in biology, Hrdy draws similar conclusions to Oakley in regard to the ‘naturalness’ of the expectation that women should be primarily or solely responsible for child care. Anthony McMahon (1999) also provides a relatively
recent and insightful critique of these beliefs in the era of ‘the new man’ and ‘new fatherhood’.

Finally, as a precursor to much recent work on the question of the ‘naturalness’ of gender (see, in particular, Fausto-Sterling, 2000c; Hird, 2004; Kessler, 1996), Oakley draws upon research with intersex individuals to support the contention that the psychological characteristics associated with ‘feminine’ and ‘masculine’ behaviour are not necessarily linked to female and male biology. She concludes, on the basis of studies with hermaphroditic individuals, that, in general “gender identity is established early and usually irreversibly” (1972, p. 164), and its development follows not from our biological make-up, but from a combination of role identification and imitation, differential parental responses to their male and female children, and social classification and stereotyping. On the biology of gender, Oakley says, finally:

If gender has a biological source of any kind, then culture makes it invisible. The evidence of how people acquire their gender identities, taken together with the facts set out in the previous chapter [on research into intersex individuals], suggests strongly that gender has no biological origin, that the connections between sex and gender are not really ‘natural’ at all (1972, pp. 187-188).

Despite the large volume of contemporary research that appears to confirm Oakley’s view, the ability to separate sex from gender via social means is once again being questioned. The story of David Reimer, the boy who was reassigned and raised as a female as the result of a bungled circumcision (ablatio penis), and reverted at age 15 to living as a male before taking his own life in 2004 at age 38, has been widely-reported as disproving this contention (see Colapinto,

---

30 But I note in passing the ubiquitous use of ‘gender’ on official and other forms to indicate one’s biological sex (ie, female/male).
However, Theresa Wizemann & Mary-Lou Pardue note that interim results from ongoing studies of boys reassigned as girls are equivocal – while “more than half” identify as boys, “consistent with their male-typical prenatal androgen exposure, and not with their female-typical rearing”, some of the children accept their female identity (2001, p. 84). The relative contributions of nature and nurture to our gendered identities are, then, still uncertain, and any claim for an unquestioned ‘natural’ connection between sex and gender therefore seems premature.

The publication of *Sex, gender and society* in 1972 – prior to the publication of Edward O. Wilson’s *Sociobiology: a new synthesis* (1975) and Richard Dawkins’ *The selfish gene* (1976), and the subsequent upsurge of sociobiological theories – precluded Oakley from specifically addressing ‘sociobiology’ in this work. Similarly, her book predates the explosion of genetic information in the nearly two decades since the inception of the Human Genome Project (HGP) in 1990. Confronting these more recent developments, of necessity, has been left to others, and many have taken up the challenge.

Representative of most of those who confronted (and continue to confront) sociobiological theory are texts produced in the early 1980s by feminists Janet Sayers (1982) and Ruth Bleier (1984). Again, both are early exemplars of feminist responses to sociobiology. Sayers is widely recognised as having

---

31 And nor does the 1985 ‘revised’ edition deal with questions of sociobiology. She has more recently, however, described it as a ‘delusional system’ (see Oakley, 2002).
32 Ruth Bleier trained in medicine and neuroanatomy, and developed a strong interdisciplinary interest in feminist analysis of science. Janet Sayers trained as a clinical psychologist.
inspired other feminists with her groundbreaking work, *Biological Politics* (1982), while Bleier was among the first to combine a successful career as a working scientist with a proactive involvement in feminist approaches to scientific knowledge. The fine detail of the research discussed by these authors, and others like them, is not rehearsed in great detail here because it by and large falls within those general categories covered by Oakley. That is to say, both Sayers and Bleier specifically challenge research into sex differences of personality, aggression, social and gender roles, the effects of sex hormones on behaviour, and beliefs about human universals.

Unlike Oakley, however, Sayers and Bleier are explicitly in conversation with and against social Darwinian and sociobiological theory. Their expressed purpose is to expose, in Sayers’ words, the “way in which those opposed to changes in women’s social role have sought to appropriate biology for their cause” (1982, p. 1); or, as Bleier puts it, the conservative “mythology of women’s biological inferiority as an explanation for their subordinate position in the cultures of Western civilizations” (1984, p. vii).

In a text that is equally concerned with the problem of sociobiological theory, and with feminism’s response to it, Janet Sayers devotes five of her nine substantive chapters to those sociobiological theories she identifies as most damaging to

---

33 See, for example, the 2004 Special Feature “Biological politics: feminist and anti-feminist perspectives – a reappraisal” in *Feminism & Psychology*, 14:3, edited by Colleen Heenan, and including papers by Lynda Birke, Linda Gannon and Hilary Rose, among others, as well as a response and afterword by Janet Sayers.

34 In Sayers’ case, with historical as well as contemporary social Darwinism.
women. She first takes on W.D. Hamilton’s theory of inclusive fitness, whereby the altruism of individuals (such as the protection of children and kin) is recast as a selfish desire to ensure the survival of their genes. She claims that this reduction of altruism to ‘self-interest’ is taken a step further by Robert Trivers’ theory of parental investment, later endorsed by Richard Dawkins (see Dawkins, 2006; Hamilton, 1972; Trivers, 1972; Trivers, 2002). This theory proposes that, while both sexes will attempt to maximise their reproductive success, women will invest more in their offspring because they are more directly involved in reproduction and, crucially, because they provide the largest gamete and thus the greatest resources. This, in turn, leads to differing mating strategies (males are more promiscuous, females are ‘choosier’), and to strategies designed to “exploit and outwit” each other (Sayers, 1982, p. 53). Emily Martin (1991) later expands upon this particular argument in her influential paper, ‘The egg and the sperm: how science has constructed a romance based on stereotypical male-female roles’.

Asserting that sociobiological theories of inclusive fitness and parental investment are flawed, Sayers argues that Trivers and Dawkins gloss the differences between their own theories and Darwin’s theory of evolution, in two significant ways. To begin with, she claims they mistakenly argue that natural selection favours the maximisation of reproductive success (more offspring), in contrast to Darwin’s notion of relative reproductive success (more offspring compared to others of the same species). Further, she contends that Trivers and Dawkins suggest ‘fixity’ in behavioural adaptations, contrary to Darwin’s
concept of a system open to change and chance variation (1982, p. 55). Sayers then goes on to demonstrate that Trivers’ account of human sex roles relies on “social – not biological – presuppositions” (1982, p. 56) and that both he and Dawkins rely upon circular reasoning. She makes the case that wife guarding to protect property inheritance is nonsensical across classes and societies where there is no ownership of property; that labour power is not freely alienable and therefore the notion of mutual exploitation relies upon “forms of relatedness” that are not universal (1982, p. 60), and finally, that there is no reason to suppose that a female’s prior parental investment would be any more likely than a man’s to secure an offspring’s future. In sum, she says, sex roles in childcare, relations between the sexes, and the double standard, far from being biologically determined and fixed, are dependent on the “existence of particular forms of society” (1982, p. 64).

Turning next to theories on dominance via greater innate male aggression, Sayers asserts that this theory, expanded on by E.O. Wilson in *Sociobiology: a new synthesis* (1975), rests on the uncertain hypothesis that competition results in success. After discussing the historical antecedents of this idea, Sayers rejects the arguments that men’s greater “biological propensity for aggression” (1982, p. 81) is the basis for men’s dominance, based as they are upon comparative animal studies and, in particular, the linking of the activity of hunting with contemporary male wage-earning activities (1982, p. 79). She argues that it is by no means clear that dominance is dependent upon aggression, or that it is competitive in nature. Further, she notes that social organisation is not
universally hierarchical, and concludes that male dominance is a “learned phenomenon, a response to the material conditions of life” (1982, p. 82).

More recently, Lynne Segal has written that “Few things are more depressing for me to have to write about than the renaissance of a Darwinian fundamentalism” (1999, p. 80). Like Sayers, Segal takes Richard Dawkins and E.O. Wilson to task for their attempts to characterise all human behaviour as based in a reproductive process which ensures that males will be “aggressive, hasty, fickle and undiscriminating”, while females will be ‘coy’, holding back “until they can identify the male with best genes (1999, p. 84; see also Hubbard, 1990, pp. 117-118). Describing the arguments of sociobiology as “circular” and “anthropomorphic” (1999, p. 85), she also confronts what she calls the “shallowness” of similar arguments about mating strategies emanating more recently from evolutionary psychology, at the same time noting the convergence of many of these arguments with cultural theory (1999, pp. 86-94).

Other recent feminist scholarship similarly endorses and extends Janet Sayers’ conclusions. Research undertaken by Janet Shibley Hyde demonstrates that where gender differences are found in aggression, they are dependent on methods of measurement and study setting, concluding that the “variation in results is consistent with social-role theory” (1996, p. 319). Ruth Hubbard concurs that “competition and dominance hierarchies are not characteristic of all human societies” (1990, p. 118), and, elaborating on this insight, Segal points out that it is the competitive inequalities and differences *between men* that are most often
destructive within manifestations of dominance. Rather than an innate aggression, she demonstrates that male dominance is a function of a social hierarchy, where men compete with each other, and where “boys in caring, non-violent households in non-violent neighbourhoods, are hardly more likely to be violent than girls from similar backgrounds” (1999, p. 162).

Ruth Bleier is more strident in her criticism of sociobiology than is Sayers: it is, she says, “deeply flawed conceptually, methodologically and logically as a science” (1984, p. 16). In a discussion that ranges widely through the sociobiological literature, and confronts much the same ground as Sayers, she highlights the flaws she detects as: ethnocentricity and anthropomorphism; the “absence of any precise description or definition of the behaviors Sociobiologists are seeking to explain” (1984, p. 26); the omission of data that fails to support the argument; the ‘manipulation’ of language and logic, and, in common with Sayers and Segal, the kind of circular logic that is “validation by prediction” (1984, p. 38). Despite her acknowledgement of genetic and hormonal influences on developing human brains, her politics are evident:

Distinctions of human characteristics and temperaments into innate male and female natures have been social, cultural constructs and are not natural. They are part of an ideology that attempts to make what are in fact social and political distinctions appear to be natural and biological and, therefore, to justify differences in social roles and also relationships of dominance and subordinance (1984, p. 7).

That such scepticism about and opposition to sociobiology is widespread is clear: for example, Ruth Hubbard asserts that sociobiology does not sufficiently...

---

35 As I note in chapter 4, throughout this thesis, I use the umbrella term ‘sociobiology’ to encompass both ‘classic’ or Wilsonian sociobiology, and evolutionary psychology; however, as I make clear also in that chapter, feminist sociobiologists such as Sarah Hrdy are critical of and have distanced themselves from the evolutionary psychology approach to sociobiology.
consider cultural and historical circumstance in its universalising of ‘reified’ human traits (1990, p. 112); Hilary Rose accuses sociobiology of attempting to colonise “social science under the banner of biology” (2000); Lesley Rogers states that “sociobiology is a mode of thinking that is based entirely on reductionism” (2000, p. 45), and Barbara Herrnstein Smith takes evolutionary psychologists Steven Pinker, Leda Cosmides and John Tooby to task for being “captive to an unregenerate Two Cultures mentality, with its familiar intellectual provincialisms and disciplinary antagonisms” (2000, p. 139). Zuleyma Tang-Martinez specifically targets those she terms ‘sociobiological feminists’, wondering whether they have, despite their “very important contributions”, added anything to the more usual approaches to ending sexist oppression. She says:

The main problem with sociobiological feminism is that it draws from a discipline that is not only scientifically suspect but also has been, and will continue to be, used to justify male dominance and sexist oppression ... Sociobiological feminists should guard against becoming so enamored of their method that they fail to realize that they are helping to legitimate a field that inherently justifies and condones male domination, Western patterns of male-female gender roles, and many other forms of social inequality (1997, p. 143).

Feminists who deal specifically with new knowledge emerging from post-genome research, as distinct from sociobiological theories, are similarly sceptical of the links that are drawn between genetics and behavioural attributes. Thus, many feminist responses to the HGP, and the work in molecular biology that follows from it, are most focused on its reductive vision and promise of control.

36 Defined as feminists who use “a sociobiological approach, informed by a feminist perspective, to reinterpret data” and those who use “a sociobiological methodology and analyses in an attempt to understand the origins of male domination and female oppression” (Tang-Martinez, 1997, pp. 117-118). This definition would include, for example, Sarah Hrdy, Meredith Small, Patricia Gowaty, Barbara Smuts, among others.
Dorothy Nelkin and Susan Lindee, for example, draw attention to the powerful metaphors surrounding the project (Nelkin, 1993b; Nelkin & Lindee, 1995a). Ruth Hubbard is similarly concerned with “genomania” and “genetic mythologizing” (1999, p. xvii), calling the goals of the HGP “reductionism at its most extreme” (1999, p. 3) and asserting that claims of cures for disease as an outcome of the achievement of these goals are “misleading” (1999, p. 65). Gisela Kaplan and Lesley Rogers claim that molecular genetics (and evolutionary psychology) are founded upon “many flawed and grandiose assumptions” (2003, p. 10). However, they themselves base much of their argument on their own assumption that reductionism promotes a vision which attempts to “explain behavior entirely in terms of hormones or genes” (2003, p. 21, my emphasis). As I attempt to show in this thesis, some forms of reductionism do not inevitably equate (as much critique would have it) with immutability. Evelyn Fox Keller, herself an early critic of the genome project, acknowledges that, as a result of the knowledge arising from it, she is now willing to:

... celebrate the surprising effects that the successes of this project have had on biological thought. Contrary to all expectations, instead of lending support to the familiar notions of genetic determinism that have acquired so powerful a grip on the popular imagination, these successes pose critical challenges to such notions (2000, p. 5).

Keller’s insight into the new potential for change in our ways of thinking about determinism – and thus, I suggest, sex difference research as a whole – supports the case made by others for a reflexive feminist ‘antiessentialism’ in much of the critique directed at reductionist science (Wilson, 1998; see also Fuss, 1989; Kirby, 1997); that in fact the restricted vision created by ideological ‘blinkers’ that is so

37 Discussed in greater detail in the next chapter.
criticised on the part of reductionist scientists can, and does, affect participants on both sides in this debate. Elizabeth Wilson, for example, observes that “[d]espite an avowed interest in the body, there is a persistent distaste [among feminists] for biological detail” (1998, p. 15). She cites the work of Ruth Bleier (1984) and Lesley Rogers (1988; see also 2000) as examples of the ways in which feminist politics intervenes in the “critical habits” of feminists engaging with the science of neurological difference, thus ensuring that scientific politics cannot be thought as anything other than either “objective sites of truth” or “oppressive forces of social control” (Wilson, 1998, p. 17).

In sum, ‘outsider’ feminists who engage with sociobiological and genetic science do walk across the campus to see what is going on in these fields, while ‘insider’ feminists have the (at times difficult) advantage of being able to utilise both their science and their feminism in their critique. The scientific knowledge and insights of this latter group provide an invaluable entrée into an understanding of the more difficult aspects of these fields for the non-scientist. However, reading around the ‘science’, I am in agreement with Wilson that some of the feminist critique suffers from an ideological recoil from biology. Further, I suggest that the terms of this critique from both sides of the campus still builds upon the methodology used by Oakley and Sayers, in particular. That is, most accounts advance their case by setting forth the contested theory; discussing the relevant research; drawing on cross-cultural and anthropological work, and then asserting that even where biological differences exist, they are neither sufficient nor significant enough to explain or excuse the social and political dominance of
men in society. In the end, these accounts do not avoid the ‘trap’ of dualistic thinking in proposing alternative (social) explanations which return us to a dualistic frame. The outcome is often a zero sum game where the social construction argument ‘wins’ via the agency of asserting an ‘on balance’ argument (on balance, innate male/female natures are socially constructed) or an ‘extent’ argument (biological predispositions may exist, but they are completely overshadowed by the extent of social influence) that works to exclude biology.

And so, despite the many differences in tone and approach, and regardless of the many years intervening, the final stance most often adopted on theories of biological sex difference by feminists who engage critically with reductionism is still concisely captured by Ann Oakley:

To sum up, then, we can say that the chief importance of biological sex in determining social roles is in providing a universal and obvious division around which other distinctions can be organised. In deciding which activities are to fall on each side of the boundary, the important factor is culture (1972, p. 156).

Conclusion

The biosocial politics of difference, as they continue to be played out in the lives of real women, almost mandate that feminist body theory will embrace nurture as the defining influence on differential social and behavioural outcomes for women and men. To do otherwise is to risk the most unacceptable consequences of a biologically determinist view of the world, ‘fixing’ us within a naturalised status quo.
Feminists adopt various strategies in their attempts to deal with the intransigent problem of the nature/nurture, mind/body dualisms, and with the challenges posed by reductionism. I have asserted that the most extreme positions of strong essentialism and constructionism, in dealing with the dualism by either adopting or denying it, both effectively make questions about biological sex difference redundant, and are therefore not useful to the questions I wish to ask in this thesis. A third strategy, what I have called the metatheoretical approach, is useful in pointing out the ways in which social realities force us to focus on dualistic alternatives such as sameness/difference, while at the same time highlighting the pervasive nature of dualistic thinking, but again it does not advance my key question.

The final two feminist strategies considered here both struggle with the dualisms. Corporeal theorists are concerned to overcome interpretations of sexual difference that continually return us to the familiar reductionist explanations inherent in the mind/body, sex/gender, nature/nurture splits. They attempt to respond to the concerns underscored by the metatheorists by advancing a notion of the body that is neither a product of social inscription, nor of biological prescription, but of something in between. That they cannot, in the end, avoid re-situating the body within the realm of the social world illustrates both the slipperiness of dualistic thinking and the strength of our recoil from allowing biology any meaningful toehold in the formation of sex-differentiated psychological characteristics.
Such biological explanations of sex differences, and the reasons why we recoil so vehemently from them, are the focus of those feminists who, as insiders and as outsiders, challenge the models of human behaviour advanced by the reductionist sciences. Their critique of these models is informed by insights from either – or in some cases both – the physical and the social sciences, and succeed in exposing and holding them to account for the more outrageous of their determinist claims. Nevertheless, in pursuing an ‘in sum’ or ‘on balance’ conclusion, or, alternatively, a ‘predisposition’ versus ‘extent’ argument, this strategy, like the others discussed here, does not sufficiently come to terms with dualistic thinking to be able to escape it.

As long ago as 1972, Ann Oakley said that the “enduring questions are these: does the source of the many differences between the sexes lie in biology or culture? If biology determines male and female roles, how does it determine them? How much influence does culture have?” (1972, p. 15). That we – feminists and reductionists alike – still pursue answers to these questions in this form, clearly illustrates that we remain caught up in a nature/nurture, social construction/genetic reductionism model.
Part two: The vampires in the sacristy

Biology is the key to human nature.  

Both the text and the notes stride through some disciplines and territories in which I’m not qualified as an expert; so I’m bound to have got some things wrong, although I believe I’ve also got some right. Straying into these ‘expert’ lands is something I’m convinced we all must do …  
Two
Genetics, genomes and control: biology’s Holy Grail

A more important set of instruction books will never be found by human beings. When finally interpreted, the genetic messages encoded within our DNA molecules will provide the ultimate answers to the chemical underpinnings of human existence.

James D. Watson (1990, p. 44)

The Human Genome Project is the quintessential reductionist endeavor.

Allison Morse (1998, p. 225)

Introduction

The pursuit of the human genome has its roots deep in a human history that documents an insatiable desire to interrogate not only the world around us, but also the world within. The Human Genome Project (HGP), officially begun in 1990 and completed in 2003, was seen by many as the culmination of millennia of wonder and conjecture about what it is that makes a human being. The project’s goal of identifying and sequencing all of the genes in human DNA held out the promise of answering this question and, in so doing, providing us with an unparalleled level of control over the future of human existence.

The promise of such control over our biological destiny provokes division amongst those who consider what it might mean to be living in the post-genomic
era. For the scientists involved in the project, the post-genomic era means that we humans now have available to us the ‘book of life’ and all that remains is to learn how to read it. Their hope is that reading and understanding the genetic code contained in the DNA of all living things will allow them in effect to ‘edit’ the text, forestalling, curing and eventually eradicating genetic disease; they will be able to mend, alter, and even create life. So, whilst acknowledging the legitimate concerns surrounding ethical, legal and social issues, for scientists the knowledge arising from the Human Genome Project provides for an exciting and positive future. The goals of the project were, without question, biology’s Holy Grail.

On the other hand, for many feminist and other critics and commentators, the post-genomic future brings with it the worrying prospect of increasing genetic surveillance, a new eugenics, social upheaval and biological reductionism. Feminist commentators, in particular, have noted how the metaphoric discourse around the Human Genome Project is steeped in the rhetoric of control, and this has been extensively documented (for example, Doring, 2005; Gogorosi, 2005; Hellsten, 2005; Keller, 2000; Kerr & Cunningham-Burley, 2000; Morse, 1998; Nelkin, 2001). Metaphors such as the ‘master molecule’ and the ‘master code’, the ‘book/code/language of life’, ‘our instruction book’, a map, a blueprint, the ‘blueprint of destiny’ – each of these can be read as a language of determinism, implying a genetic mechanism that both makes and controls us. A step further removed is a concern that once scientists have ‘cracked the code’, control over individual humans as machines that are (or can be) programmed, books or blueprints that can be read (or rewritten), will pass to them.
The construction of (mostly male) genome scientists as explorers, questers and heroes setting out on a voyage of discovery is also endemic to the literature around the genome project. The home page for the project on the National Human Genome Research Institute (NHGRI) website begins:

The Human Genome Project (HGP) was one of the great feats of exploration in history – an inward voyage of discovery rather than an outward exploration of the planet or the cosmos; an international research effort to sequence and map all of the genes – together known as the genome – of members of our species, *Homo sapiens*. Completed in April 2003, the HGP gave us the ability to, for the first time, to (sic) read nature’s complete genetic blueprint for building a human being (NHGRI, n.d.-a, para 1).

This paragraph deliberately draws parallels between the project and such things as the moon landing, the more recent journeys of NASA probes to Mars and the outer planets and, in the American context, with the terrestrial journeys of exploration undertaken by Columbus and the founding fathers.

Mary Rosner and T.H. Johnson, in their own exploration of the metaphors of the project, assert that “the Project intends to chart nature and to ‘discover’ certain kinds of information there” (1995, para 4), and go on to state that:

... by comparing nature to things inanimate, dehumanized, and ordinary – books, machines, and territories – and the scientist to the librarian, mechanic, and explorer, [James] Watson (and he is not alone) promises a heroic and patriarchal tale. In this tale, nature is a distanced and impersonal Other that science can, should, and will subordinate (1995, para 5).

Similarly, Ruth Hubbard argues that the selection of ‘the Holy Grail’ and ‘the book of life’ as metaphors for DNA “underlines the ideological content of molecular genetics” (2003, p. 794), which is presumably an ideology of patriarchal conquest and control.
Likewise, Dorothy Nelkin & Susan Lindee argue that the appeal of ‘the blueprint of destiny’ lies in its seductive promise of predictive science and its justification of ‘social agendas’. They go on to discuss the social implications of predictive typing and risk assessment for individuals, concluding that this may “create an underclass of individuals whose genes seem to have marked them for the nowhere track” (1995a, p. 67) and that “the world view of genetic essentialism leads to policies that restrict the … rights of individuals, for it suggests that order in a society depends on the genetic qualities of its population” (1995a, p. 68).

In her discussion of DNA as the ‘master molecule’ of life, Evelyn Fox Keller focuses on the ways in which knowledge of the process reverses our understandings of nature as destiny and nurture as freedom, leading to a belief that we can now “more readily control the former than the latter” (1993a, p. 288). This view of nature as malleable in turn leads to a problematising of the concept of health, such that molecular biology searches not for those traits accepted as ‘normal’ but rather for the genetic basis of ‘unhealth’. She concludes that eugenics “has become a vastly more realizable prospect” through our own complacency about changing definitions of normality (1993a, p. 299).

The construction of the gene as the ‘master molecule’, of course, is the most fundamental of all metaphors. Although acknowledged to be a convenient shorthand for intricate concepts, critics and commentators on genetic discourse take some trouble explaining that the ‘gene’ as a discrete entity does not in fact exist (Hubbard, 2003; Keller, 2000; Lewontin, 1993, 2000b). It seems clear from
recent literature, and especially given the pace of new discovery, that even geneticists can no longer agree on what a ‘gene’ actually is, other than to agree that it is far more complex than the popular conception of it as a bead on a string of DNA.\(^3\) Nonetheless, the gene is still reified in popular culture as the immortal master molecule that directs and controls, at the very least, our physiology, and even possibly our psychology. In *The century of the gene*, Keller notes that the idea of ‘gene action’ had:

… endowed the gene with a most curious constellation of properties. At one and the same time, the gene was bestowed with the properties of materiality, agency, life, and mind (2000, p. 47).

The use of such simplified metaphors helps to create an everyday understanding of the gene as a causal agent and the genome as a master code, an understanding that in turn impacts on concepts of individual, racial, ethnic, sexual/sexed and kinship identities. This can lead to a belief that there is now scientific support for the theories about biological sex difference and the more reductionist sociobiological theories discussed in the next chapters in this thesis. It is useful, then, to walk across the campus and look beyond the metaphors surrounding the HGP, to the history of modern genetics, the project itself, its objectives, and its actual outcomes and projected benefits.

---

\(^3\) I take up the discussions around what a ‘gene’ actually is in Chapter 4 of the thesis.
Background to modern genetics

Alfred H. Sturtevant makes the point that the beginnings of genetics\textsuperscript{39} go farther back even than Aristotle (2001, first published 1965, p. 1), and Portugal & Cohen state that:

Two thousand years before Miescher discovered DNA [1869], the ancient Greeks had speculated on the process by which male and female genital secretions gave rise to an organism that in appearance closely resembled the parents (c1977, p. 90).

Speculation about human genetic inheritance is, then, by no means a novel pursuit. Most historians and commentators place the shift from speculation to ‘science’ in the modern beginnings of genetics, with the work on particulate inheritance in pea plants by Gregor Mendel (1822-1884), the Austrian monk often called the first geneticist, or the ‘father of genetics’ (Judson, 1993; Moore, 2001; Orel, 1996; Portugal & Cohen, c1977; Sturtevant, 2001, first published 1965; Tudge, 2000; Watson, 2003b). Mendel’s work was published in 1866,\textsuperscript{40} but went largely unnoticed until simultaneously rediscovered by three independent researchers in 1900.\textsuperscript{41} His laws of inheritance – dominance, segregation, and independent assortment – are widely accepted as providing the basis for modern genetics (The Columbia Electronic Encyclopedia, 2005; Tudge, 2000), although Sturtevant comments that the “systematic study of the genetics of man (sic)” began earlier, with Francis Galton (a first cousin of Charles Darwin), in 1865

\textsuperscript{39} The introduction of the word ‘genetics’ is attributed to William Bateson, in 1905 (Portugal & Cohen, c1977, p. 118). Although anachronistic prior to that time, for purposes of clarity I use the word throughout when discussing this field of research.

\textsuperscript{40} There is some confusion in the literature as to year of publication, but it seems clear that the paper was read in 1865 at meetings of the Natural History Society of Brunn, and published in the Proceedings of the Natural History Society of Brunn in 1866.

\textsuperscript{41} Carl Correns in Germany, Hugo deVries in Holland, and Erich Tschermak in Austria (Kevles, 1986, p. 43). See (http://www.esp.org/foundations/genetics/classical/browse/chrono-lst.html) for facsimiles of their original papers.
Mendel’s overlooked paper was followed by numerous other incremental discoveries in the 19th and early 20th centuries, including Johann Friedrich Miescher’s discovery of DNA in 1869, Walther Fleming’s observation of what would later be known as chromosomes, and Hugo de Vries’ development of a concept of intracellular communication which would later be largely confirmed by the discovery of messenger RNA (Portugal & Cohen, c1977). James Watson identifies Walter Sutton’s discovery in 1903 of the Chromosomal Theory of Heredity as another important step forward in the development of a science of genetics (2000, p. 180) and, from 1907, Thomas Hunt Morgan’s famous ‘fly room’ at Columbia University provided both important developments in genetics (through studies of *Drosophila*) and a research home for young geneticists such as Sturtevant and Hermann J. Muller (Portugal & Cohen, c1977, p. 124). In the early 1950s, Maurice Wilkins’ and Rosalind Franklin’s work on x-ray diffraction patterns pointed to a helical structure for DNA (c1977, pp. 240-246), providing a critical step toward what is probably the 20th century’s most well known announcement in genetics, that of the discovery in 1953 of the double helical structure of DNA by James Watson and Francis Crick (Watson & Crick, 1953).

---

42 Published in *Cell Substance, Nucleus, and Cell Division* (1882).
43 See de Vries (1910).
44 See Sutton (1903).
45 Sturtevant is credited with having developed the first sequence map (Judson, 1993, pp. 45-47); Muller with demonstrating that x-rays caused mutations in fruit flies (Judson, 1993, pp. 47-48) and Morgan himself with discovering sex-linkage and recombination (Watson, 2003b, pp. 13-16).
46 There has been controversy over Watson & Crick’s access to Franklin’s work without her knowledge (see Maddox, 2002; Sayre, 1975).
Unfortunately for the fledgling discipline of human genetics, much of the early work on Mendelian inheritance was used as support for eugenic beliefs and for the implementation of various negative eugenic social policies in the United States, England and Germany.\(^{47}\) The practice of eugenics, of course, is not new; it is inherent in Darwin’s evolutionary principles, and in his formulation of evolution it is practised by most species instinctively in their mating strategies. It has also been consciously applied by humans to their animal and plant breeding methods for centuries. However, it is the application of deliberative social policies for enhancing so-called ‘good (human) stock’ and, more importantly, for preventing the proliferation of ‘bad (human) stock’, that we think of as eugenic and which gives the word its pejorative colouration. The word ‘eugenics’, literally ‘good birth’,\(^{48}\) was coined by Francis Galton, and defined as “the study of agencies under social control that may improve or impair the racial qualities of future generations either physically or mentally” (Searle, 1976, p. 1).\(^ {49}\)

Right from the beginning, then, knowledge about genetic inheritance has been an ‘applied’ rather than a ‘pure’ science. Galton’s vision of a “planned human genetic improvement” (Hellyer Corning, 1973, p. 732) was enthusiastically seized upon by eugenic movements, which drew much of their credibility, and many of their adherents,\(^ {50}\) from the emergent science of genetics in the early 20th century.

\(^{47}\) James Watson notes that eugenics “proved a disaster for the emerging science of genetics, which could not escape the taint” (2003b, p. 33).
\(^{48}\) From the Greek ‘\textit{eu}’ = good, ‘\textit{genesis}’ = generation, creation (Shorter Oxford English Dictionary on historical principles).
\(^{49}\) Watson also credits Galton with being the first to refer to the “nature/nurture” dichotomy (2003b, p. 21), as does Sturtevant (2001, first published 1965, p. 128).
\(^{50}\) Among them, biologists William Bateson, Charles Davenport (director of Cold Spring Harbor Laboratory in 1910), and Harry H. Laughlin (superintendent of the Eugenics Record Office); Karl Pearson (joint founder of \textit{Biometrika}, and foundation professor of Galton Eugenics at University
Support from scientists was progressively withdrawn as eugenic beliefs, based on insufficient or misinterpreted science, were translated into such abhorrent social practices as forced sterilisations, discriminatory immigration policies, the stigmatisation of whole families as ‘degenerate’ or ‘feebleminded’, and ultimately led to the experimentations and genocidal practices of the Nazis during World War II. Daniel J. Kevles notes that:

In 1935, the American geneticist and future Nobel laureate Hermann J. Muller was moved to write that eugenics had become “hopelessly perverted” into a pseudoscientific facade for “advocates of race and class prejudice, defenders of vested interests of church and state, Fascists, Hitlerites, and reactionaries generally.” By the mid-thirties, mainline eugenics had generally been recognized as a farrago of flawed science (1986, p. 164).

James Watson reiterates these sentiments, and further observes that “by midcentury the valid science of genetics, human genetics in particular, had a major public relations problem on its hands” due to its association with the “utterly reprehensible” social programs carried out in the name of eugenics (2003b, p. 33). Earlier, however, he has qualified this statement somewhat by stating that:

Eugenics these days is a dirty word, associated with racists and Nazis – a dark, best-forgotten phase of the history of genetics. It is important to appreciate, however, that in the closing years of the nineteenth and early years of the twentieth centuries, eugenics was not tainted in this way, and was seen by many as offering genuine potential for improving not just society as a whole but the lot of individuals within society as well. Eugenics was embraced with particular enthusiasm by those who today would be termed the “liberal left” (2003b, p. 22).

College, London), geneticist Ronald Fisher and, for a time, future Nobel laureate Hermann J. Muller. Leading figures in the birth control movement, including Margaret Sanger and Marie Stopes, were also proponents of eugenics (see Pickens, 1968).

The most well-known examples of which are Henry Goddard’s characterisation of the entire illegitimate line of the ‘Kallikak’ family as ‘defective degenerates’ (Watson, 2003b), and the case of *Buck v Bell* in 1927 where Justice Oliver Wendell Holmes ordered that Carrie Buck be sterilised since “three generations of imbeciles is enough” (Pickens, 1968). I note that Pickens does not himself appear to question that Carrie Buck was ‘feebleminded’.
Here, Watson appears to be attempting to ‘rescue’ eugenics from its ‘utterly reprehensible’ past, and while it may be the case that the ‘liberal left’ were well-intentioned, he still glosses over the question of who, exactly, were the individuals whose lives were to be improved (the poor, the feebleminded, the ‘native’?) and of how this was to be accomplished (by restricting their procreative rights?). Despite the passing of time, concern about the study of human genetics lingers. The spectre of eugenics, raising as it does these and other questions about possibilities for individual ‘improvement’ and the potential for negative personal and social consequences, provides the backdrop for many of the fears expressed about the Human Genome Project (see, for example, Alper et al., 2004; Duster, 2003; Galton, 2002; Kerr & Shakespeare, 2002; Kevles, 1986, 1993).

The Human Genome Project and its objectives

The 15-year Human Genome Project formally began in October 1990, when the Department of Energy (DOE) and the National Institutes of Health (NIH) presented a joint five-year plan to the US Congress. Chroniclers of the project place its real beginnings some six years earlier, at a conference held in Alta, Utah, in December 1984. It was reading a report of this conference that gave Charles DeLisi, the then new director of the Office of Health and Environmental Research (OHER) at the DOE, the idea for a project to map the human genome (Cook-Deegan, 1989; DeLisi, 1988; Kevles, 1993), and in 1985, two conferences were held: one at the University of California convened by molecular biologist Robert

---

The publisher’s note to the Kerr & Shakespeare text states that they reject “the two extreme positions that tend to pervade contemporary debates about human genetics: genetics as either fatally corrupted by, or utterly immune from, eugenic influence”. Thus debates around eugenics are seen as creating yet another dualism within the literature around genetics.
Sinsheimer, and one at Santa Fe, commissioned by DeLisi and David A. Smith, to “assess the feasibility of a Human Genome Initiative” (ORNL, n.d.-c).\(^{53}\) Funding for pilot projects was provided by the OHER in 1986 and by the NIH in 1987. The published goals of the Human Genome Project were to:

- identify all the approximately 30,000 genes in human DNA [originally estimated to be around 100,000],
- determine the sequences of the 3 billion chemical bases that make up human DNA,
- store this information in databases,
- develop faster, more efficient sequencing technologies,
- develop tools for data analysis, and
- address the ethical, legal, and social issues (ELSI) that may arise from the project (ORNL, n.d.-d).

In April 2003, the International Human Genome Sequencing Consortium announced the successful completion of the project, coinciding with the 50th anniversary of Watson & Crick’s discovery of the double helix (Watson & Crick, 1953). The genome project – the “crown jewel of 20th century biology” (Roberts, 2001, p. 1182) – was biology’s first foray into what has been called Big Science, and involved the cooperation of 20 research laboratories across six countries (Great Britain, France, China, Germany and Japan, as well as the United States). Jointly funded by the DOE and the NIH, US investment in the project grew from a modest joint annual budget of $US27.9m in 1988, to $US437m in 2003. In the

\(^{53}\) ORNL is the Oak Ridge National Laboratory.
end, budget allocations for the project over these 16 years totalled just under $US3.8 billion.\textsuperscript{54}

Much has been made of the enormous cost of the project. To put these figures into some context, expenditure on the Human Genome Project in any single year over the life of the project was, in comparison to expenditure on other projects, only a small proportion of the total yearly budget for the NIH.\textsuperscript{55} For example, of the proposed total NIH budget of $US27.3 billion in 2003, genome project funding totalled only 1.36\%, compared with 20.1\% for cancer-related research (MedicalNewsService, 2002).\textsuperscript{56} As well, the joint Human Genome Project Budget page notes that:

The Human Genome Project was sometimes reported to have cost $3 billion. However, this figure refers to the total projected funding over a 13-year period (1990 – 2003) for a wide range of scientific activities related to genomics. These include studies of human diseases, experimental organisms (such as bacteria, yeast, worms, flies, and mice); development of new technologies for biological and medical research; computational methods to analyze genomes; and ethical, legal, and social issues related to genetics. Human genome sequencing represents only a small fraction of the overall 13-year budget (ORNL, n.d.-b, para 1).\textsuperscript{57}

The sequencing of all the DNA in the 24 human chromosomes, or an estimated 3.1 billion base pairs,\textsuperscript{58} was completed two years ahead of schedule, covering 99 per cent of the genome with an error rate of less than 1 in 10,000 base pairs (NHGRI, n.d.-g). Although called the Human Genome Project, it also included the

\textsuperscript{54} The exact figure, arrived at by simple addition of each year’s budget allocation from 1988 through to 2003, is $3,798,300,000 (ORNL, n.d.-b). NHGRI puts the actual cost of the project at “about $2.7 billion in FY1991 dollars” (NHGRI, n.d.-g).

\textsuperscript{55} Of the two agencies, NIH has been lead funding agency on the project.

\textsuperscript{56} The figure quoted for cancer-related research in fiscal year 2003 is US$5.5 billion; the NIH budget for the HGP in fiscal year 2003 was US$372.8 million.

\textsuperscript{57} As noted above, pilot funding for the program began in 1988.

\textsuperscript{58} The complementary strands that make up DNA are comprised of four nucleotide bases – cytosine, guanine, adenine and thymine. C always pairs with G, and A always with T, giving the base pairs AT or CG.
sequencing of the genomes of a number of model organisms, including the
*Saccharomyces cerevisiae* (baker’s yeast) genome in 1996, *E. coli* (bacteria) in 1997,
*Caenorhabditis elegans* (nematode worm) in 1998, and *Drosophila melanogaster* (fruit
fly) in 2000. By late 2006, some 50 genomes had been or were being completed by
laboratories worldwide. The mouse and rat genomes have been sequenced, and
other model organism sequencing continues, with the rice and honey bee
genomes recently completed. The Sanger Institute is, at time of writing in early
2009, sequencing the zebrafish (*Danio rerio*) genome, as part of a ‘suite’ of
genome-related projects (Sanger Institute, n.d.). In September 2006, researchers
from the Johns Hopkins Kimmel Cancer Center (JHKCC) announced that they
had completed the first draft of the genetic codes for breast and colon cancer, and
expect the identification of the genes involved in these diseases to guide further
research (JHKCC, 2006). Also in 2006, the National Cancer Institute (NCI) and
NHGRI began a joint pilot project investigating the feasibility of the development
of a complete cancer atlas (Collins & Barker, 2007). These and many other
projects are ongoing.

**Outcomes and benefits**

Proponents of the Human Genome Project have claimed that potential benefits
across a number of areas will flow from the knowledge gained from the project
itself, and from post-HGP projects such as those above. Many of these benefits
are already evident: new techniques in forensic science are being used to help
identify crime suspects and to exonerate those who have been wrongly accused
of crime; in agriculture, disease and insect resistant crops have been engineered
and biopesticides developed, reducing the use of harmful chemical pesticides (Watson, 2003b, p. 145).

Advances in molecular medicine have been slow, and plagued by setbacks, but are beginning to gather momentum. There has, however, been progress in the diagnosis and prevention of some diseases. Most recently, for example, Australian scientists at the University of Queensland have used genetic technologies to develop a cervical cancer vaccine that is 100 per cent effective against the strain of the human papilloma virus (HPV) that causes two-thirds of all cervical cancers. The vaccine is only effective in those who are not infected with HPV, and work is continuing on production of a second vaccine that will treat existing infections (UQ News Online, 2005). The Australian Government moved relatively quickly to add this vaccine to the national inoculation schedule, and mass vaccinations of target groups of girls and young women began in 2007.

There is no clearer example of how the promise of control is fulfilled, than in the fact that a primary use of this information is in the development and application of gene therapy and genetic diagnosis for various purposes. New treatments utilising gene therapy and therapeutic cloning continue to be trialled, with predictably uneven results (see Thrasher, 2005). The discovery in 1993 of the mutation that causes Huntington disease – an unusually large number of CAG repeats in the gene located at 4p16.399 – has until recently allowed for certainty in

---

diagnosis of the disease, but provided no treatment options. Within the last few years, however, researchers have reported some hope of a potential treatment and eventual cure utilising gene therapy. A report from the University of California Urvine Medical Center, in 2002, noted that:

A protein developed in the laboratory halts the progression of Huntington’s disease in fruit flies ... and it may help researchers find effective ways to use gene therapy to prevent – or halt – the disease (UCI Medical Center, 2002, para 1).

Researchers at the University of Iowa have conducted experiments on mice using a technique called RNA interference that switches off unhealthy genes, which they are hopeful will lead to treatments in humans for Huntington and Alzheimer’s diseases (Holmes, 2003; Xia, Mao, Paulson, & Davidson, 2002; Xia et al., 2004). Similarly, experimental gene therapy in humans with cystic fibrosis has been underway since 1993, although the search for the most effective delivery system is continuing (NHGRI, n.d.-d). Pre-implantation genetic diagnosis (PGD) has been used in Britain to select embryos who do not carry the gene for either disease, and University College, London, has recently announced the birth of a baby girl ‘selected’ to be free of the breast cancer gene, BRCA1 (Quinn, 2009). In the last few years, gene therapy has been used to treat metastatic melanoma with limited success – with two of 17 patients treated remaining disease-free a year after treatment (Maisey, 2006) – and to treat an inherited disease that causes blindness (Bainbridge, Smith, Barker, Robbie, & et al, 2008; Maguire, Simonelli, Pierce, Pugh, & et al, 2008).
Perhaps the most publicly visible gene therapy, because so widely reported in the media, has been its use in patients with severe combined immune deficiency syndrome (SCID). The NHGRI reports the successful treatment with gene therapy for two young girls with one form of SCID in the United States in 1990 (NHGRI, n.d.-h), and clinical trials of gene therapy for X-linked SCID were conducted in Paris and reported in the journal *Science* in 2000 (see Cavazzana-Calvo et al., 2000). Following reports of the development of cancer in three patients between 2002 and 2005, the Food and Drug Administration (FDA) in the United States suspended gene therapy trials (Harris, 2005), as did Australia (NH&MRC, 2005). However, an April 2002 report on BBC News Health reported the success of the therapy in an 18-month-old boy with SCID (BBC News Health, 2002), and similar gene therapy trials underway at the Great Ormond Street Hospital, London, were permitted to continue, on the basis that the “study represents a significant milestone in the development of gene therapy treatments” (Medwirenews, 2002, penultimate para).

Therapeutic cloning – more correctly somatic cell nuclear transfer (SCNT) – has also been at the forefront of public discussion in Australia in the last few years, with the drafting of a Bill by Senator Kay Patterson to lift the ban on cloning of human pre-embryos for stem cell research. This Bill was based on the Lockhart Review presented to the Australian Government on 19 December 2005, which recommended that SCNT be permitted. The government’s initial reaction was to ignore this recommendation of the committee:

> After careful reflection, the Government is not disposed to make any changes to the existing national legislative framework for research involving human embryos, agreed in 2002 (Prime Minister of Australia, 2006).
However, widespread challenge to this position prompted the then Prime Minister, John Howard, to allow parliamentarians a conscience vote on the issue. Patterson’s Bill, amended to disallow the use of animal eggs to create animal-human hybrids, passed the Senate on 7 November 2006, by a close 34 to 32 votes, and then passed the lower house without amendment a month later by a convincing margin of 82 to 62. This Bill replaced that passed in 2002 which had allowed the extraction of stem cells from ‘spare’ IVF embryos, but prevented SCNT.

Stem cell research is currently being used to target a wide range of diseases and trauma-related injuries, including diabetes, Alzheimer’s and Parkinson’s diseases, and spinal cord injury. Scientists at the University of Wisconsin-Madison have announced success with converting stem cells into spinal nerve cells, which they hope will be “a stepping stone” towards a cure for motor neurone disease (Jeffery, 2005, para 8), and in February 2005, British researchers were also granted a licence to clone human embryos for research into motor neurone disease (Jeffery & Perrone, 2005). In related work, researchers at the University of California Irvine, report that “paralysed rats regained their ability to walk after having their spinal cord injuries treated with human embryonic stem cells” (Jeffery, 2005, para 8).

American researchers have successfully used embryonic stem cells to vaccinate mice against lung cancer, although they point out that this research is in its very early stages and is unlikely to be of benefit to humans for some considerable time.
(European Organisation for Research and Treatment of Cancer, 2006). Stem cell research is, however, beginning to move ahead in a number of other areas. In late 2004, French cardiovascular surgeon, Dr Phillippe Menasche, reported on a trial to use embryonic stem cells to treat heart failure patients (Menasche, 2004); two years later, British researchers harvested stem cells gathered from a heart attack victims’ own bone marrow, injecting them into the heart within a few hours of the attack, in the expectation that the stem cells would help to repair the damaged heart muscle (Towie, 2006).

In November 2008, pharmaceutical giant Pfizer announced the launch of a Regenerative Medicine Unit, co-located in the UK and the USA and employing around 70 researchers. Their press release notes that the focus of the unit will be on exploring “the use of stem cells to develop future treatments that may prevent disability, repair failing organs and treat degenerative diseases. The ultimate goal will be to deliver new medicinal products that can pave the way for the use of cells as therapeutics” (Medical News Today, 2008, para 2). At University College London, the London Project to Cure Blindness (LPCB) is developing embryonic stem cell therapy to treat patients with age-related macular degeneration (LPCB, 2007). Most recently, on 23 January 2009, the American Geron Corporation received regulatory permission to treat up to 10 patients who are paralysed from the chest down with embryonic stem cells injected into their spines (Geron Corporation, 2009; see also Stein, 2009).
Controversies

The proposal to undertake the Human Genome Project provoked controversy on many levels, at least initially as much from within scientific circles as from without.\textsuperscript{60} This was to be biology’s first foray into ‘Big Science’, and there was considerable discussion, at times heated, amongst leading scientists about whether it was worth doing at all, and of its effects on more traditional laboratory-driven research (see Cook-Deegan, 1995, in particular chapter 8). While Walter Gilbert, winner of the Nobel prize in chemistry in 1980 and co-founder of biotech company Biogen,\textsuperscript{61} asserted from the beginning that the “total human sequence is the grail of human genetics” (Lewin, 1986, p. 1598; see also Cook-Deegan, 1995, p. 88; Gilbert, 1993; Kevles, 1993, p. 19), others were not so sanguine. David Botstein\textsuperscript{62} was reported as saying that the project “endangers all of us, especially the young researchers” (Lewin, 1986, p. 1598), and, at the same time, Nobel prize winner David Baltimore commented, “The idea is gathering momentum. I shiver at the thought” (Lewin, 1986, p. 1600). A year later, leading cancer researcher Robert Weinberg said he was “surprised consenting adults have been caught in public talking about it” (Hanna, 1991, p. 154). There was also early debate about which of two major US agencies – DOE, with its strong research background in human mutation grounded in its involvement in atomic

\textsuperscript{60} See, for example, James Watson’s account of what he called the “backlash against the human genome project” (1993, p. 165).

\textsuperscript{61} Founded in 1978; see http://www.biogenidec.com/site/history.html.

\textsuperscript{62} One of the researchers who, in 1980, had suggested a method of producing a linkage map of genes through the use of restriction fragment length polymorphisms (RFLPs), a method later used in the genome project. A RFLP (rif-lip) is defined in the Talking Glossary on the NHGRI website (www.genome.gov) as “Genetic variations at the site where a restriction enzyme cuts a piece of DNA. Such variations affect the size of the resulting fragments. These sequences can be used as markers on physical maps and linkage maps”.

82
research, or NIH, the primary US government health agency – should lead the project.

While these debates were settled early, and indeed critics Botstein, Baltimore and Weinberg all became involved in genome research, controversies surrounding the implications for and impacts upon equitable and ethical societies of this knowledge are ongoing. These controversies, originally centred around the pursuit of the project itself, now focus on the very many fields of research that have either been made possible, or been advanced, by scientists’ access to the completed genomes of humans and various other species, and by the technological advances that in many ways progressed hand-in-hand with the project.

In their Introduction to The Code of Codes, a collection of essays first published in 1992 addressing the scientific and social issues likely to arise from the project, Daniel Kevles and Leroy Hood said:

The search for the biological grail has been going on since the turn of the century, but it has now entered its culminating phase with the recent creation of the human genome project, the ultimate goal of which is the acquisition of all the details of our genome. That knowledge ... will transform our capacities to predict what we may become and, ultimately, it may enable us to enhance or prevent our genetic fates, medically or otherwise (1993a, p. vii).

This anticipated capacity to transform our genetic fates is a focal point for controversy about the application of genomic knowledge. Writing in 2003, James Watson, co-discoverer of the structure of DNA, said:

The Human Genome Project is much more than a vast roll call of As, Ts, Gs, and Cs: it is as precious a body of knowledge as humankind will ever acquire, with a potential to speak to our most basic philosophical questions about human nature, for purposes of good and mischief alike (2003b, p. 173).
His early recognition of the potential for harmful consequences and of the wide range of issues likely to arise out of genome research informed Watson’s independent decision, shortly after his appointment as inaugural director of the Human Genome Project at the NIH, to set aside three per cent of genome program funding for ELSI research. This figure increased over time to five per cent of the genome budget (2003b, p. 349). In an address delivered in October 2003, Watson discussed his reasons for funding the ELSI working group:

Increasingly, both what we as humans expect from ourselves and how we deal with other human beings will be affected by genetic knowledge. Assigning genetic causation to human capabilities and disabilities almost always has ethical consequences ... Geneticists no longer should have a largely hands-off attitude to how their results bear on the functioning of human society (2003a, p. 83).

While Watson’s motives have been called into question as political by some commentators (for example Mahowald, 2000, pp 3-4; Spanier, 1995) – and the ELSI program itself judged as limited, unable to be honestly critical of genome research because it is not independently funded (Lehrman, 2000) – Watson does have a track record of support for outside scrutiny of scientific advances:

In the early 1970s, Watson was almost alone among scientists in supporting a commission on reproductive technology and new biomedical advances. The Senate had a hard time finding a scientist who did not regard such commissions as intrusions onto sacred scientific lands, but Watson spoke out in favor of public deliberation (Cook-Deegan, 1995).

The genome project was unique in providing internal funding for this kind of scrutiny (Clayton, Fisher, Mclnerney, Press, & Walters, 2000; Mahowald, 2000). Daniel Kevles comments that:

The commitment of NIH resources to the provocation of ethical debate was unprecedented, as was making bioethics an integral part of an NIH biological research program. By no means all biologists ... supported Watson’s policy, but Watson, undaunted, defended it ... Watson was not only undaunted in his
commitment to ethics but also, it would appear, shrewd. His policy undoubtedly helped defuse anxieties (1993, p. 35).  

New government-funded genetic research, in the United States at least, routinely now includes ELSI (or EELS – ethical, economic, legal & social issues – as it is now also sometimes called) as part of its brief.

**Ethical, legal & social issues**

I foreground my discussion of the implications of the use of new or existing technologies as a result of knowledge arising from molecular biological research by saying that the religious arguments deployed in opposition to them will not be dealt with here. These arguments, involving divisive and firmly oppositional views on such questions as when life begins, the sanctity of human life, and of what we should and should not do based on religious belief, are not capable of being rationally resolved and are a distraction for this thesis. In any event, the foregoing discussion makes it clear that we have already travelled too far along this road to turn back. In my view, we need now to focus our attention on the much more critical issue of how we grapple with the implications of the actions that are already being taken, and those which seem likely to be taken in the future, as a result of this knowledge.

---

Watson is undoubtedly a strange character who also has a track record for speaking his mind and for ‘rubbing people up the wrong way’. However, he can also surprise with what are, on the face of it, enlightened, liberal (if ‘shrewd’) responses to complex issues. He is prone to holding firm opinions on divisive issues where others fear to tread. For example, his strong advocacy of germline gene therapy is both positive in its recognition that it is primarily women who still bear major responsibility for child rearing and care, and negative in its rushing toward a therapy with unknown consequences (see 2003b, pp. 424-427). This is just one instance that confirms Bonnie B. Spanier’s argument that, contra Cook-Degan and Kevles, Watson puts forward “an exaggerated positivist view” of science. She further argues that he is “one of the most overtly ‘political’ scientists in the world” and that his “obfuscation of politics and politicking in science is pure propaganda” (1995, p. 131).
The ELSI research program identifies seven program priorities, or ‘grand challenges’ for genomic research including intellectual property issues; access and use of genetic information in health, and in non-health (for example, the criminal justice system or employment) settings; ethical conduct in genetic research on humans; the impact on concepts of race, ethnicity, kinship and identity; implications of the discovery of genetic contributions to behaviour and human traits; and individual, cultural and religious views on the ethical use of genomics (NHGRI, n.d.-f).

Each of these seven areas of interest has aroused concern and provoked discussion in the academic literature, government publications, and the public arena. The ELSI program was originally envisaged as a dedicated space where such concerns could be pursued, and the NHGRI claims some significant successes for ELSI right from the beginning of the project. These include the “decision to sequence the DNA of several anonymous individuals, rather than a known individual, in order to protect privacy”, and “the development of widely used genetic privacy guidelines and draft legislation” (NHGRI, n.d.-g, para 22).

Given the focus of this thesis, I will concentrate here on those areas that potentially support reductionist theories or have an impact on notions of the body; that is to say, the implications of genetic contributions to behaviour and human traits, and the related issue of impacts on concepts of identity.

---

64 Given the preoccupations of sociobiology and sex difference research, it is noteworthy that gender is not accorded the same conceptual status as race and ethnicity but, presumably, is subsumed under identity along with sexual preference.
There are a number of issues that come to mind when we turn our attention to
critical debates around the knowledge arising from the genome project. Claims of
‘genes for’ behavioural attributes, such as sexual orientation, intelligence,
anxiety-related personality traits, and susceptibilities to alcoholism and eating
disorders, raise all kinds of questions. These range from questions about the
purpose the information serves, to questions of accountability, to questions of
social policy. Would knowing that people with a homosexual orientation are
‘born gay’ change the way society views them? If so, would it be for the better? If
I have a genetic susceptibility to alcoholism and, when drunk, I become
aggressive and harm people, can I be held accountable? If pre-implantation
diagnosis reveals that my genetic make-up endows me with a ‘below average’
level of intelligence, is it worth trying to educate me? More fundamentally, should
such knowledge change the way society (and individuals within society) views
gay people, holds individuals accountable for their actions, or fulfils its
obligations to provide (to the best of its ability) for each of its citizens? And, of
course, the pursuit of such behavioural genes raises the question of eugenics: if a
‘gay gene’ were discovered, how would society respond to a parent’s wish to
abort a child for no other reason than that it had such a gene? Would society
pressure parents to abort children with an IQ quotient below a certain level?

Gene patenting has also been a concern since the beginning of the project, and
the debate in Australia has most recently revolved around the attempted
enforcement of patents over BRCA1 and BRCA2, the genes implicated in breast
and ovarian cancers, by biotechnology firm, Genetic Technologies (see Gaglioti,
The Community Affairs Committee of the Senate is currently conducting an inquiry into gene patents and is due to report its findings by the last sitting day for 2009. Among other things, the committee will consider whether the *Patents Act 1990* should be amended “so as to expressly prohibit the grant of patent monopolies over such materials” (Senate Community Affairs Committee, 2008, point (c)).

Also of concern is the current state of knowledge about identified disease genes such as, for example, those for Huntington disease, and cystic fibrosis. As previously mentioned, we now know that the cause of Huntington disease is an unusual number of CAG repeats in the gene located at 4p16.3 (NINDS, n.d.) and that cystic fibrosis is caused by mutations in the gene located at 7q31.2 (Collins, 1992). Although some treatments exist for the management and amelioration of these conditions (and Francis Collins is optimistic about the potential of gene therapy as a future treatment for cystic fibrosis), the only current ‘cures’ are pre-implantation genetic diagnosis (PGD) and abortion.

Ethical concerns have centred around the utility of this genetic knowledge, particularly for diseases such as Huntington disease, where the individual will most likely have 40 years free of disease, and cystic fibrosis where the presence,

---


66 NINDS is the National Institute of Neurological Disorders and Stroke, NIH.

67 Francis S. Collins succeeded James Watson as director of the Human Genome Project (later NHGRI) in 1993 and remained director until August 2008. A physician and geneticist by training, he led teams that identified the genes responsible for cystic fibrosis, neurofibromatosis, Huntington’s disease and Hutchinson-Gilford progeria syndrome. He is known for the approach to the identification of genes which he labelled ‘positional cloning’ (see the brief biography of Collins at http://www.genome.gov/10000779), and for his strong religious faith and ethical approach to science (see Collins, 2003).
but not the severity, of the disease can be detected (see, for example, Ciske, Haavisto, Laxova, Rock, & Farrell, 2001; Kotwicki et al., 2001).68

The related debate then centres around notions of negative eugenics inherent in testing for and aborting embryos found to have a genetic condition which will cause them to be born with some disability or disease. Here, ethical and moral questions revolve around ‘perfectibility’, and the value of human life. For example, disability rights groups argue that testing and aborting for disability or disease devalues their lives;69 others, such as James Watson, maintain that it can only be a good thing to ‘prevent’ suffering caused by disease, although Watson states that this is a decision that must ultimately rest with individual women.70

Again, the debate circles back to imponderables. For my purposes, it is more useful to examine how the funding provided through the ELSI program has advanced research dealing with issues such as these that directly impact upon notions of personal identity, the genetics of behaviour, and women’s rights.

---

68 See also Hubbard & Wald’s discussion of treatments of inherited conditions, and of gene manipulation in general, in chapter 8 (1999, pp. 108-116).
69 And some groups have lobbied for the right to make a deliberate choice to have a child that would be considered within the wider society to have a disability (see Savulescu, 2002; Spriggs, 2002).
70 This appears to be an enlightened and well-meaning response to these issues, and I choose to read it in that way. In keeping with my general view in this thesis that individual scientists involved in these enterprises are not ill-intentioned, I see no reason for suspecting Watson of any covert scheme (for example, to make things easier for men by shifting the ultimate decision in these matters onto women, effectively returning us to a situation of ‘mother-blaming’) or of a merely rhetorical gesture to women’s reproductive rights. This is not to say that it is not also a somewhat naïve view, however. When placed within the wider debates around women’s rights, and issues around the real or illusory nature of control and choice (all of which have been well rehearsed in other contexts, such as debates surrounding abortion rights and the uses of new reproductive technologies), it is clear that such decisions are very far from being so simple.
There is no doubt that ELSI funding has been significant in enabling research projects investigating a broad range of ethical and practical issues, including a large body of work aimed at enhancing the public understanding of genetic science and its impact on society generally (for example, legal and policy outcomes, and debates around informed consent, ownership and gene patenting).\footnote{Over 550 research outputs in the 11 years between 1990 and 2001, including documentaries, educational materials, refereed journal publications and books. For a complete list, see NHGRI (n.d.-e).} The ELSI funded research around questions of personal identity has focused on race and ethnicity, genetic difference, and pre-symptomatic testing for genetic disease, but comparatively few projects are specifically related to gender issues.\footnote{As I review this in late February 2009, however, I note that the US Department of Health and Human Services has reissued a program announcement for funding of research into women’s mental health and sex/gender differences research – see \url{http://grants.nih.gov/grants/guide/pa-files/PA-09-108.html}.} Mary Mahowald’s project into the gender justice effects of the HGP on women is one exception (Mahowald et al., 1996; Mahowald, 2000). However, I have been unable to locate any ELSI research on the implications for women of, for example, research on the genetics of intelligence and between intelligence and brain volume, a large and significant body of sex difference research that I discuss in the next chapter of this thesis.

Indeed, while research that purports to have found a genetic component to a range of behaviours is well represented in the academic literature (for example, Chorney et al., 1998; Hamer & Copeland, 2000; Hamer, 2004; Illies, Arvey, & Bouchard Jr, 2006; LeVay, 1994; LeVay & Hamer, 1994; Plomin, Defries, Craig, & McGuffin, 2003; Spector, 2003) and is almost continually present in the press, it does not seem to be as widely represented in the ELSI research program. This
‘absence’ is problematic. There are some exceptions, however. In an extended
discussion of behavioural genetics prepared by Erik Parens (principal
investigator of The Hastings Center project, Crafting tools for public conversation
about behavioral genetics), Parens both the promise and the dangers of this research are
made clear:

Given that those at greatest risk for being hurt are those who already hurt as a result of the current organization of our society, there is a special obligation to guard against allowing research aimed at increasing knowledge and reducing suffering from being hijacked by the desire to justify the status quo (2004, p. 59).

Parens distinguishes between the ‘species-typical’ perspective – which, as its name suggests, searches for traits or behaviours that are ‘normal’ – and the ‘individual differences’ perspective, which instead asks why we are different. Behavioural geneticists, he says, adopt the latter approach, beginning with the “fact of human variation” which can be visually represented by the distribution along a bell curve (2004, p. S7). In the discussion that follows, Parens notes that, while researchers are moving closer to being able to understand causal relationships between genes and phenotypes through the technology called microarray analysis (2004, p. S21), thus far results from behaviour genetics have been modest, and those genes identified in association studies in the future are “expected” to be those having small effects (2004, p. S22).

---

73 The Hastings Center is an independent, nonpartisan, and nonprofit bioethics research institute. Erik Parens is a senior research scholar with the Center. This report was a collaborative effort with The American Association for the Advancement of Science (AAAS), and was funded by NHGRI.

74 Parens adds, however: “One of the few exceptions to the rule is the discovery that an allele for an enzyme involved in the metabolism of alcohol may help to explain why some people are less prone to excessive alcohol consumption than others. (Too, if Alzheimer’s disease falls within the purview of behavioral genetics, then we should note that association studies have found that individuals with a copy of a particular allele [APOE4] are six times as likely to develop the common form of Alzheimer’s disease as those without it)” (2004, p. S22).
Be that as it may, there are a number of behavioural traits listed in the Online Mendelian Inheritance in Man (OMIM) database, and while many of these are trivial – for example, handedness (2p12-q22), tongue curling, and arm folding and hand clasping patterns (none of which have an identified gene locus) – some are potentially of considerable social significance. These include behaviours such as novelty seeking (11p15.5); susceptibility to tobacco addiction (9q22.1, 5p15.3, 20q13.2-q13.3, 19q13.2); to alcoholism (13q14-q21, 4q22, 4p13-p12), and neuroticism (including anxiety – mapped to 17q11.1-q12). Intelligence – the heritability of which OMIM notes is likely to be due to multiple genes – has been mapped to genes on chromosomes 2q, 4, and 6p. Finally, male homosexuality has been linked to a gene called the SMCY gene on the Y chromosome and also, of course, to Xq28, which OMIM lists as the location of the ‘disorder’ in the form of [?Homosexuality, male].

What is encouraging in Parens’ report, however, is his discussion of a major study which showed a strong correlation between a genotypic condition (low MAOA levels) and an environmental situation (abuse) in developing antisocial behaviour of some kind (2004, p. S22; see also Caspi et al., 2002), thus proving the importance of taking an interactive approach to understanding behavioural outcomes. Parens concludes by saying that:

If it would be a serious mistake for behavioral geneticists to forget that their findings can be used to justify inequalities, it would be an equally serious mistake for commentators to ignore how the individual-differences perspective of behavioral genetics research could be put to salutary purposes ... while the

---

individual-differences perspective harbors an old danger, it also harbors a new opportunity (2004, pp. S31-S32).

The ELSI Working Group is also careful to distance itself from more controversial work in this area, and felt it necessary to release a statement directly commenting on claims made by Richard Herrnstein and Charles Murray in their book *The Bell Curve* (1994). After stating their concern about this and other books with ‘similar themes’, they say:

We are only beginning to explore the intricate relationship between genes and environment and between individual genes and the rest of the human genome. If anything, the lack of predictability from genetic information has become the rule rather than the exception ... Genetic arguments cannot and should not be used to determine or inform social policy in the areas cited by Herrnstein and Murray [eliminating welfare to discourage poor women from reproducing and developing programs to encourage women from higher socioeconomic classes – and thus they argue with higher IQs – to have more children]. Since the lessons of genetics are not deterministic, they do not provide useful information on deciding whether or not to pursue various programs to enhance the capabilities of different members of society (ELSI Working Group, 1996).

As well, in their paper on the NHGRIs vision for the future of genome research, Francis Collins and colleagues note the poor design and detrimental effects of some research in behavioural genetics. They emphasise the need for “even greater responsibility than in other areas” to ensure that research into genetic contributions to human traits and behaviour is robust and that the social implications are fully considered (2003, p. 844). Notwithstanding all these caveats, there can be no doubt that behavioural genetics is a growing enterprise (although whether, as a science, it will stand the test of time is, of course, yet to be seen).77

76 Including a very clear explanation of the limitations on behavioural genetics that can be found in the HGP pages at [http://www.ornl.gov/sci/techresources/Human_Genome/elsi/behavior.shtml](http://www.ornl.gov/sci/techresources/Human_Genome/elsi/behavior.shtml).
77 See also NHGRI (2000, Part D, para 5).
Although the genetics of sex difference and sex differentiated behaviour are not an obvious focus, extra-mural researchers funded by ELSI have considered many other issues of direct relevance to women, including difficulties surrounding reproductive decision-making (for example, prenatal screening and diagnosis, amniocentesis, ‘designer’ babies and genetic engineering more generally), and the impact on women of testing for breast cancer susceptibility genes. They have also conducted research with, or of particular interest to, women belonging to specific ethnic groups including Mexican, African-American, Native American and Jewish women. As well, the ELSI program administers a specific grant for advancing novel science in women’s health research, and has directly funded research undertaken by feminist critics on such questions as the impact of genome information on society and of the genome project on women (Mahowald, 1993, 1994, 1996; Mahowald et al., 1996; Nelkin, 1993a, 1993b; Nelkin & Lindee, 1995a, 1995b).

While the ELSI program funds what appears to be a varied and productive range of research, a number of concerns – including communication difficulties between genetic/genomic and ELSI researchers, the accessibility of ELSI data to outside researchers and policymakers, and the need to ensure that the ELSI research community includes representatives from a range of communities and disciplines – resulted in the provision of additional funding to establish Centers of Excellence in ELSI Research (CEERs). For a fuller discussion of the concerns that led to the establishment of the CEERs, see Ethical, Legal and Social Implications (ELSI) Research Advisors (2005). Funding was provided by the NHGRI, DOE, and the National Institute of Child Health and Human Development (NICHD).
in August 2004, and two further centres received funding in September 2007.\textsuperscript{79}

The overall goals of the CEER program are to:

Transcend the boundaries between genetic and genomic research and ELSI research; translate ELSI research to safe, effective and just genetic and genomic policies and practices in research, health and non-medical settings; and train the next generation of ELSI researchers to ensure disciplinary and demographic diversity (Ethical Legal and Social Implications (ELSI) Research Advisors, 2005, p. 6).

CEERs are expected to “promote intensive and sustainable interactions” between researchers drawn from all areas of the academy who may have a contribution to make to ELSI issues. As well, although again there seems to be no specific mention of gender issues, the CEERs are explicitly charged with “addressing potentially controversial issues” and with encompassing a diversity of voices (including those of ‘vulnerable groups’), in their development of public policy options (NHGRI, n.d.-c, para 2).

The CEERs bode well for furthering interdisciplinary work that recognises and validates a multiplicity of approaches to ELSI questions and, since the scheme’s inception, the six full CEERs, each with a different research focus, have received grants of between US$3.8 and US$5.6 million.\textsuperscript{80} Within their individual

\textsuperscript{79} Two exploratory centres located at Harvard University and at Howard University, Washington, are also currently funded.

\textsuperscript{80} Case Western Reserve University’s Center for Genetic Research Ethics and Law, focuses on “the ethical issues in the design and conduct of human genetic research, including issues regarding the protection of human subjects in research”; The Duke Center for the Study of Public Genomics, will “gather and analyze information about the role of publication, data and materials sharing, patenting, database protection and other practices that may affect the flow of information in genomics research”; University of Washington’s Center for Genomic Health Care and the Medically Underserved, conducts “research on the ethical, legal and social factors that influence the translation of genetic information to improved human health”; Stanford University School of Medicine’s Center for Integration of Research on Genetics and Ethics, “will focus on the ethical, legal and social consequences of uncovering the genomic contributions that may contribute to behavioral and neurological conditions”; Center for Genomics and Society at University of North Carolina-Chapel Hill, “is conducting an integrated set of research, policy, and education activities focused on new or heightened ELSI issues resulting from large-scale gene discovery and disclosure.
overarching research foci, diverse research questions are being pursued. Of particular interest for women are the programs being undertaken at the University of Pennsylvania Center for the Integration of Genetic Healthcare Technology (Penn CIGHT), where researchers are investigating implications around the ways in which uncertain genetic information is communicated to society; and at Stanford, where the ELSI implications of genomic contributions to behaviour are the primary focus. For example, in 2006, the Stanford University School of Medicine Center for Integration of Research on Genetics and Ethics (CIRGE) hosted a conference entitled, ‘Interpreting complexity: the scientific and social meaning of behavioral genetics’, the object of which was to “explore the complexity of gene-environment interactions in mediating behavior, as well as the complexity of the relationship between scientific and social interpretations of behavior” (CIRGE, n.d.-c, para 3). Speakers at this conference included feminist philosopher Helen Longino, whose paper asked whether behavioural genetics challenges or reinforces stereotypes; and cultural and medical anthropologist Nancy Press, who focused in her presentation on issues of phenotypes and social construction. Press is also a co-editor of the volume resulting from the five-year ‘Crafting tools’ project mentioned earlier (Parens, Chapman, & Press, 2006). One of the research premises of this project was:

If our society is to promote the salutary uses and resist the pernicious uses of behavioral genetics, there must be open and informed public conversation about what the scientific findings do—and do not—tell us about behavior and about who we are (The Hastings Center, 2009, 3rd premise).

---

projects”; and University of Pennsylvania Center for the Integration of Genetic Healthcare Technology (Penn CIGHT), focuses “on the implications of the communication and use of potentially uncertain genetic information” (NHGRI, n.d.-b).
A more recent symposium (in May 2008) was aimed at “considerations in measuring the environment, how to tease out the interplay between genes and environments, and ethical and policy implications of gene-environment research” (CIRGE, n.d.-c, para 2).

At Penn CIGHT, researchers are currently undertaking a sociological and historical analysis of the ethical, legal and social consequences of prenatal testing for Downs syndrome and cystic fibrosis, and are researching the “long term psychological social and medical effects of testing for BRCA1/2 genes on African-American women and their families” (Penn CIGHT, n.d., para 3). At Stanford, current projects include the building of a ‘master map’ of gene/disease/environment interactions, with a view to providing new ways of thinking about how genes and environment interact in disease (CIRGE, n.d.-a), and “a multi-method approach to explore how and what scientists think about the societal and ethical considerations related to life science research” (CIRGE, n.d.-d, para 1). A documentary film on the “biology of spirituality” investigated the “biological underpinnings of faith and religious experience” (CIRGE, n.d.-b, para 4).

This range of projects illustrates the difficult political issues ELSI research raises for feminists: the first three of these projects are clearly relevant to, and

---

81 I note that the blurb for this documentary film exemplifies what has been the object of much criticism of sociobiological research in the feminist literature; that is, in asking whether “some of us [are] hardwired through our DNA to understand God better than others?”(my emphasis) it appears to make a pre-emptive assumption of fact (in this case, that God exists) upon which it then builds its research.
potentially beneficial for, women. The final project has no obvious relevance, yet a search for a ‘biology of faith’ (see Hamer, 2004), could fuel yet more of the all-too-easy, vulgar reductionist slippages that are explored in chapter five of this thesis: in this instance, from a ‘God gene’ to any number of sex-differentiated behavioural genes. In raising this concern, I am not proposing that such a project should not be undertaken, merely that this appears to be the type of project that, without due care, lends itself to the sorts of popular misinterpretations of genetic research that defend a particular kind of status quo.

In sum, ELSI research is wide-ranging and, although health and health-related issues are a central focus, there is considerable research being undertaken on the social issues surrounding genome research. Some of this research holds special interest for women. In particular, it is encouraging to see clear statements recognising past bad acts, and the possibilities for behavioural genetics to be used as support for inequitable beliefs and practices. It is, however, a cause for concern that gender is not specifically mentioned in the program priority dealing with impacts on concepts of race, ethnicity, kinship and identity, even though it can be discerned as a component within the broader range of ELSI research, and is the major focus in a small percentage of them. What this overview of ELSI reveals is that the gendered implications of genome research are still less well-considered than are the implications of this research for other areas of social concern.
Conclusion

A decade ago, Lee Silver forecast a future society in which there will essentially be two ‘species’ of humans – the Genrich-human and the Natural-human, so distinct as to be unable to interbreed (Silver, 1999, p. 282)\textsuperscript{82} – and Damien Broderick predicted that those of us now living, or our children, may well be the last mortal generation (Broderick, 1999). Just as these more extreme visions of a genetic future have lost their grip on the imagination over time, many of the more extreme claims and metaphorical descriptions that surrounded the pursuit of the human genome in the early years have been tempered by the complexities uncovered in its realisation. However, there seems little doubt that genome research does hold out a realistic promise of enriching the lives of individuals through the alleviation of suffering across a broad spectrum of disease and injury-related conditions. The potential for the elimination of hitherto intractable genetic conditions also clearly exists.

In terms of my specific concerns, it is also clear that we are looking at the prospect of finding genetic determinants of behaviour, even if for the present a degree of uncertainty protects us. This uncertainty, however, is constantly under challenge. That being so, what is there here that we can utilise in our conversation, in the world, about biological determinism?

\textsuperscript{82} Silver’s vision goes even further than the futuristic dystopia portrayed in the 1997 movie, \textit{Gattaca}, where genetically enhanced humans – the genetic elite – could still breed with In-Valids (those conceived naturally).
My walk across campus has reassured me that I am correct in my assumption that here, at least, scientists are neither ill-intentioned nor unaware of the wider implications associated with their endeavours. Moreover, even if the politics of gender are not foregrounded to the same extent in ELSI research as are certain other socially significant issues, the genome enterprise is at least not completely gender blind. There is some attention being paid both to the specific health and social concerns of women within the context of research into and practical applications of treatments arising from such knowledge.

Importantly, there is an understanding that our ‘instruction book’ is both a treasure chest and a Pandora’s box (Collins, 2003, p. 152). Genome scientists are conscious of the ethical, legal and social issues and controversies surrounding their work and have expressed an interest in conversations around them. Crucially, without denying a foundation in biology, there is a recognition in the associated ELSI literature that even where genetic ‘determinants’ of behaviour are proposed, these will inevitably act as part of a complex of genetic, social and environmental elements and thus, that our behavioural ‘instruction book’ is continually open to being rewritten. The assertive acknowledgement of the interactive, fluid and flexible nature of ‘nature’ is an important outcome of the genome project for feminist body theorists, linking both enterprises in a public repudiation of bodies as fixed and stable entities. Each of these things opens up a space for alliances to be built between scientists, and feminist and other commentators and critics of future genetic research, to enable positive

---

83 While noting in passing that the unreflective use of metaphors that imply certainty and control, while tempered, has not completely disappeared from the rhetoric around the human genome.
interventions to be made into some of the more determinist assumptions and claims evident in the subject matter discussed in the next three chapters of this thesis.
Three
The bent twig: cognitive sex differences

Can we imagine a woman, in an interesting situation, dividing her time between morning sickness and Malthus on Population ... suffering all those flutterings, palpitations, whims, and fancies, frequent in the impregnated state, and plunging absorbedly into Kant’s *Critique of Pure Reason*, Buckle’s *Civilisation*, or Colenso on the *Pentateuch*?

James McGrigor Allan (1869, p. cc)

For two millennia, “impartial experts” have given us such trenchant insights as the fact that women lack sufficient heat to boil the blood and purify the soul, that their heads are too small, their wombs too big, their hormones too debilitating, that they think with their hearts or the wrong side of the brain.

Beth B. Hess (1990, p. 81)

**Introduction**

Throughout history, the search for human nature has been accompanied by a fascination with the psychological differences between the sexes. Although research on sex differences has been ongoing, it had become more commonplace in the latter half of the 20th century to attribute whatever psychological differences were thought to be evident between the sexes to the differing environments inhabited by individuals of each sex – that is, to nurture rather than to nature. Since the early 1990s, however, there has been an increasing shift back toward explanations that rely much more heavily on notions of inherent differences in the genetic and hormonal make up of males and females. Two
developments have been influential in bringing this shift about. Firstly, as we have seen in the previous chapter, the search for the human genome, while focusing on ‘human’ nature and not on sex difference per se, holds out both the promise of genetic discovery to buttress existing beliefs about psychological sex differences, and provides the technological means to extend such research. Secondly, in the early 1990s, a new iteration of sociobiology arose which focused particularly on psychological sex difference, and provided comfort for a proliferation of vulgar reductionist theories about women and men in popular culture.

Contemporary sex difference research aims to identify and quantify differences between women and men in intellectual functions, abilities and behaviours. It begins, as such inquiry always has, from an observation that differences exist, proceeds to identify and describe these in terms of biological function and then, in some cases, extends further into associating difference with social role. In this chapter, I first briefly survey the history of theories about human nature and the nature of ‘woman’, the sexual dimorphism of the brain, and the impact of these on beliefs about the intellectual capabilities of women. Although many of the theories I discuss were criticised in their time, I note this only in passing, since my focus is on the persistence of such theories, rather than on the objections to them. I then turn to contemporary research on sex differences, in the brain and elsewhere, and how this has material effects on our everyday lives. Finally, I consider whether, how far, and in what ways, current research and attitudes have moved away from the earlier versions.
From antiquity

Plato, together with his teacher, Socrates, and his pupil, Aristotle,\(^{84}\) may well have been “one of the shapers of the whole intellectual tradition of the West” (Plato, 1979, Frontispiece), but that tradition has not been kind to women. Indeed, it is only in recent times that it has, in only some places and in still limited ways, begun to approach the remarkable vision of Plato’s Athenian –

Almost every state, under present conditions, is only half a state, and develops only half its potentialities … so far as possible, in education and everything else, the female sex should be on the same footing as the male (Plato, 1970, p. 294)

– or of Socrates, debating the merits of differentiating between female and male Guardians in occupation and education in Book 5 of The Republic:

If the only difference apparent between them is that the female bears and the male begets, we shall not admit that this is a difference relevant for our purpose (Plato, 1979, p. 233).

This is because these equalitarian sentiments, momentarily heartening as they are when one stumbles upon them, are in no way representative either of Plato’s views in general, or of the Platonic and Aristotelian intellectual traditions that have helped to shape modern Western societies (see, for example, Spelman, 1999; Tuana, 1993). Rather, it is precisely this difference between bearing and ‘begetting’ that has historically been considered of most relevance to questions of the relative ‘fitness for purpose’ of the sexes. Specifically, it is the reproductive capacities of women that have been constructed as the essential feature of ‘woman’, as the key source of difference in cognitive process, ability and occupation between the sexes, and as justification for social and political inequality.

---

\(^{84}\) Socrates c. 469BC-399BC; Plato 427BC-347BC; Aristotle 384BC-322BC.
In the creation myth *Timaeus*, Plato proposes that woman was created only after the ‘fall’ and rebirth (as women) of those men who had failed to conquer their passions:

Man is the primary creation, the true form of humanity. Woman is a degeneration of the original state of being … Plato implied that mind or reason is more perfect than sensation or emotion … they must be “conquered” by the intellect of man or he will lose perfection … women, since they were created from “those who were cowards or led unrighteous lives” in their first birth, will be less capable of such control (Tuana, 1993, p. 7).

This positioning of ‘man’ as primarily associated with mind, and ‘woman’ as primarily associated with body, is discouraging, although very familiar. It has been reflected, refined and recycled by others throughout recorded history, especially by and among men as a basis for having common interests above and beyond their individual differences.

Nancy Tuana notes that Plato’s seemingly liberal approach to the sexes in *The Republic* is further nullified through its location within his primary focus on class (1993, p. 54). While women situated further up the class hierarchy are considered to be ‘better’ than men at the lower levels, they are physically and intellectually weaker and therefore ‘less’ than the men within their own class. Thus, for example, while women in the Guardian class might share in all the activities of the male Guardians, they, along with their children, are to be held in common by the men who will win, as a reward for service “in war and in other activities … more frequent opportunities to sleep with women” (Plato, 1979, p. 241). Plato’s vision is, then, an early example of a sexual hierarchy (within which the male is more perfect than the female *ab initio*), further complicated by eugenic overtones in which the characteristics of social class are expressed in the body’s biology.
Aristotle accepted and extended this hierarchy of the sexes, providing a biological (as distinct from a religious or spiritual) explanation for man as the ‘proper form’ of human with his theory of heat as the source of perfectibility. The less heat generated, the weaker and less perfect the animal. Aristotle reasoned that men generate more heat than women, evidenced by their ability to transform the male equivalent of menstrual fluid from the red colour of blood to the white of semen. Since a female embryo was formed through a lack of the requisite heat to “bring it into its proper form”, females were a ‘monstrosity’, a “departure from type” (Tuana, 1993, p. 19; see also Battersby, 1999, p. 348). This weaker and colder nature, however, was not without purpose since Aristotle further reasoned that it is necessary for reproduction, providing woman with the residual matter (which man has used up in perfecting his form) needed to nourish a foetus (Tuana, 1993, p. 21). If, then, as Charlotte Witt argues, what “a thing is, for Aristotle, is determined by what it can do”, then clearly what woman is, is intricately tied to her role in reproduction (Witt, 1998, p. 127).  

Nancy Tuana details how the Aristotelian system of heat-dependent sexually differentiated biology has continued to influence theories of woman’s nature across diverse disciplines up until the 20th century: in medicine, through the Greek physician Galen, who proposed that a woman’s internal genitals were further proof of her lesser perfection (since she lacked the requisite heat to cause them to protrude); in Christianity through the writings of Thomas Aquinas in the

---

85 Although Witt is here speaking about Aristotle’s notion of the ‘becoming human’ of infants as they begin to develop their physical and intellectual abilities.
12th century, who argued that God had intentionally made woman less perfect than man, in order that she fulfil her generative function; in biology in the work of 16th century biologist Ambroise Paré and 19th century biologists Patrick Geddes and John Arthur Thomson, and in the work of psychologist Sigmund Freud in the early 20th century (1993, pp. 21-25; see also Russett, 1989, pp. 89-92). As well, Plato’s dualistic constructions of man and woman, mind (soul) and body, reason and passion, echo down the centuries, entwined around and within Aristotle’s biology and carried forward through the writings of historians, theologians, alchemists, scientists and philosophers (see Tuana, 1993, in particular chapters 2, 3 & 8).

This, then, in broad terms, is the intellectual legacy inherited by 19th century scientists and scholars engaged in addressing questions of sex differences within human nature: that woman is, by nature, weaker/colder, less perfect than man, by a design that ties her purpose to reproduction and thus, to the body. In Mary O’Brien’s words, women were positioned as “the handmaidens of biological continuity” (1981, p. 19).

The Victorians

The 19th century was an especially fertile period for theories about difference. Differences between men and women, and between those of different races, are entwined in much of the Victorian literature as are taken-for-granted, and therefore often unexpressed, assumptions about class difference. I will not be considering here those themes of racial and class difference evident in the works
under discussion, but will rather confine myself to picking out only those aspects of the theories that are relevant to perceptions of sex difference.86

Herbert Spencer clearly expresses the widespread belief about these differences:

That men and women are mentally alike, is as untrue as that they are alike bodily. Just as certainly as they have physical differences which are related to the respective parts they play in the maintenance of the race, so certainly have they psychical differences, similarly related to their respective shares in the rearing and protection of offspring (1985, first published 1873, p. 17).87

Spencer’s was just one of many voices raised in this debate throughout the 19th century, on both sides of the Atlantic. Cynthia Eagle Russett attributes the renewed ‘liveliness’ of interest in the subject to the emergence of a more empirical ‘sexual science’ that was fuelled by developments across a number of fields of inquiry, including physical anthropology, psychology, sociology and, in particular, to the rise of the new field of biology, based upon evolutionary theory (1989, pp. 3-5).

The science of phrenology, for example, begun in the early 1820s by Franz Joseph Gall, proposed that one’s character is revealed by the shape of the skull. Phrenology confirmed many of the conventional distinctions between the sexes – that the superior faculty in men was intellect, in women, emotion; that men were more combative, women more “timid and careful”; that men have more interest in sexual love and women in maternal love, and that men have greater self-esteem while women are more vain. More positively, Russett notes that

86 For a comprehensive discussion of the racial science of the 19th century, see Stephen Jay Gould’s The mismeasure of man (1996).
87 Herbert Spencer (1820-1903), philosopher, and coiner of the phrase ‘survival of the fittest’ which is often incorrectly ascribed to Charles Darwin.
phrenology was reformist in nature, recognising that human nature was both “malleable” and “improvable”, and supporting women’s professional and personal aspirations in areas traditionally reserved for men (1989, pp. 16-22). Interestingly, unlike the tenor of much vulgar reductionist theory today, phrenology “did not insist on the cultivation of separate spheres” but, rather, “accommodated, even encouraged, distinctly unconventional aspirations for women” (1989, p. 22). Further, in asserting that the brain is the instrument upon which the mind depends, it complicated the independence of mind from body, challenging both contemporary religious belief and Cartesian notions of a mind/body split.

Since the conformation of the skull could not be empirically linked to observations of temperament and intellectual ability, however, phrenology’s ‘science’ did not stand up to scrutiny and was dismissed by the wider scientific community (1989, pp. 22-24). Yet scientists did not abandon pursuit of a means of establishing relative intelligence; and these other means of inquiry into differences of race and sex stepped back not only from the scientific underpinnings of phrenology, but also from it’s somewhat more enlightened views on the malleability of nature. Subsequent scientific endeavours put woman firmly back in her place, bearing children and caring for the household.

The first of these, the study of craniology, used skull size, facial angle, and the size and weight of the brain to determine the relative intelligence of the different races and sexes. It will be immediately obvious, as indeed it was to many people at the time, that the use of brain size and weight as a measure of intelligence has
serious flaws, primarily, of course, the ‘elephant’ problem. Nevertheless, the notion of greater brain size as an indicator of superior intelligence flourished in the later 19th century, often via breathtakingly tortuous logic when the facts were inconsiderately contrary. The most well-known of the craniologists, Paul Broca, and others of his school, concluded from the data they collected that male brains always outweighed women’s brains. Stephen Jay Gould points out that Broca, despite recognising that part of this difference in brain weight between men and women was a consequence of average differences in body size, “made no attempt to measure the effect of size alone, and actually stated that he didn’t need to do so”:

We might ask if the small size of the female brain depends exclusively upon the small size of her body … But we must not forget that women are, on the average, a little less intelligent than men … We are therefore permitted to suppose that the relatively small size of the female brain depends in part upon her physical inferiority and in part upon her intellectual inferiority (quoted in Gould, 1996, p. 136).

Others drew similarly ill-founded conclusions. Paul Topinard asserted that men needed more brain than women because they were more active (quoted in Gould, 1996, p. 136), and Gustave Le Bon – whom Gould labels the “chief misogynist of Broca’s school” – likened women’s brains to those of gorillas, stating that women “represent the most inferior forms of human evolution” (quoted in Gould, 1996, pp. 136-137).

---

88 Russett quotes suffragette Helen Hamilton Gardener (1853-1925) as commenting that “Almost any elephant is several Cuviers in disguise, or perhaps an entire medical faculty” (1989, p. 36). Naturalist Georges Cuvier (1769-1832) had a brain that weighed 1,830 grams – well above the European male average of 1300 to 1400 grams. Paul Broca (1824-1880) himself had a brain that weighed only slightly over the average at 1424 grams (Gould, 1996, p. 124). Anne Fausto-Sterling discusses both the ‘elephant problem’ and the ‘bird problem’ – that is, Cuvier’s notion of estimating the relative sizes of cranial and facial bones to gauge intellect – in Myths of gender (1985, p. 37).

The calculated brain weight differentials between men and women – ranging from 181 grams or over 6 ounces, to 113 grams or just under 4 ounces – prompted psychologist George J. Romanes to state:

Seeing that the average brain-weight of women is about five ounces less than that of men, on merely anatomical grounds we should be prepared to expect a marked inferiority of intellectual power in the former (reprinted in Spender, 2001, p. 11). 10

Belief in woman’s intellectual inferiority based on these ‘missing five ounces’ remained a feature of the literature until it came under scrutiny in the early 20th century, in research undertaken by Alice Lee of Karl Pearson’s Biometrics Laboratory and by Pearson himself. Their criticisms/conclusions were also supported by the research of American anatomist Franklin P. Mall, and by anthropologist Franz Boas (Russett, 1989, pp. 164-166; Gould, 1996, pp. 112, 140). 11 From the studies conducted in his laboratory, Pearson concluded that:

For practical purposes it seems impossible … to pass any judgment from size of head to ability or vice versa (quoted in Russett, 1989, p. 165),

while Mall, commenting more particularly on the relevance of the research to male and female brains, said:

---

10 Russett notes that a German scientist reported figures that showed a smaller (140 grams, or 5 ounces) average difference between men and women (1989, p. 35) than that reported by Gould on Broca’s figures (181 grams, or 6.4 ounces). I also note that Russett and Gould differ considerably on Broca’s figures for females. Gould gives 1325 grams for males and only 1144 grams for females, while Russett gives 1323 grams for males but 1210 grams for females, a smaller average difference of 113 grams, or four ounces.

11 Karl Pearson (1857-1936), protégé of Francis Galton, socialist and feminist sympathiser, was a statistician and population biologist, Director of the Galton Laboratory for National Eugenics and first Galton Professor of Eugenics (later Genetics) at University College, London; co-founder of the journal, Biometrika, and founder of Annals of Eugenics (later Annals of Human Genetics) (Pickens, 1968; Kevles, 1986, 1993). Alice Lee (1859-1939), earned her doctorate in the Biometrics Laboratory at the University of London, and was co-author with Pearson on a number of papers (see Love, 1979; Porter, 2004; Sweeney, 2001). Franklin Paine Mall (1862-1917), was Professor of Anatomy at Johns Hopkins, with research interests in embryology (see Magoun, 2003, pp. 86-87; Sabin, 1934). Franz Boas (1858-1942), sometimes called the father of modern American anthropology, was known for applying the scientific method to anthropological research, for his views on race as an invalid concept, and for theories of cultural relativism (see Boas, 1929, 1965; Williams Jnr, 1996).
Until anatomists can point out specific differences which can be weighed or measured, or until they can assort a mixed collection of brains, their assertions regarding male and female types are of no scientific value (quoted in Russett, 1989, p. 166).

At the same time as Broca and others were proposing their various craniometric measures to establish the superior intelligence of (educated, white, European) males, others were emphasising women’s more evident involvement in the procreative process as the main reason for why they could not equal men in intellectual matters.

Debates around women participating more fully in education, and in the public sphere more generally, mobilised arguments based on both the biological events in women’s lives connected to reproductive process, and on the social constraints placed upon women as a consequence of their potential to bear children. Thus, menstruation was constructed as a recurring weakness, periodically rendering women incapable of sustained or intense intellectual labour or, indeed, of logical thought:

Although the duration of the menstrual period differs greatly according to race, temperament, and health, it will be within the mark to state that women are unwell, from this cause, on the average two days in the month, or say one month in the year. At such times, women are unfit for any great mental or physical labour. They suffer under a languor and depression which disqualify them for thought or action ... imagine a woman, at such a time, having it in her power to sign the death-warrant of a rival or a faithless lover! (Allan, 1869, pp. cxcviii-ccxix).

American, Edward H. Clarke, author of the widely-read and much quoted Sex in Education, or A Fair Chance for Girls, believed that higher education placed women
at risk of sterility, atrophy of the uterus and ovaries, and masculinisation.\textsuperscript{92} He proposed that girls should study less than four hours a day, and not at all whilst menstruating:

\begin{quote}
The system is then [during menstruation] peculiarly susceptible; and disturbances of the delicate mechanism we are considering, induced during the catamenial weeks of that critical age by constrained positions, muscular effort, brain work, and all forms of mental and physical excitement, germinate a host of ills. Sometimes these causes, which pervade more or less the methods of instruction in our public and private schools, which our social customs ignore, and to which operatives of all sorts pay little heed, produce an excessive performance of the catamenial function; and this is equivalent to a periodical hemorrhage. Sometimes they produce an insufficient performance of it; and this, by closing an avenue of elimination, poisons the blood, and depraves the organization. The host of ills thus induced are known to physicians and to the sufferers as amenorrhoea, menorrhagia, dysmenorrhoea, hysteria, anemia, chorea, and the like. Some of these fasten themselves on their victim for a lifetime, and some are shaken off. Now and then they lead to an abortion of the function, and consequent sterility (2006, first published 1875, pp. 11-12).\textsuperscript{93}
\end{quote}

In the United Kingdom, Henry Maudsley echoed these sentiments, claiming that, in deciding whether females should be educated in ways that had been designed for males, it was “needful to consider whether this can be done without serious injury to her health and strength” (1985, first published 1874, p. 77).\textsuperscript{94} He pointed out that the ‘real educational strain’ would coincide with puberty, which in itself places great demands on the not-inexhaustible resources of the body. Women’s bodies and minds, unlike those of men, “for one quarter of each month during the best years of life [are] more or less sick and unfit for hard work” (1985, first published 1874, p. 86). Such education as was provided to women must, then, be adapted to their,

\textsuperscript{92} Edward H. Clarke (1820-1877), was a physician and medical writer in private practice in Boston, and Professor of Materia Medica at Harvard Medical School until 1872.
\textsuperscript{93} ‘Catamenia’ = the menstrual discharge.
\textsuperscript{94} Henry Maudsley (1835-1918), had a distinguished career as a physician, and was professor of medical jurisprudence at University College from 1869-1879 (see Anonymous, 1918).
… foreordained work as mothers and nurses of children … For, it would be an ill thing, if it should so happen that we got the advantages of a quantity of female intellectual work at the price of a puny, enfeebled, and sickly race (1985, first published 1874, p. 81).

While many women, and some few men, raised their voices in opposition to these views, society’s need for women to bear children, and to continue in their social roles confined to the private sphere as homemakers and carers, is similarly tied to woman’s biological ‘nature’ in other late 19th and early 20th century discourses.

Resolving the biological distinction in this way enabled the active promotion of social distinctions. Edward Drinker Cope’s catalogue of traits ‘proved’ that woman was the more infantile of the sexes and, by extension, that “Woman is no longer a misbegotten man. She has become an unevolved man” (Tuana, 1993, p. 44). Cope argued against women’s suffrage on the basis that men have a greater capacity for rational processes and for mechanical skill. While women have a greater aesthetic sense, he asserted that they have less endurance for mental strain, and therefore more easily “breakdown under stress” (1985, first published 1888, p. 211). The effect of a maternal instinct “in those departments where affection should not enter” – that is, rationality, logic and justice – rendered woman man’s inferior (1985, first published 1888, pp. 211-212).

---

95 See, for example, the rebuttals reprinted in Newman, 1985. For a contemporary discussion of the politics of menstruation see Emily Martin’s *The woman in the body* (2001).

96 Edward Drinker Cope (1840-1897), American paleontologist, in later years curator of the National Museum in Washington, D.C. (1884), Professor of Zoology and Comparative Anatomy at the University of Pennsylvania (1895-1897), and President of the American Association for the Advancement of Science (1896) (see Davidson, 1997). For Cope’s list of traits see Tuana (1993), p. 43.
Canadian-born author Grant Allen, while claiming to be an enthusiast on the ‘Woman Question’, nevertheless also believed that since most women must be mothers, they “should therefore be trained, physically, morally, socially, and mentally, in the way best fitting them to be wives and mothers” (1985, first published 1889, p. 127). Allen held that women who were educated like men “became unsexed in the process, and many others acquired a distaste, an unnatural distaste, for the functions which Nature intended them to perform” (1985, first published 1889, p. 128). In particular, the education of women of the ‘cultivated classes’ had rendered them unfit for their roles as wives and mothers since many refused marriage and, following Maudsley, Allen declared that others who did marry produced only ‘enfeebled’ descendants. In his view, those who refused marriage or, for some other reason remained a “self-supporting spinster” were an abnormality and a “deplorable accident”, and to educate women for occupations other than motherhood was to “sacrifice the race to a handful of barren experimenters” (1985, first published 1889, p. 130).

In a similar vein, Canadian gynaecologist A. Lapthorn Smith, argued that the “duties of motherhood are direct rivals of brain work” and that educated women would be unable to please or be pleased by men. Writing in 1905, Lapthorn Smith asserted that women were then in worse health than in the past, and blames this on their over-education,

---

... which first deprives them of sunlight and fresh air ... second, takes every drop of blood away to the brain from the growing organs of generation; third, develops their nervous system at the expense of all their other systems ... fourth, leads them to live an abnormal single life ... fifth, raises their requirements so high that they can not marry a young man in good health (1985, first published 1905, p. 149).

In our reading of these passages today, most of us, I suggest, would react with amusement (and some horror) that such assumptions could be made, that such views could be pronounced so seriously, with such finality, and hold such popular general appeal. Yet, these were deeply held beliefs, which had a very real impact on the ways in which women and men were organised, policed, and lived their lives, and they were beliefs with the whole weight of scientific authority behind them. Are we doing any better in the 21st century? Or is our reliance upon, and willingness to defer to the authority of scientific method, as oftentimes similarly deep and unexamined?

In the twentieth century sex and scientific inquiry continue their uneasy relationship in new areas with new vocabularies – hormonal research, brain lateralization, and sociobiology among them. The level of discourse is in every way more sophisticated, and the content less overtly tendentious ... Still, the historical emphasis on difference continues to be put to the use of an ideology of incapacity, though the two concepts are not logically linked (Russett, 1989, p. 14).

Modern times

In January 2005, the then Harvard University President, Lawrence H. Summers, was at the centre of controversy over remarks he made to a conference on diversifying the science and engineering workforce. In his unscripted comments, Summers proposed three “broad hypotheses” as explanations for the

---

98 A. Lapthorn Smith (1855-?) was Professor of Clinical Gynaecology at Bishop’s University, Quebec, Canada.
shortage of women in “high-end scientific professions”: the “high-powered job hypothesis”, the “different availability of aptitude at the high end”, and “different socialization and patterns of discrimination in a search” (2005b, para 2).

Summers’ discourse on variance in aptitude between women and men, resulting in an estimate of five to one in men’s favour ‘at the high end’, provoked outrage amongst women in the conference audience, and widespread media coverage both in the United States and abroad (Angier & Chang, 2005; BBC News, 2005; Dobbs, 2005; Jaschik, 2005; Kleinfeld, 2005; Lawson, 2005; Taylor Jnr, 2005). Based on available newspaper reports, indignation appears to have been focused primarily around this question of sex differences in aptitude. However, Summers in fact wove together all three hypotheses using two additional strands of difference, both of which are also problematic.

Summers first frames his discussion of the high-powered job hypothesis around (particularly married) women not wanting to undertake a job that requires an 80-hour-a-week commitment – effectively, women do not want high-powered intense work, and those few who do are predominantly unmarried and childless. Why most women do not want this work is then inferred – they prefer, and thus make a choice rather to be married and to have children; although the reason
why they make this ‘choice’ – and why it is not expected of married men – is not explored.\footnote{Although Summers does, later in his speech, give some recognition to the difficulties of obtaining child care, I note that this in itself features as a problem for women but not for men.}

What is then explored in some detail is the “reasonably strong evidence of taste differences between little girls and little boys”. The evidence is provided by anecdotes of experiences in kibbutzim in Israel, the toy preferences of his two-and-a-half year-old twin daughters, and his assertion of the failure of the ‘socialization hypothesis’ to account for these (Summers, 2005b, para 5).\footnote{For a discussion of the differing perspectives on sex differentiation of roles in kibbutzim, see Carol Tavris and Carole Offir (1977, pp. 285-291).}

Summers concludes that:

So my best guess, to provoke you, of what’s behind all of this is that the largest phenomenon, by far, is the general clash between people’s legitimate family desires and employers’ current desire for high power and high intensity, that in the special case of science and engineering, there are issues of intrinsic aptitude, and particularly of the variability of aptitude, and that those considerations are reinforced by what are in fact lesser factors involving socialization and continuing discrimination (2005b, para 6).

That someone of Summers’ stature would make such claims and draw such conclusions, albeit in a relatively informal and unconsidered way, is testament to the current resurgence and renewed strength of Russett’s ‘ideology of incapacity’, even within erstwhile educated circles.\footnote{In a letter to the Harvard community dated 19 January 2005, Summers apologised, saying “I deeply regret the impact of my comments and apologize for not having weighed them more carefully” (Summers, 2005a, para 1). Summers resigned from his post as president effective June 2006, after a five-year tenure that encompassed other controversies, including the decrease in numbers of women appointed to tenured positions at Harvard during his time as president (Goldenberg, 2005).} Within the context of the whole argument, and despite the nod to ‘people’s legitimate family desires’, there is a deafening silence around the social constraints that may be imposed upon men by their ‘family desires’; it is simply assumed they have none. Again,
women’s cognitive and reproductive capacities are linked, and then conflated with their social roles as wives and mothers. Yet again, women’s (but not men’s) aptitudes, ‘choices’ and tastes all converge to explain and excuse the status quo through a particularly gendered recourse to nature.

Carol Bacchi revealed for us this predicament: that women are constantly being put into the position of casting ourselves as the problem to be ‘fixed’. The link between desire, choice, and biology is not explicit for men, as it is for women. Summers’ comments illustrate this point very nicely. He is no more able to contemplate male biology in terms of constraint than the previous history of male scholarship suggests. Male biology is advantaged within the context of social choice, while women’s choices are configured as constrained by a disadvantaged biology. The recourse to nature applies only to women – there is no such recourse to nature in men’s biology.

As we have seen, in the past scientific explanations of sex differences have tended to conform to the existing social structures and popular views on the place of women and men in society. So we must ask of the contemporary scientific research on sex differences, how far, and in what ways, does it help to shift us from the received wisdom of the past and its trenchant preoccupation with linking ‘women’s nature’ to ‘women’s place in society’, and its equally trenchant complacency about men’s nature?
Research into sex differences in modern times has conventionally fallen within two broad categories of investigation: cognitive ability, and preferences and behaviour. Sex difference in cognitive ability is still most often studied within the three general cognitive domains of verbal, visuospatial and quantitative abilities (Maccoby & Jacklin, 1974), relying on various test instruments administered to a general population and also to populations with specific brain injuries. Research into human sex differences in preference and behavior often draws its conclusions from studies of individuals with congenital hormonal abnormalities (for example, girls with congenital adrenal hyperplasia (CAH)); with naturally occurring variations in prenatal hormone levels (such as in females with a male twin; see Miller, 1998), as well as from studies of animal models (thus making assumptions of comparability with humans).

Throughout the 1990s and into the first decade of the 21st century, sex difference research generally has not only continued but has been extended and reinvigorated by the genome and post-genome research juggernaut. With the completion of the Human Genome Project and the advent of powerful new genetic technologies, the field now includes research on sex dimorphic gene expression (Becker et al., 2008); non-reproductive sex differences consequent upon the chromosomal differences in males and females (that is, that males are XY, females XX), and an emergent body of research on the phenotypic effects of

---

103 CAH girls are relied upon widely in the research literature to establish hormonal influences on sex differentiated behaviour in humans; however, the Committee on Understanding the Biology of Sex and Gender differences conclude that: “For all behaviors studied, the differences between females with CAH and unaffected females are less than the differences between typical males and typical females” (Wizemann & Pardue, 2001, p. 93), with the implication that the influence of sex-related socialisation must be considered.
sex difference generally at the molecular level (see Wizemann & Pardue, 2001, pp. 28-78).

As well as providing the means for newer areas of inquiry, new genetic knowledge and techniques are being utilised to extend and deepen understanding within some of the more familiar research approaches. A brief excursion into this literature demonstrates the extensive range of characteristics, abilities and behaviours that are reported to differ naturally between the sexes: there are accounts of differentiation as a result of brain structure and sex hormones (Hampson & Kimura, 1992; Kimura, 1992, 2000; McEwen, 1999; McEwen, Alves, Bulloch, & Weiland, 1998); relative volume of certain brain regions (Goldstein, Kennedy, & Caviness Jr, 1999; Reiss, Abrams, Singer, Ross, & Denckla, 1996), and hemispheric asymmetry, lateralisation and differential hemispheric reliance for language tasks (Harshman, 1985; Hiscock, Inch, Hawryluk, Lyon, & Perachio, 1999; Jancke, Schlaug, Huang, & Steinmetz, 1994; Kansaku & Kitazawa, 2001; Kimura, 2000; Shaywitz et al., 1995).

The consequences of these somatic differences are reported to include behavioural sex differences in: toy choice (Alexander, 2003; Alexander & Hines, 1994, 2002; Berenbaum & Hines, 1992; Campbell, Shirley, Heywood, & Crook, 2000) and playmate choice (Benenson, Morganstein, & Roy, 1998; Hines & Kaufman, 1994; Maccoby, 1980, 1999); play (Hines & Kaufman, 1994; Maccoby, 1988, 1999; O’Brien & Huston, 1985; Servin, 1999) and exercise behaviours (Jonasen, 2007; McDonald & Thompson, 1992); aggression (Maccoby, 1999; Maccoby & Jacklin, 1974); pain perception (Berkley & Holdcroft, 1999; Berkley,

It is not my purpose here to evaluate these studies. The scientific literature on ‘natural’ physical and behavioural sex differences has been effectively and exhaustively assessed and critiqued elsewhere.\(^{104}\) It is fair to say, however, that the preponderance of conclusions drawn across the entire spectrum of this research into sex-based behavioural differences support a male dominant/female subordinate status quo.\(^{105}\) That is to say, such research – which finds that from infancy (prior to any possible socialisation), females show more interest in people, and males in objects, for example – reinforces the notion that females are hardwired to be more emotionally engaged, while males are more mechanically or intellectually inclined and more emotionally distanced. Moving from infancy into childhood, sex-specific toy choice – dolls for girls, trucks or guns for boys – has obvious implications for claims about the biological imperatives behind traditional family and social roles. Research on same-sex play partners, sex-differentiated play styles and group dynamics – that boys are rougher and less democratic in play, that girls are more sedentary, quieter, more egalitarian in leadership styles – has similar implications for expectations that are placed upon us all as we mature (for example, the expectation that women will be less assertive and aggressive than men is one obvious outcome). Studies on pain sensitivity may work to reinforce perceptions of women as ‘the weaker sex’.

Finally, of course, findings that occupations are chosen because of sex-specific,


\(^{105}\) Of course, some research reports findings contrary to the majority.
biologically-based strengths and weaknesses has in the past resulted in the ghettoisation of so-called ‘women’s work’, with poor pay and lower status.

Questions of biologically-based motivations driving occupational choice bring us back to President Summers’ comments, and to the extensive body of research into sex-differentiated cognitive function and problem solving abilities. Since this journey through contemporary sex difference research began with claims of differential aptitude in science and engineering (particularly in mathematical and spatial ability, and therefore with an inferred difference in comparative intelligence between men and women), this set of studies will repay somewhat closer attention.

In numerous studies, tests show that women do better than men in problem solving tasks involving perceptual speed, object location, ideational and verbal fluency, precision manual tasks, and on mathematical calculation tasks. Men, on the other hand, do better than women on spatial tasks such as mental rotation, on target-directed motor skills, disembedding tasks, navigational skills, and on tests of mathematical reasoning. In a well-known and often-quoted article published in *Scientific American* in 1992 (reprinted in 2002), Doreen Kimura suggests that this is because “men’s and women’s brains are organized along different lines from very early in life” (1992, p. 87). She attributes these mixed patterns of ability to differing prenatal hormonal environments:

---

106 Bibliographies of research on cognitive and problem solving ability in particular skills can be found in the ‘Further reading’ sections of each chapter of Kimura’s text; but see also Anne Fausto-Sterling (2000c), Steven Gangestad & Jeffry Simpson (2007a), Melissa Hines (2004), Myra Hird (2004), and Bobbi Low (2000).
Since, with the exception of the sex chromosomes, men and women share genetic material, how do such differences come about? Differing patterns of ability between men and women most probably reflect different hormonal influences on their developing brains. Early in life the action of estrogens and androgens … establishes sexual differentiation (1992, p. 82).

As well, Kimura reports that research undertaken by herself and colleagues has found that cognitive ability in both sexes is affected by fluctuations in hormone levels postnatally – across the menstrual cycle in women, and seasonally and at different times of the day in men (1992, p. 87). One such study found that men perform better on tests of spatial skill in the (northern hemisphere) spring, than they do in the autumn when testosterone levels are higher, while there was no seasonal fluctuation in women. Other studies have found that fluctuations in women’s hormone levels over the menstrual cycle had broader consequences, affecting verbal fluency, articulatory-motor and spatial skills. A further study found that men’s performance on tests of spatial skill improve as their testosterone levels decrease over the course of the day (2000, pp. 119-120).

In *Sex and cognition* (2000), Kimura notes that the presentation to a scientific meeting of the findings of changes in cognition associated with the menstrual cycle created both great media interest and a “storm of controversy” (2000, p. 118). She explains that this was reportedly because some women commentators were concerned at the possibility that data from this research could be used to

---

107 The comment that ‘with the exception of the sex chromosomes, men and women share genetic material’ is moderated in Wizemann & Pardue (2001, p. 4), who state that “there are multiple, ubiquitous differences in the basic cellular biochemistries of males and females … Many of these … are a direct result of the genetic differences between the two sexes.”

108 Kimura indicates only that this scientific meeting took place ‘a few years ago’, which would place it somewhere in the mid to late 1990s. The research upon which the discussion relies is that of Hampson (1990a, 1990b), Hampson & Kimura (1988) and Saucier and Kimura (1998).
reinforce stereotypical views of women as “creatures of their moods” (2000, p. 119), while others suspected that the studies masked a hidden agenda:

Unfortunately, as a result, most people missed the real significance of the research: that a substantial part of the cognitive difference between any two individuals may well be related to differing hormonal environments, either early in life or at the time of testing (2000, p. 119).

The subtext of Kimura’s comment, of course, is that it was the reaction of women commentators to the research findings, and the media coverage of those findings, that created controversy, which then overshadowed the ‘real significance of the research’. However, Kimura’s slightly exasperated tone indicates that she, herself, perhaps has missed the significance of the commentary, and of her own reaction to it.

The fact that hormonal fluctuations affect the cognitive functioning of individuals of both sexes is of interest and potential usefulness, when reported and used in a way which does not render difference as unequal. If we pause for a moment here to take a step back, what is interesting in this report is that the research that shows the daily and seasonal effects of the hormonal environment on men’s ability to think is not the research that is chosen to be presented at this scientific meeting or that is deemed worthy of reporting.\(^{109}\) Also interesting, the fact that the research has been carried out with both men and women is not used here as part of a defence against the criticisms of hidden agendas or bias. Clearly, in the context of menstrual cycle research, the two individuals of whom Kimura speaks are women. This failure to foreground research that finds in men the same kinds

\(^{109}\) Although it may have been presented, we simply do not know. One might assume, however, that if it had been so presented, Kimura may have provided some discussion here of its reception as a useful comparison.
of biological cycles, demands, weaknesses, and peculiarities as are so readily reported when found in women, is an inequality that works to sustain the well-worn dualistic notions of man/mind/culture, woman/body/nature.\textsuperscript{110}

It is axiomatic that studies of this kind, noteworthy for their political sensitivity, will inevitably attract both positive and negative attention. Findings such as these are a magnet for those who wish to perpetuate the long history of scientific ‘proofs’ of women’s incapacity already demonstrated in this chapter, and their use for the perpetuation of such beliefs must and will be pre-empted or countered as a matter of course. As well, more measured expressions of concern about, and interrogation of, the findings will often be intermingled with kneejerk reactions of denial, or with calls for research of this sort to not be undertaken at all. And of course, both the findings and the reactions to them – especially the more extreme reactions – are a gift to the reporter looking for a good story.\textsuperscript{111} Under these conditions, it is naïve at best to expect that the ‘real significance’ of this research will be regarded as a relatively neutral and non-judgmental comparison of two individuals of unspecified sex.

In any event, it seems unlikely that the scholarly attention paid to this research was solely concerned with its potential political implications (although this may well have been the focus of media reports). Where Kimura writes with some

\textsuperscript{110} As a further example of this failure, it is noteworthy that the section on the ‘Cognitive effects of sex hormones on adults’ in the Wizemann and Pardue report (2001) focuses almost exclusively on the effects of estrogen on the cognitive skills of women (see, in particular pp. 97-104).

\textsuperscript{111} Though, in this case, there is not enough detail provided to enable me to locate the media reports referred to with any certainty.
certainty, Melissa Hines is less convinced by the evidence for hormonal influences on cognitive abilities:

The idea that hormones have activational influences on human cognitive performance has come to be largely accepted. However, the precise nature of these effects remains unclear, and, as outlined below, the evidence that they exist at all is not strong (2004, p. 176).

Hines expresses deep misgivings about the usefulness of menstrual cycle studies. In particular, she notes a number of limitations with the studies conducted by Hampson and Kimura (Hampson, 1990a, 1990b, 1990c; Hampson & Kimura, 1988), including that they did not use the same measures, and nor did they analyse the data in the same way across the four studies. Most tellingly, she reports that the only skills that showed a consistent relationship with high estrogen levels across the studies were “more motor skills than cognitive ones” (Hines, 2004, p. 177). Speaking generally of studies of the relationship between menstrual cycle and hormonal influences on cognition, she concludes that for a number of reasons, these are not the most rigorous way to identify hormonal effects on cognition:

Particularly if hormones are not measured, it is relatively easy to give women cognitive tests and ask them to report their cycle phase or their use of contraceptives. This could lead to a situation where many studies are done, but only those reporting the expected results are published (2004, p. 178).

Hines further notes that studies conducted on the relationship between testosterone and cognitive function in men, and on the influence of estrogen replacement on cognitive ability in postmenopausal women, are equivocal. Some of the male studies report positive effects, some negative, and some no effect at all (2004, pp. 178-179). Similarly, for postmenopausal women, there are some

---

112 I have chosen to compare Kimura and Hines because both are women, and because of the similarities in their professional backgrounds – both are Professors of Psychology, and both have been conducting research in this field for over 20 years.
studies which show a positive effect of estrogen replacement therapy, but not all do so, and in any event, there are questions of selection bias, in that better educated, more health-oriented women may be more likely to take estrogen (2004, pp. 179-180). In sum, she says:

Taken together, the results of the many investigations of activational influences of androgen and estrogen on human cognitive performance do not provide convincing evidence that such influences exist. Although some studies have reported positive results, others have not, and the likelihood that a study will find an association between hormones and cognition is not related to its methodological rigour (2004, p. 180).

All of this is not to say that Hines disagrees with Kimura on the fact of sex differences across a range of skills and behaviours. However, the differences in approach to this material, and the conclusions drawn from it by these two researchers, are stark. At the outset, Hines points to the particular difficulties that complicate research in this field. Following Maccoby & Jacklin (1974), she reports these as including: over-reporting of significant differences or of positive results; the influence of stereotypical beliefs on both the researcher and the researched; situational specificity, and the use of different study methodologies leading to incompatible results (Hines, 2004, pp. 5-8).

In an account of individual sex differences, Hines lists many as negligible. These include general intelligence, vocabulary, reading comprehension, mathematical and verbal ability, and spatial visualisation. Small to moderate sex differences are found in tests of two-dimensional mental rotation, problem-solving, spatial perception, rough-and-tumble play, and aggression (favouring males); and in computational skill (especially in childhood), speech/verbal production and
verbal fluency (favouring females). Both sexes show substantial differences in preferences for same-sex playmates, and sex-specific toy preferences are said to be present from a young age and can be large. Studies have found a large sex difference favouring males in tests of three-dimensional mental rotation (2004, pp. 11-17) and she notes that unlike some other specific skills such as mathematical ability and visuospatial ability, the sex difference gap in this skill has not changed (2004, pp. 160-161). She argues, however, that these sex differences are unlikely to seriously affect women’s occupational success:

... sex differences in circumscribed abilities, even if found to be completely determined by hormones [are unlikely to] seriously limit the ability of women to succeed at occupations that require a broad range of abilities. For instance, most scientific fields require numerous skills in addition to (and probably even more than) the ability to rotate a three-dimensional shape in the mind (2004, p. 222).

By contrast, in her discussion of mathematical aptitude, Kimura first questions the usefulness of using parental and teacher expectations as an explanation for boys’ superior performance, and challenges findings of a real decrease in differences in math test scores. Citing the 1990 study conducted by Janet Hyde and colleagues (Hyde, Fennema, & Lamon, 1990), she notes that there is insufficient detail of the test characteristics used in the various studies to be able to draw firm conclusions about any narrowing of the gap between the sexes (Kimura, 2000, pp. 67-69). With the probable continued existence of the gap thus asserted, Kimura’s explanation for it is a straightforward one: girls are better at simple mathematical tasks (that is, computation, considered to be the least complex task in mathematics), while boys are better at the most complex task, of

---

113 Similar problems to those Hines associates with the menstrual cycle studies cited by Kimura.
finding the correct solution (2000, p. 72). Querying why it is that girls do as well as boys in their grades, but not as well in aptitude tests, she asks:

Is it that women, on average, are more organized in their study habits, and/or that they write better exams? Do they work harder, or are they more motivated to get good grades? Or is it that they do better when dealing with material that is familiar, where solutions have already been presented and novel problem solving is not required? (2000, p. 74, my emphasis).

Is it, in other words, that women are less creative? And consequently, perhaps, less intelligent? This may not be what is meant, but when read together with a following discussion about the comparable productivity of male and female scientists (2000, pp. 75-77), it is fair to conclude that this is the direction she is heading. Coupling findings that females do less well on tests of mathematical aptitude, with statements that males scientists are more productive even where professional age and educational background are equal (and making no mention of the probability that the life experiences and situations of these male and female scientists are almost certainly not equal), she reasons that the aptitude tests predict competence in occupation. Her inference that women are less competent as scientists, because less competent mathematically, is almost inescapable.

A decade earlier, in 1990, Janet Hyde announced: “We have come a long way from the belief, a century ago, that women’s brains, and therefore their mental abilities, are smaller than men’s” (1990; reprinted in Laslett, Kohlstedt, Longino, & Hammonds, 1996, p. 319). Yet Kimura’s work suggests we have not travelled very far from the intent of such craniological research. Nor, as it happens, has craniology disappeared. In research published just two years after Hyde’s
comment, C. Davison Ankney (1992) and J. Philippe Rushton (1992) reported
that, after controlling for body size, men’s brains are larger and heavier than
those of women. Since then, there has been considerable research conducted into
overall intelligence, working memory and comparative brain volume. Brain
anatomy and size and intelligence have been correlated in a number of
contemporary studies (see, for example, Gur et al., 1999; Jensen, 1998; Jung &
Haier, 2007; Lynn, 1999; Lynn & Irwing, 2008; McDaniel, 2005; Reiss, Abrams,
Singer, Ross, & Denckla, 1996; Wickett, Vernon, & Lee, 1994, 2000). At the same
time, there have been an increasing number of studies investigating the role
played by genetics in the relationship between intelligence and brain volume,
with most, but not all, concluding that heritability is significant (see, for example,
Dambrun & Taylor, 2005; Pennington et al., 2000; Posthuma et al., 2002;
Posthuma et al., 2003; Rushton & Osborne, 1995; Schoenemann, Budinger, Sarich,
& Wang, 2000; Thompson et al., 2001; Toga & Thompson, 2005; Winterer &
Goldman, 2003).114

In 1996, Allan Reiss and colleagues reported that, from their MRI studies of the
brains of children and adolescents aged between 5 and 17 years old:

The results show that total cerebral volume is 10% larger in boys compared with
girls … Increased corticol grey matter is the primary contributor to larger brain
volume in boys … IQ is positively correlated with total cerebral volume in
children, in particular, with the volume of corticol grey matter in the prefrontal

---

114 Michael Dambrun & Donald Taylor (2005) argue, rather, that differences in cognitive ability are
‘largely illusory’, while P. Thomas Schoenemann & colleagues (2000) find that, although still
potentially important, genetic associations may be ‘quite small’, and that non-genetic factors may
have a role in the association between brain volume and cognitive ability.

115 Shaw et al qualify the importance of the grey matter/IQ relationship in children, stating that
“‘Brainy’ children are not cleverer solely by virtue of having more or less grey matter at any one
Other research with adult subjects also points to the correlation between IQ and the volume of cortical grey matter in the brain (Gur et al., 1999; Thompson et al., 2001). In his meta-analysis of 37 studies, Michael McDaniel finds a mean correlation between brain volume and intelligence of 0.33, with a higher correlation (0.40) for females than for males (0.34), and concludes that it “is very clear that brain volume and intelligence are related” (2005, pp. 342-343).

In related research, John Wickett and colleagues examined the relationship between IQ, head size, and brain volume in a sample of healthy, right-handed adult male sibling pairs, reporting that there “is no longer any doubt that a larger brain predicts greater intelligence” (2000, p. 1096). This finding complements that of previous work undertaken by the same researchers with a sample of healthy, right-handed adult females, which also concluded that brain size was positively correlated with intelligence (1994, p. 831). However, what is more interesting about the later study are the conclusions drawn in relation to the positive correlation between head size and intelligence: “Overall, the indication is quite clear that the size of the head predicts the size of the brain” (2000, p. 1119); effectively, a “larger head indicates greater intelligence” (2000, p. 1105).

---

116 Gur et al report that women have a higher percentage of grey matter, while men have higher percentages of white matter and cerebrospinal fluid (1999, p. 4065). This study differs from other studies which have found that men generally have higher volumes of all three (Posthuma et al., 2003; Reiss, Abrams, Singer, Ross, & Denckla, 1996).

117 This relationship is tentatively supported by Jung & Haier who note that “At this point, it does appear that, across several studies and groups, the relationship of intelligence to white matter volumes, chemical composition, and perhaps water diffusivity may be higher in women than in men” (2007, p. 153).
All this, of course, again raises the ‘elephant’ problem, which contemporary researchers overcome by noting that although humans have neither the largest brain nor the greatest cortical volume, recent research has found that we do have a “relatively large number of cortical neurons (~11,000 million), and relatively high conduction velocity between those neurons”, and, “the prevailing lore is strongly prejudiced toward the relatively massive (when compared with other species) human frontal lobes as causally related to intelligence” (Jung & Haier, 2007, p. 136). In other words, when comparing intelligence inter-species, overall size is less important than the size of specific areas, the concentration of neurons, and the speed of the connections within the brain.

Intra-species, however, the logic of this research seems to lead somewhere rather different. Here, the expectation that men will have larger heads (because of their generally larger body size) begins a chain of premises: if the size of the body predicts the size of the head, and the size of the head predicts the size of the brain; if larger brains generally predict greater intelligence; and if men not only have a larger cerebellar volume, but do indeed also have larger volumes of the ‘little grey cells’ and white matter that are implicated in higher IQ, then men can, in general, be expected to be more intelligent than women.\(^{118}\)

While the main body of research avoids stating this in so many words, Richard Lynn and Paul Irwing confront it explicitly, claiming an adult male advantage of

\(^{118}\) I note, however, that at least one research group finds that while body size is related to the size of the head, it does not appear to be related to the size of the brain, and they discount body size as a mediating factor in the correlations between head size and cognitive ability (Wickett, Vernon, & Lee, 2000, pp. 1109-1110).
between 3.5 and 6 IQ points (Lynn, 1999; Lynn & Irwing, 2004; Irwing & Lynn, 2005, 2006), with intelligence defined as “the sum of verbal comprehension, reasoning and spatial abilities” (Lynn, 1999, pp. 5-6). Research conducted by Douglas Jackson and J. Philippe Rushton (2006), using 1991 Scholastic Aptitude Test (SAT) outcomes for 46,509 male and 56,007 female 17-18 year olds, similarly concludes that there is a 3.63 IQ point difference in favour of males. It would seem that debates around the relevance of those ‘missing’ five ounces must once again be confronted.

Conclusion

So at birth the twig is already bent a little – what are we to make of that? (Wilson, 1978, p. 132)

Progressively greater insight into and knowledge about human genetics and the complexity of biological process, new imaging and other technologies, and increased interest and therefore funding, are all contributing to an invigorated focus on contemporary studies of sex difference. This program of research, which (somewhat naively) I expected to be the most progressive, and the most useful of

119 Curiously, these differences in IQ points are almost the same as the brain weight differentials between men and women found by Paul Broca and discussed earlier in this chapter.

120 Both Lynn and Rushton are controversial figures, this controversy arising in particular from their work on comparative intelligence between different racial groups. Lynn is Professor Emeritus of Psychology at the University of Ulster; Rushton is a professor at the University of Western Ontario, and a Guggenheim Fellow. Rushton is current President and Lynn a Director of The Pioneer Fund, a non-profit organisation founded to “advance the scientific study of heredity and human differences”, funding research into behavioral genetics, intelligence, social demography, and group differences of sex, social class, and race. Despite its claim that it is neutral on social and political issues, the Fund has been criticised as supporting racist and eugenicist views. Its first president was Harry Hamilton Laughlin 1880-1943, inaugural Director of the Eugenics Record Office at Cold Spring Harbour, and active in the American eugenics movement, in particular the movement for compulsory sterilisation. The Fund’s website can be found at http://www.pioneerfund.org/.
the reductionist ‘vampires’ for my purposes when I began this thesis, has lived up to this latter promise in ways I did not anticipate.

Certainly, as I expected, there is enough reputable research to support the notion that biology plays a part in the formation of sex-differentiated psychological characteristics across a range of behaviours and skills. And in contrast to the historical literature presented here, contemporary research into these differences is, as Cynthia Russett noted (1989, p. 14), couched in new, more careful, more sophisticated vocabularies. What is most interesting, however, when compared to the generally more progressive approach to the fluidity of biology in research outcomes within and since the Human Genome Project, is how little the basic presumptions operating here have changed over time. Many of the preconceptions, inferences, and conclusions drawn from modern sex difference research still show evidence of, and are considerably hampered by, the patrilineal heritage elaborated in this chapter.

That is, the implications of the differences are often still critically examined from within a deficit model. It is significant, for example, that females, described as the ‘basic’ or ‘default’ human type within other reductionist models discussed in this thesis (and still often leading to a conclusion that males are in some way ‘added value’), are here so indisputably ‘the bent twig’, the deviation from the (male) norm. The false question, so eloquently identified by Carol Bacchi (1990) – why are women different from men? – permeates sex difference research, with the inescapable consequence that women are still positioned as the ‘problem’ (how
do we accommodate the shortcomings and peculiarities of women?). On the other hand, as I have demonstrated, effectively nothing at all is said about the biological constraints operating on men, with the result that men are both empowered and freed within this paradigm.

While I am still entirely reluctant to ascribe conscious ill-intention to any scientist, my ‘walk’ around this particular reductionist enterprise has served to re-emphasise the poverty of claiming objectivity, where responsibility for and investment in the social implications of one’s research are left outside the laboratory door. It is difficult to escape the conclusion that, for the most part, the intent of sex difference research has changed little, and its ways of thinking are still shaped by deeply embedded cultural beliefs about ‘science’, and about women and men, that continue to underpin (some) scientific research and its reception and reporting in society. So, if we ask the question: what is the real-world usefulness of this research, the answer must be that, for the overwhelming majority of it, it is pretty much the same as it has always been, and that is to reaffirm and support the status quo.

As it stands, then, there is little comfort here for feminists, unless it be in the more moderate voices from within the discipline who are explicitly challenging not the fact of difference itself, but the presuppositions and implications that are evident in the selection of questions to be pursued, the investigative methodologies chosen, the meanings attributed to difference, and the ways in which results are being conveyed. These voices are relatively few, and
comparatively muted, yet they do demonstrate the existence of internal resistance to the more familiar conservative and deeply reactionary voices. Together with the growing accretion of knowledge about the biological bases of human *sameness* as well as about human lability *and* variability, these more balanced approaches to the meanings to be taken away from sex difference research potentially allow us to more positively theorise biology’s role in the construction of sexed bodies. And it is to the reductionist stories about the social consequences of biology’s contributions to sexed bodies that I now turn.
Four
‘Just so’ stories? : sociobiology as persistent paradigm

What on earth do you think you are, if not a robot, albeit a very complicated one?
Richard Dawkins (2006, pp. 270-271)[121]

We have at last come to the key phrase: genetic determinism. On its interpretation depends the entire relation between biology and the social sciences.
Edward O. Wilson (1978, p. 55)

Introduction
Sociobiology, although ubiquitous in popular culture and well represented within the academy, has always been a vexed subject, evoking equally passionate espousal, repudiation and defence. The application of biological (genetic) and evolutionary determinative pressures to human society and individual social behaviour is defended by its adherents as scientific method and welcomed as an explanation for everyday, ‘commonsense’ observations of the world. Its opponents have criticised it for providing support for a discriminatory status quo, comfort for eugenicists, and as having profound implications for concepts of free will and individual responsibility, the efficacy of liberal social policy, and beliefs about humanity’s unique and superior place in the scheme of things. Condemnation of the theories has, in some cases, escalated to attacks on

individual sociobiologists, who have been recipients of the full spectrum of critics’ displeasure, ranging from immoderate verbal attacks, to demonstrations, to threats of and actual physical assault, and bomb threats.\textsuperscript{122}

Generally accepted as having its modern beginnings in 1975 with the publication of Edward O. Wilson’s *Sociobiology: a new synthesis* (hereafter *Sociobiology*), sociobiology is interpreted by its many critics and commentators – among them distinguished biologists, philosophers and feminists – as social Darwinism, reductionism, and/or biological determinism (for example, Bleier, 1984; Ehrlich & Feldman, 2003; Fausto-Sterling, 2000a; Grosz, 1999; Kaplan & Rogers, 2003; Lewontin, 2000a; Midgley, 1979, 1983; Rose, Kamin, & Lewontin, 1984; Rose & Rose, 2000; Sayers, 1982).\textsuperscript{123} Within the academy, Wilson’s attempted subsumption of the social sciences and humanities under the umbrella of (socio)biology created further conflict (see Segerstråle, 2000, p. 86).

Most importantly for my purposes, sociobiology is widely regarded as anathema in much of the feminist literature. Ruth Bleier, writing in 1984, encapsulates the criticisms of many:

> Because Wilsonian sociobiology is a particularly dramatic contemporary version of biological determinist theories of human behavior, because it is powerful and persuasive, because it is a particularly good example of bad science, because it provides “scientific” support for a dominant political ideology that directly

\textsuperscript{122} Mary Midgley’s article in the journal *Philosophy* (1979) criticising Richard Dawkins’ *The Selfish Gene* is a well-known example of an immoderate verbal attack; Lionel Tiger writes of bomb threats and threatened ‘kneecapping’ for “venturing to explore the role of biology in our social lives” (1998); and, at a meeting for the American Association for the Advancement of Science (AAAS) in Washington, DC in February 1978, E.O. Wilson was doused with a jug of ice-water as he stood at the microphone to deliver his talk (Segerstråle, 2000, p. 23).

\textsuperscript{123} As Ullica Segerstråle points out, this is also possibly because of its grammatical resemblance to the German word *Sozialbiologie*, linking it to past eugenic practices (2000, p. 26). Past and potential future eugenics is discussed in chapter 3.
opposes every goal and issue raised by the women’s movement, and because it has been aggressively marketed and perceptibly incorporated into our culture, it seems a fitting area with which to begin the examination of science and scientific theories of biological determinism (1984, p. 15).

These attitudes have not softened over time. In a paper written in 1999, slightly amended and republished in 2005, Elizabeth Grosz calls for a possible alliance between Darwinism and feminism, arguing for a “more open feminist inquiry into the value and relevance of any discourse, not just Darwin’s”. Such an inquiry, she states:

involves not only feminist critique, not simply inspection for errors and points of contention, but more passively and thus dangerously, a preparedness to provisionally accept the framework and guiding principles of that discourse or position in order to access, understand, and possibly transform it (2005, pp. 27-28).

At the same time, she endorses the need to differentiate Darwin from the social Darwinian adoption of his concepts – for example, approving of the way Janet Sayers,

carefully distinguishes Darwin’s theory from the more pointedly politicized and self-serving readings of the social Darwinists of Darwin’s own times, and their current counterparts, sociobiologists (2005, p. 14, my emphasis).

While there is ample feminist critical inquiry into sociobiological discourse, there is little evidence of any such passive/dangerous preparedness to even provisionally accept the framework of sociobiology, or to assess the potential effect/value of an integration of its theories (as distinct from Darwin’s theory) with feminist body theory. Unsurprisingly, most feminist interaction with sociobiology is focused on critique of its ‘suspect’ science and of its use in support of a male dominant and oppressive status quo (see, for example, Bleier,
1984; Fausto-Sterling, 1985, 2000a; Greene, 2004; Leavitt & Gordon, 1988; Nelkin, 2000; Nelson, 2003; Oyama, 1991; Sayers, 1982; Schell, 2007; Segal, 2001; Spanier, 1995; Sperling, 1996; Tang-Martinez, 1997). This is not to say there are not sympathetic and transformational feminist readings of some sociobiological theory (for example, Gowaty, 1997c; Hrdy, 1999, original publication 1981; Vandermassen, 2005), but these are the exception rather than the rule; they rarely intersect directly with feminist body theory, and even more rarely emanate from feminists who are not also scientists.\textsuperscript{124}

As stated earlier, and evidenced in the previous two chapters, there is a continuing resurgence of interest in finding biological explanations for human sex differences. That this extends to a greater interest in evolutionary and sociobiological theory is evident in the visibility of high profile exponents (such as Richard Dawkins and Steven Pinker) in the media, the wealth of new texts being published in the field, and in the extensive coverage and celebrations for the 200\textsuperscript{th} birthday of Charles Darwin on 12 February 2009.\textsuperscript{125} Given this level of interest, and with the weight and pace of scientific research and discovery discussed in the previous two chapters poised to further unsettle comfortable notions of the body’s meanings and agencies, it seems to me that a “more open feminist inquiry into the value and relevance” of sociobiological discourse is timely. Yet any such attempt has been largely foreclosed for feminist body theory, because the general – if not unanimous – consensus appears to concur

\textsuperscript{124} Janet Radcliffe Richards and Griet Vandermassen are such exceptions; they are philosophers whose work attempts to bridge the gap between evolutionary psychology and feminism.

\textsuperscript{125} And as I review this, I have received a call for the 2009 Feminist Ethics & Social Theory conference to be held this year in Florida, which will include a panel discussion on evolutionary psychology.
with Zuleyma Tang-Martinez’s assertion that there are ‘irreconcilable differences’ between the two endeavours (Tang-Martinez, 1997). This notion, which is implicit or, indeed, asserted explicitly in the critical literature, requires unpicking if you suspect, as I do, that sociobiology may have something to offer to feminist conversations about differences, dualisms and related theoretical dilemmas.

In this chapter, then, I propose that conversations between sociobiologists and feminist body theorists have become unproductive, and ask whether there is a way through the ideological differences that impede more positive dialogue. I do this by looking closely at what the sociobiologists actually say, with a specific focus on the vexed questions of the ‘genetic determinism’ of both universal human, and sex differentiated, behaviours. To do this, I propose to ignore Grosz’s strictures against sociobiology and to accept some of its frameworks and guiding principles as a starting point for dialogue.

Two questions and a personal belief motivate this approach. Many feminists, including many of those working within science and sociobiology, share misgivings about science as it is practised and constructed and, particularly, about any science which in its application tends to prop up those status quo dichotomies which are detrimental to the interests of women.126 While the many excellent reasons for caution and mistrust have been extensively documented, I have an apprehension that, together with ideological difference, this may have created ‘blind spots’, leading to some misinterpretation and misunderstanding.

126 So, for example, intellectual/emotional; passive/active; hunter/gatherer (lunch chaser/nest defender); private/public; competitive/collaborative, and so on.
and obscuring the possibilities for constructive communication. More importantly, I wonder: If the sociobiologists prove to be right, how do we salvage a feminist body theory that has rejected their framework? Finally, and recognising that even a generous reading of the work of some individual sociobiologists cannot help but detect an apparent misogyny, I prefer to believe (perhaps naively) that, generally, sociobiologists are not engaged in a conspiracy to subvert women’s agitation for change/liberation.

Personal beliefs aside, my interest lies in examining the literature for its positive aspects (as a foundation for more productive conversations), so I will not be preoccupied with instances of misogynist bias here. Nonetheless, I acknowledge it, and it is important to remember, as an underlying theme, that sociobiology as a field has opened itself up to criticism of its academic enterprise by failing to critically scrutinise misogynistic claims in its own literature, and by not more vigorously policing and protesting against the extension of those claims into popular culture. This latter point will be taken up in the next chapter, which looks at the ways in which sociobiological theory is popularised.

Beginning, then, from an acceptance that academic sociobiology can legitimately be called a science (the debate on this question being tangential to my argument and too large to encompass here), I aim to step inside sociobiological discourse to engage with it as an ‘intelligent reader’ rather than simply subjecting it to critique. An intelligent reader in this context is someone who, entering this body

---

127 But see, for example, Fausto Sterling (1985), Kaplan & Rogers (2003), Rogers (2000) and Rose & Rose (2000).
of work without preconceptions or distrust, is prepared to read more generously
and to be open to the positive interpretations available – and to the possibilities
for rescuing it from its misogynistic leanings – without sacrificing the critical
faculties of the average intelligent reader.

I have been writing as though ‘sociobiology’ is a discrete and integrated field of
study, and, of course, like ‘feminism’ it is no such thing. Just as there are many
feminisms, which straddle, cross and recross boundaries, defying simple
taxonomy, there are different schools of thought within sociobiology, each
branching out, like evolution itself, as variations from a central theme. Also like
feminism, sociobiology as an academic framework is a very broad church,
drawing proponents from across the academy with disciplinary affiliations to
fields such as zoology, ethology, ornithology, evolutionary and developmental
biology, genetics, primatology, paleontology and psychology, but encompassing
also anthropologists, philosophers, feminists and theoreticians.

Sociobiology is an interdisciplinary, multilayered, but still relatively cohesive
field of inquiry. That is, just as diverse feminisms share a fundamental concern
for the wellbeing of women, what unites the diverse endeavours that fall under
the rubric of sociobiology is their shared pursuit of a “systematic study of the
biological basis of all social behavior” (Wilson, 1975, p. 4). Although it is not a
well-regarded word, I choose to use ‘sociobiology’ in preference to other more
neutral terms (such as evolutionary biology/biologist, for example), for a couple
of reasons. Firstly, it succinctly describes the elements of the discussion – the
social and the biological – and the relationship between them; and secondly, I
want to challenge the negativity surrounding it and begin to reclaim the word as a useful one. For my purposes then, ‘sociobiology’ represents the entire field (and, for convenience, I will also refer to researchers in the field as ‘sociobiologists’ throughout, although many of them would not claim this label for themselves). Within this field, I identify three main (both more and less interrelated) approaches: two of these are academic, broadly characterised as ‘classic’ sociobiology and evolutionary psychology, discussed in this chapter; and the third is ‘popular’ sociobiology – which, I suggest, is more aptly labelled described as vulgar reductionism – which is examined in the next chapter.\footnote{128}

The first distinction that can be drawn between ‘classic’ sociobiology and evolutionary psychology is a disciplinary one: with some few exceptions, ‘classic’ sociobiologists are primarily evolutionary biologists, while evolutionary psychologists are overwhelmingly drawn from the ranks of psychologists and anthropologists.\footnote{129} ‘Classic’ sociobiologists focus on identifying the evolutionary mechanisms of natural selection, and in pursuing this aim they cast a wide net and tend to use animal/insect models for specific behavioural examples.\footnote{130} Thus, most of Wilson’s observations on behaviour in humans are drawn from his study of ants; Dawkins makes use of a variety of animal models; and feminist

\footnote{128} Sayers (1982, p. 79) claims that Wilson crosses the line into “‘vulgar’ sociobiology”.

\footnote{129} Within these definitions, Wilson, Dawkins, Maynard Smith, DeVore, Huxley, Mayr, Trivers, Haldane, Fisher, Dobzhansky, Gowaty and Hamilton are in the former group; Cosmides, Tooby, Barkow, Buss, Pinker and Miller are in the latter. Anthropologist Sarah Blaffer Hrdy, however, self-identifies as a sociobiologist, and feminist philosopher Griet Vandermaassen defends evolutionary psychology.

\footnote{130} Dawkins has said in interview that he “almost never has humans in mind” (1996, para 5).
anthropologists Sarah Blaffer Hrdy and Barbara Smuts\textsuperscript{131} extrapolate human behavioural evolution from the study of non-human primates.

Evolutionary psychologists share this interest in identifying evolutionary mechanisms. Since they focus specifically on human psychology, their models are human, and their interest lies in linking proximate human behavioural traits to ultimate evolutionary pressures. To do this, they apply the concept of conceptual integration to the fields within which they place their study:

\textit{... to understand the relationship between biology and culture one must first understand the architecture of our evolved psychology. Past attempts to leapfrog the psychological – to apply evolutionary biology directly to human social life – have for this reason not always been successful ... it is now possible to provide conceptually integrated analyses of specific questions: analyses that move step by step, integrating evolutionary biology with psychology, and psychology with social and cultural phenomena (Cosmides, Tooby, & Barkow, 1995, p. 3).}

The other important distinction is in the way in which some evolutionary psychologists conceptualise the structure of the human brain as composed of both ‘content-independent’ and ‘content-specific devices’, adaptations which have evolved “to solve long-enduring adaptive problems characteristic of our hunter-gatherer past” (Tooby & Cosmides, 1995, pp. 49-50). John Tooby and Leda Cosmides provide, as examples of these: a language acquisition device; mate preference mechanisms; sexual jealousy mechanisms; mother-infant emotion communication signals, and social contract algorithms (1995, p. 39). They go on to state that:

\begin{quote}
Our ability to perform most of the environmentally engaged, richly contingent activities that we do depends on the guiding presence of a large number of highly specialized psychological mechanisms. Far from constraining, specialized
\end{quote}

\textsuperscript{131} Barbara Smuts is an anthropologist and psychologist, and lists her area of expertise as ‘biopsychology’.
mechanisms enable competences and actions that would not be possible were they absent from the architecture (1995, p. 39).

With these distinctions in mind, I will largely discuss both approaches together under the rubric of ‘sociobiology’ since, for my purposes, they share a common project, albeit each with a slightly different focus.

The Biogram

... although many social and biological scientists are willing to concede that the body is full of the most intricately functional machinery, heavily organized by natural selection, they remain skeptical that the same is true of the mind (Tooby & Cosmides, 1995, p. 57).

All sociobiology presupposes the existence of a biogram\textsuperscript{132} that provides a template of qualities shared by all human beings, while allowing for a large measure of flexibility at the level of the individual. A flexible-within-limits biogram for physical characteristics is evident and non-controversial: clearly a genetic ‘recipe’ exists which ensures that the offspring of humans are human in physical form and function. For the most part, we understand and accept the general constraints of this biogram (none of us can fly unaided, for example), the specific physical constraints imposed upon individuals by their sex (only women bear children; men, on average, are stronger and heavier than women), and the social constraints imposed by the wide diversity of human physical attributes which range from the relatively trivial (at 155cm tall, I will never be able to reach the top cupboard unaided), to the life-limiting.

However, as its name suggests, sociobiology extends the concept of the biogram to include psychological and social qualities:

\textsuperscript{132} Effectively, an evolved biological ‘blueprint, although ‘blueprint’ has been questioned as an appropriate metaphor. See Richard Dawkins (2004, p. 105 ff), who proposes instead the use of ‘recipe’ or the metaphor of the body as a blanket suspended by rubber bands.
The evidence is very strong that there does exist a human biogram, a pattern of potentials built into the heredity of the species as a whole (E.O. Wilson, Foreword, in Barash, 1977, p. xiv).\textsuperscript{133}

While it is commonplace to regard other animals (and insects) as having, in addition to a physical biogram, inherent natures which guide their everyday behaviour, this hypothesis is fiercely contested when applied to humans, largely because of the implication of a determinative human psychology which then predisposes us to certain social structures. Indeed, in some sociobiological literature, ‘culture’ has been theorised explicitly as part of the human social biogram (Carruthers, Laurence, & Stich, 2006; Dawkins, 1999, original publication 1982; Gangestad & Simpson, 2007b; Hill, 2007; Lumsden & Wilson, 1981, 1985; Richerson & Boyd, 2006; Tooby & Cosmides, 1995). More specifically, sociobiology, in common with some behavioural genetics and sex difference research, theorises both a universal human social biogram (that both men and women share an evolved predisposition to altruism, for example), and a differential female and male social biogram on the basis of our differently sexed physical biograms. Thus, while the common constraints of the human physical biogram are by and large uncontroversial, as are the general abilities and constraints of the sexed physical biogram, the same cannot be said of the psychological and social constraints that, in sociobiological theory, inform a sexed social biogram arising out of our sexed physical biograms. A clearer understanding of this implication is a focus of this chapter.

\textsuperscript{133} See also Tiger & Fox (1971) for their concept of a ‘biogrammar’. 

148
Positive engagement with sociobiological discourse is not possible without first accepting the principle of the social biogram as an organising framework, and I do so unequivocally. This acceptance creates a simultaneous sense of comfort and discomfort: comfort because it assists in resolving my longstanding disquiet at the recognition of evolutionary processes on body but not mind, and because I see some potential in this discourse for resolving the problem of the body/mind split; discomfort because the social biogram, as it is currently interpreted, provides a basis for claims and social practices which are harmful to women, and, as Ruth Bleier notes, opposed to feminist goals.

My acceptance of the reality of some form of social biogram is nevertheless unequivocal because the strong social constructionist or blank slate view of mind – what has been called the Standard Social Science Model (SSSM)\(^{134}\) – has always seemed to me untenable, and the complex interplay of biology and culture is by now so well established that holding to this view is clearly indefensible. Indeed, commentators on both sides of the nature/nurture divide have long agreed upon and voiced some variation on the view that culture and biology are so closely intertwined that any attempt to clearly separate one from the other is futile.

Feminist Ruth Bleier says:

> … most importantly, it is not possible the tease apart genetic and other biological factors from environmental and learning factors in human development (1984, p. 6).

And sociobiologist E.O. Wilson says, similarly:

> Here is what I believe the evidence shows: modest genetic differences exist between the sexes; the behavioral genes interact with virtually all existing environments to create a noticeable divergence in early psychological

\(^{134}\) See Barkow, Cosmides and Tooby (1995).
development; and the divergence is almost always widened in later psychological development by cultural sanctions and training. Societies can probably cancel the modest genetic differences entirely by careful planning and training (1978, p. 129).

Common as this view of cultural and biological complexity may be, though, it still conventionally generates a ‘yes, but’ response. ‘Yes, but’, the argument goes, ‘one of nature or nurture is the primary influence, because …’. Thus, seeming agreement that nature/nurture cannot be teased apart shifts, often imperceptibly, straight back into the dichotomy everyone claims to be intent on avoiding. Indeed, Sarah Hrdy speculates that: “Nature cannot be compartmentalised from nurture, yet something about human imaginations predisposes us to dichotomize the world that way” (1999, p. 147). And so, despite the best efforts of many commentators on both sides of the argument, nature and nurture persist as rival, quantifiable elements of a dichotomy.

The real argument then, it seems to me, has always been this one of quantification; or, as Wilson puts it:

The question of interest is no longer whether human social behavior is genetically determined; it is to what extent (1978, p. 19).\textsuperscript{135}

The sticking point is whether the biogram contains only the most basic of universal behaviours, “primitive drives like hunger and sex” (Fox, 1971, p. 279), or whether it provides an evolutionary foundation for far more significant social behaviours:

... we suspect that human biological universals are to be discovered more in the generalities of eating, excreting and sleeping than in such specific and highly variable habits as warfare, sexual exploitation of women and the use of money as a medium of exchange (Leeds et al., 1975, penultimate para).\textsuperscript{135}

See also Donald Symons' discussion of the characteristics of the nature/nurture argument (1987).
It seems to me that there is an inability on both sides of the argument to accept that the social/psychological biogram may be both limited (in its content), and limiting (in its effect), even though we have little difficulty doing so for the physical biogram (we both accept it and live happily with it – the diversity of the physical biogram is of little hindrance, except in the case of sex). On the one hand, sociobiology invests the social biogram with content of considerable potential determinative power. On the other, feminist theorists and other critics struggle with the notion that individual and cultural flexibility may be limited by an evolved social biogram.

Importantly, then, if the social biogram does contain more than primitive drives, predisposing us to perform (aspects of) culture only within certain limits, why are those limits (seen to be) invested with so much more determinative power than are the limits imposed upon us by the physical biogram? And why does sociobiology appear to only interpret that performance in ways that tend to limit individual flexibility and maintain the status quo? It is when we move out into this wider arena of behaviours long understood to be socially constructed (and thus assumed to be malleable, open to change), that the social biogram shifts sociobiology onto dangerous, contested ground:

For feminists, the central problem of evolutionary biology is biological determinism. Explicitly, this is the idea that the public and private subordination of women in our own society is inevitable because the sexes have been differentially selected for genetically controlled behaviors (Allen, 1997, p. 516).
Genetic determinism

So we have come at last, as Wilson says, to the key phrase. The charge of biological determinism encompasses two concepts: firstly, determinism *per se*, that is, that the genotype determines the phenotype; and, secondly, that determinism then directs and limits proximate individual human behaviour, and in turn shapes human culture and consequently the status quo.

Any bald assertion that ‘A *determines* B’ easily lends itself to the critics’ major complaint that sociobiological discourse is, like the language of genome and sex difference research, just another language of control. As such, it carries with it a heavy load of unwelcome baggage: of ideas about hierarchical worth, of ‘natural’ place (specifically in relation to women) and, more generally:

Sociobiology is a reductionist, biological determinist explanation of human existence. Its adherents claim, [first,] that the details of present and past social arrangements are the inevitable manifestations of the specific action of genes (Rose, Kamin, & Lewontin, 1984, p. 236).

“Inevitability” is the key to understanding these readings of the biological determinist character of sociobiology as social Darwinism. In this model, nature constrains nurture to the extent that there is little room for free will, individual improvement, and social mobility, and little point in policies aimed at achieving social justice. The implication here is that in a clash between the two, nature will inevitably limit nurture. So, for example, if one is born with an average IQ, this will always hold one back, no matter how enriched the environment may be, or how relevant IQ may be to an enriched or meaningful life or, indeed, to basic civil rights. The differing selection pressures on women to reproduce and to
nurture, acting through a universal and particularly female genetic endowment, will inevitably lead to their social subordination to men. This positions sociobiology as the quintessential essentialist and reductionist discourse, implying closure, finality, fixity; biology determines/directs individual capacity and behaviour towards an inevitable end.

Read in this way, it is unsurprising that sociobiology attracts forceful repudiation. It is also unsurprising that this is a reading and charge that sociobiologists have uniformly and vigorously denied. Richard Dawkins, for example, in reply to Rose, Kamin and Lewontin, stated:

Unfortunately, academic sociobiologists ... do not seem anywhere to have actually said that human social arrangements are the inevitable manifestations of genes... The myth of the “inevitability” of genetic effects has nothing whatever to do with sociobiology (1985, para 7).

Similarly, the late eminent biologist, John Maynard Smith, commenting more recently than Dawkins, and more broadly on the nature/nurture debate, said:

There has been fierce criticism of genetic determinism. It is seen as a deeply wicked attitude, which it very probably is, but I don’t know of any geneticists who actually hold that view. Holding the view that human beings have genetic tendencies does not mean that you believe those tendencies are inevitable (Humanist News, 2001, penultimate para).

In sum, there are a couple of things going on here: firstly, whether social (cultural) behaviours reside in the social biogram at all, and if so, why (only) these particular behaviours (which fortuitously just happen to prop up the status quo)? And secondly, how flexible or inflexible is the behavioural (cultural) repertoire of the social biogram said to be? There is abiding disagreement about
what the sociobiologists say they are saying on these issues, and how feminist and other critics read and interpret what they are saying.

So I return at this point to first principles: what do the primary texts actually say? If sociobiologists don’t champion inevitability, then in what does this perception of inevitability reside? To tease this out, we need to look at the cumulative effect of academic sociobiological theory on perceptions of the social biogram – its contents, its diversity, and its plasticity. What is said in the sociobiological literature about what makes up the social biogram: how are genotypes and phenotypes said to interact, what is said about genes and their effect on culture, and, importantly, how do these texts deal with ideas about the mutability of biology? Once we have a clearer picture of how biology’s agency is positioned, we then need to investigate which human universals and sex differences are explored and purportedly explained as being contained within the social biogram.

**Genotype/phenotype interaction**

The physical foundation of the social biogram is the genotype, one’s particular combination of inherited DNA, which is said to determine the phenotype – how we are formed, how we look and how we behave. Clearly, some phenotypic effects are determined by the genotype: as well as determining a human-specific morphology, it is also the sole determinant of particular traits in individual bodies; for example, my genotype determines that I have green eyes. The heritability of so-called Mendelian traits such as eye colour is well understood and non-controversial. It is also clear, however, that most characteristics are
polygenic in origin and many, including some physical characteristics, are almost certainly an outcome of the combination of genotype and environment. Maternal and pre- and post-natal nutrition, for example, may have a modifying effect on height. The sociobiological literature has long supported this view of the determinative power of the genotype as restricted and contingent, as the following selection shows.

No constant phenotypic effect need be associated with a particular gene. The substitution of one allele for another may have one effect in one genotype and an entirely different effect in another (Williams, 1996, original publication 1966, p. 56).

One of the greatest unknowns in human sociobiology concerns the exact limits and capabilities of genotype to influence behavioral phenotypes. But cautious open-mindedness seems most appropriate at this time (Barash, 1982, p. 160).

So, of the two effects that genes have on the world – manufacturing copies of themselves, and influencing phenotypes – the first is inflexible apart from the rare possibility of mutation; the second may be exceedingly flexible. I think a confusion between evolution and development is, then, partly responsible for the myth of genetic determinism (Dawkins, 1999, original publication 1982, p. 14).

... except for the rare behavioral conditions approaching total genetic determination, heritabilities are at best risky predictors of personal capacity in existing and future environments (Wilson, 1999, p. 154).

[A complex human nature] is not, of course, a nature that is rigidly programmed, impervious to the input, free of culture, or endowed with the minutiae of every concept and feeling. But it is a nature that is rich enough to take on the demands of seeing, moving, planning, talking, staying alive, making sense of the environment, and negotiating the world of other people (Pinker, 2003, pp. 100-101).

Each of these statements rejects any notion of an uncomplicated determinative power of genotype over phenotype; clearly, what these authors are talking about is the uncertainty involved in genotype/phenotype interactions, and the unpredictable and conditional nature of outcomes. However, much of the sociobiological literature quickly moves on from (or indeed, passes completely
over) this broader account to what appear to be a set of more reductive statements/assertions.

The essence of the argument, then, is that the brain exists because it promotes the survival and multiplication of the genes that direct its assembly. The human mind is a device for survival and reproduction, and reason is just one of its various techniques (Wilson, 1978, p. 2).

The gene specifies how development occurs, and that in turn specifies how behavior occurs (Ridley, 2004, p. 139).

Genes do not determine behavior like the roll of a player piano … At the same time, it is misleading to invoke environment dependence to deny the importance of understanding the effects of genes. To begin with, it is simply not true that any gene can have any effect in some environment, with the implication that we can always design an environment to produce whatever outcome we value … [studies] show numerous main effects of personality, intelligence, and behavior across a range of environmental variation (Pinker, 2004, p. 11).

… we, and all other animals, are machines created by our genes … I shall argue that a predominant quality to be expected in a successful gene is ruthless selfishness. This gene selfishness will usually give rise to selfishness in individual behaviour … Our genes may instruct us to be selfish, but we are not necessarily compelled to obey them all our lives (Dawkins, 2006, pp. 2-3).

Statements of this kind, with their apparent assertion of the social as subordinate to the biological, need to be unpicked, because shades of inevitability do indeed visibly hover around them. How do we reconcile Matt Ridley’s linear progression from gene to development to behaviour, or Dawkins’ linking of selfish genes to selfish individuals with, for example, Wilson’s statement that heritabilities are risky predictors of personal capacity? How are we to safely negotiate Pinker’s linking of genes and their ‘main effects’ on individual behavioural outcomes with his opening assertion that genes do not determine behaviour? To do this, we need to look next at the sociobiologists’ notion of what genes are, and of how they interact with culture.
What is a gene?

Wilhelm Johannsen is credited with having coined the word ‘gene’ in 1909:

> The ‘gene’ is nothing but a very applicable little word, easily combined with others ... As to the nature of the ‘genes,’ it is as yet of no value to propose any hypothesis; but that the notion of the ‘gene’ covers a reality is evident in Mendelism (Johannsen, quoted in Keller, 2000, p. 2).136

The applicability of Johannsen’s ‘little word’ is everywhere apparent almost a century later when gene-talk permeates our culture, and it carries a heavy load of expectation, disputation and ideological baggage (for a discussion of ‘gene talk’ see Keller, 2000, esp. pp. 136-148). While the notion of the gene is an ever-present reality, the reality of the notion is considerably more problematic since, despite significant advances in genetics, we seem no closer to a definitive and agreed upon explanation of what a ‘gene’ actually is, or even if any such discrete entity exists (Birke, 2000; Kaplan & Rogers, 2003; Keller, 1993b, 2000; Lewontin, 1993; Moore, 2001; Nelkin & Lindee, 1995a).

My purpose here, however, is not to examine arguments about the reality or composition of the gene. I am interested rather to uncover the ways in which it is deployed within sociobiological theory – as the basic unit of the genotype, as a concept, and as a fact – to imply (or indeed assert) its ‘causal’ nature, which is then read as in some way generating an inevitable course of physical, psychological, and social events. So I put those disputes about the reality of the gene to one side and accept Dawkins’ definition of a gene as “any portion of chromosomal material that potentially lasts for enough generations to serve as a

---

136 See also Ridley (2004, p. 232)
unit of natural selection” (2006, p. 28). I choose this particular definition because although, as Dawkins notes, there are differences in the ways in which the physical properties of the gene are delineated and described in the literature, geneticists and sociobiologists alike appear to agree that it is both ‘chromosomal material’ and the ‘unit of natural selection’.138

Matt Ridley then identifies seven descriptions of what a gene is: the gene as an archive (Mendel); an interchangeable part (de Vries); a disease averter or health giver (Garrod); a recipe (Watson-Crick); a switch (Jacob-Monod); a (selfish) replicator (Dawkins), and finally, drawing from sociobiology/evolutionary psychology, genes are “the mechanisms of experience” (2004, pp. 233-248). This final description, Ridley asserts, “integrates all the other six definitions” and further positions the gene as “a device for extracting information from the environment” (2004, p. 247).

Ridley’s first five descriptions of the gene are generally widely accepted: we know that genes copy and store the four bases of DNA (ATCG); that they are present in the same form in organisms other than humans, and in this sense are interchangeable;139 that they provide the ‘recipe’ or template that results in the

---

137 Dawkins credits this definition, worded somewhat differently, to George C. Williams, who said: “I use the term gene to mean ‘that which segregates and recombines with appreciable frequency.’ Such genes are potentially immortal, in the sense of there being no physiological limit to their survival, because of their potentially reproducing fast enough to compensate for their destruction by external agents” (Williams, 1996, original publication 1966, p. 24).

138 Theories of group selection have been “controversial from the start” (see Barash, 1982), although never completely abandoned, and have been extensively reconsidered in relatively recent times (see Sober & Wilson, 1999).

139 For example, researchers have successfully incorporated a gene from a firefly into the DNA of a tobacco plant (Campbell, Reece, & Meyers, 2006, p. 314).
production of proteins;\textsuperscript{140} that they switch on and off in given circumstances and in response to the actions or presence of other genes, and clearly genes can promote either health or disease.\textsuperscript{141} However, Ridley’s final two descriptions of genes as ‘selfish replicators’ and as ‘mechanisms of experience’ have attracted considerable controversy, since representing the gene in these ways imparts to it a capacity for influencing behaviour and culture well beyond that which follows necessarily from any individual physical biogram.

**Selfish replicators**

In *The selfish gene*, Dawkins starts from a conviction that the gene (rather than the individual or the group) is the unit of selection (heredity).\textsuperscript{142} He then synthesises Darwinian theories on aggression, sex ratios, inter-generational conflict, sex differences and reciprocal altruism to support his concept of genes as selfish replicators, hitching a ride through the generations safely contained within the bodies – the “gigantic lumbering robots” – they have built, and which are their “survival machines” (2006, pp. 19-20). Dawkins’ avowed interest, in this and his other work, lies in uncovering the means and methods of evolution, and, in particular, in embedding the theory of selection at the level of the gene, as against competing theories of selection which place it at the level of the individual or the

\textsuperscript{140} For a description of the basic process of transcription and translation, see Campbell et al, (2006, p. 311 ff). The notion of DNA or the ‘gene’ as an originary code has been the subject of some discussion and challenge in the feminist literature (see, for example, Diprose, 1991; Keller, 2000).

\textsuperscript{141} And although, in light of the search for ‘disease genes’ in both the pre- and post-genome era, they are more usually portrayed today as a cause rather than an averter of disease, Garrod’s characterisation of ‘unbroken’ genes as health giving is an important one, reflected in the growing literature on the relative merits of the wellness/illness paradigms in health practice.

\textsuperscript{142} A notion that is now widely accepted; however, in 1975, when Dawkins first published *The selfish gene*, it was not (see 2006, pp. 10-11). And, as noted earlier, theories of group selection – or what is now sometimes called multilevel selection (MLS theory) – are making something of a comeback. See Gangestad & Simpson (2007a) for a discussion of the debates surrounding group and individual selection.
The gene, as the unit of selection in this process, is selfish only insofar as its evolutionary interests lie in ensuring its survival; that is, in being selected into the next generation over its competing allele.\textsuperscript{143} Dawkins’ central theory then works at the level of the gene, not the individual.

That Dawkins felt his characterisation of genes as selfish replicators in \textit{The selfish gene} had been misconstrued, is clear from the chapter in his next book in which he discusses the “comic misunderstanding” of his use of language and metaphor as implying genetic determinism (1999, original publication 1982, pp. 9-29).\textsuperscript{144}

And despite his careful explanations in a variety of contexts over the intervening years, that he feels it is still misunderstood is evident from the following statement in his Introduction to the 30th anniversary edition:

> Many critics, especially vociferous ones learned in philosophy as I have discovered, prefer to read a book by title only ... I can readily see that ‘The Selfish Gene’ on its own, without the large footnote of the book itself, might give an inadequate impression of its contents (2006, pp. vii-viii).

Dawkins is justified in feeling aggrieved by much of the criticism directed at \textit{The selfish gene}. I pass over Mary Midgley’s critique (clearly the ‘vociferous philosopher’ referred to here), since I believe that her arguments are considerably weakened by her personal attack upon Dawkins and upon his scholarship.\textsuperscript{145}

---

\textsuperscript{143} An allele is defined on the NHGRI website as: “One of the variant forms of a gene at a particular locus, or location, on a chromosome. Different alleles produce variation in inherited characteristics such as hair color or blood type. In an individual, one form of the allele (the dominant one) may be expressed more than another form (the recessive one)”.


\textsuperscript{144} In Chapter 2: Genetic determinism and gene selectionism.

\textsuperscript{145} It is not surprising that Midgley’s patronising (“Up till now, I have not attended to Dawkins, thinking it unnecessary to break a butterfly upon a wheel”) and insulting piece (“Dawkins’s crude, cheap, blurred genetics is not just an expository device. It is the kingpin of his crude, cheap, blurred psychology”) still rankles some nearly 30 years later. In his note to page 55 of the 2006 anniversary edition, Dawkins refers to Midgley’s article as “highly intemperate and vicious” (p. 278); Daniel
However, other critics have formulated arguments around misreadings and/or misquotations of the text:

Of all the vulgar errors about biology presently circulating, the notion that we are “lumbering robots blindly programmed” by our genes which “control us body and mind” (Dawkins’ original dictum) is surely the most popular by a long shot (Lewontin, 1990, my emphasis).

Dawkins’ original dictum in fact was that our genes “created us, body and mind” (2006, p. 20, my emphasis). Remembering his central thesis that the gene is the unit of selection, it is not unreasonable for him to conclude that particular combinations of genes in individual bodies create us as biological entities. As he himself notes, within the framework of his argument, this is a long way from asserting that our genes control us. Looked at in this way, the slip from the suggestion of an originating source to one of control, can only be described as regrettably careless. Nonetheless, it has become so familiar a trope that it continues to be perpetuated in more recent texts. Among feminist commentators, for example, the notion that Dawkins, and sociobiologists more generally, asserts the all-powerful controlling nature of the gene continues to propagate (and also serves to link the sociobiological concept of the gene to the feminist critique of the ‘gene as master molecule’ rhetoric within the Human Genome Project). Gisela Kaplan and Lesley Rogers, for example, declare that Dawkins refers to “genes as ‘selfish’ and as ‘replicator units’ that control every aspect of our behaviour” (Kaplan & Rogers, 2003, p. 37).

Dennett refers to it as a “vituperative and uncomprehending dismissal” (1995, p. 362). See also See Dawkins’ reply to Midgley in Philosophy, and her further article (Dawkins, 1981; Midgley, 1983).

Dawkins notes that this misquotation can also be found in Not in our genes (Rose, Kamin, & Lewontin, 1984) at page 287 (Dawkins, 2006, p. 271).
In their defence, the slippage is not solely of the critics’ making. While Dawkins and others protest their innocence, they themselves create metaphors, speculate about the implications of specific genes, and draw conclusions that slip between creation and control. For example, Dawkins nowhere says that we are ‘lumbering robots blindly programmed’; he does, however, call us “survival machines – robot vehicles blindly programmed to preserve the selfish molecules known as genes” (2006, p. xxi). Later, however, he qualifies ‘blind programming’ by asserting that “it is the business of genes to program brains in advance so that on average they take decisions that pay off” (2006, p. 55). This programming, as he explains it, consists of “general strategies and tricks of the trade”, one of which is a capacity for learning (2006, pp. 55-57). What the genes ‘control’ in this scenario is not individuals or robots, but rather the provision of species-general capacities within individual bodies that are then able to be utilised in interactions with the environment (learning).

In *The extended phenotype*, published some six years after *The selfish gene*, he reflects on his choice of the “language of robotics” unaccompanied by sufficient explanation of his meaning:

> It was in an attempt to forestall errors like this [attributing foresight to genes] that I used the language of automation and robotics, and used the word ‘blindly’ in referring to genetic programming. But it is, of course, the genes that are blind, not the animals they program (1999, original publication 1982, p. 15),

and expresses his view that the behavioural effects of genes are limited:

> But, however inexorable and undeviating the genes may be as they march down the generations, the nature of their phenotypic effects on the bodies they flow through is by no means inexorable and undeviating. If I am homozygous for a gene G, nothing save mutation can prevent my passing G on to all my children. So much is inexorable. But whether or not I, or my children, show the phenotypic
effect normally associated with the possession of $G$ may depend very much on how we are brought up, what diet or education we experience, and what other genes we happen to possess (1999, original publication 1982, p. 14).

Other sociobiologists also make very clear statements on the interactive relationship between genes and culture:

Human social evolution is obviously more cultural than genetic. The point is that the underlying emotion [altruism], powerfully manifested in virtually all human societies, is what is considered to evolve through genes (Wilson, 1978, p. 153).

The capacity for rapid transmission and assimilation of new and adaptive behaviors was almost certainly favored in human evolution ... suggesting strong selective pressures favoring behavioral flexibility and innovation. This notion provides an interesting conceptual bridge between biologic and cultural factors in the evolution of human social behavior. Social learning and the passage of traditions are clearly cultural, whereas the capacity to perform such operations is biologic in terms of both the neural competence of individuals and the tendencies for social organization that make these operations possible (Barash, 1982, p. 203).

Neither ‘biology’, ‘evolution’, ‘society’, or ‘the environment’ directly impose behavioral outcomes, without an immensely long and intricate intervening chain of causation involving interactions with an entire configuration of other causal elements. Each link of such a chain offers a possible point of intervention to change the final outcome (Tooby & Cosmides, 1995, p. 39).

Culture is such a powerful set of cranes\(^{147}\) that its effects can swamp many – but not all – of the earlier genetic pressures and processes that created it and still coexist with it. We often make the mistake of confusing a cultural innovation with a genetic innovation. ... Anyone who worries about “genetic determinism” should be reminded that virtually all the differences discernible between the people of, say, Plato’s day and the people living today ... must be due to cultural changes ... (Dennett, 1995, p. 338).

The ontogenetic question is, Did genes play a role in the development of trait X? The answer is always yes, no matter what trait X is – adaptation or pathology, idiosyncrasy or species-typical organ – since every part of every organism emerges only via interactions among genes, gene products, and myriad environmental phenomena. And because the answer to the ontogenetic question is always yes, it is uninformative (Symons, 1995, p. 140).

\(^{147}\) Dennett contrasts ‘cranes’ – “excellent as lifters, and they have the decided advantage of being real” – with ‘skyhooks’ – the OED definition for which he cites as “an imaginary contrivance for attachment to the sky; an imagined means of suspension in the sky” – arguing that cranes were the instrument used in the ‘lifting’ process of evolution, rather than the “unsupported and unsupportable” skyhooks (see Dennett, 1995, pp. 73-84).
Anyone who studies behavior quickly realizes that it is impossible to separate the environment from the organism experiencing it, so that all traits are necessarily the result of an interaction between the animal and its perceived world ... The key point is this: only a difference between traits, and not a trait as such, can be said to be inherited or learned (Zuk, 2002, p. 50).

Genes are not puppet masters or blueprints. Nor are they just the carriers of heredity. They are active during life; they switch each other on and off; they respond to the environment. They may direct the construction of the body and brain in the womb, but then they set about dismantling and rebuilding what they have made almost at once – in response to experience. They are both cause and consequence of our actions (Ridley, 2004, p. 6).

... complex cultural traditions have been exerting coevolutionary selective pressures on human gene pools for about twenty thousand generations ... Genes, by themselves, can’t readily adapt to rapidly changing environments. Cultural variants, by themselves, can’t do anything without brains and bodies. Genes and culture are tightly coupled ... (Richerson & Boyd, 2006, p. 194).

And Wilson, again, some 21 years after the publication of On Human Nature:

How can anyone presume to speak of a gene that prescribes culture? The answer is that no serious scientist ever has ... All biologists speak of the interaction between heredity and environment. They do not, except in laboratory shorthand, speak of a gene “causing” a particular behavior, and they never mean it literally. That would make no more sense than its converse, the idea of behavior arising from culture without the intervention of brain activity (1999, pp. 148-149).

The brain is a product of the very highest levels of biological order, which are constrained by epigenetic rules implicit in the organism’s anatomy and physiology. Working in a chaotic flood of environmental stimuli, it sees and listens, learns, plans its own future. By that means the brain determines the fate of the genes that prescribed it. Across evolutionary time, the aggregate choices of many brains determine the Darwinian fate of everything human – the genes, the epigenetic rules, the communicating minds, and the culture (1999, p. 179).

... scenarios of the mind are all but infinite in detail, their content evolving in accordance with the unique history and physiology of the individual (1999, p. 131).

These quotations do not support the broad contention that (sociobiologists hold the view that) our genes ‘control’ every aspect of our behaviour and, therefore, determine our culture/s. There is nothing here that can be interpreted as implying a belief in any deterministic inevitability about the action of genes on individual behaviour or on human culture. Rather, they speak to an interaction between biology and culture that clearly indicates a belief in the mutability of biology. Far
from biologically deterministic, these statements speak to an understanding very much in tune with ELSI research, and with the position that is taken by sociobiology’s most vehement critics: that genes work within a sea of social experiences in multitudinous environments, the outcomes from which are unpredictable and indeterminate.

What, then, is it in Dawkins’ selfish gene theory that betrays a presumption of control? What is it that sociobiologists more generally say about gene/culture interaction that could lead one to think they are talking about genetic determinism and inevitability? How is it that, despite their many disavowals and clear statements to the contrary, they are read as contributing to the ‘myth’ of the super-deterministic gene? The answer is both much, and little – the devil, as they say, is in the detail. Leaving aside obvious misreadings and misinterpretations, and correcting for the sometimes unconscious effects of ideological difference (remembering that my purpose here is neither to accuse or defend), there remain sufficient instances of injudicious or inflammatory word choice, expression and metaphor, to arouse the critics’ concern and perhaps confuse the average intelligent reader:

Are human beings innately aggressive? This is a favorite question ... and one that raises emotion in political ideologues of all stripes. The answer to it is yes (Wilson, 1978, p. 99).

The genes hold culture on a leash. The leash is very long, but inevitably values will be constrained in accordance with their effects on the human gene pool (Wilson, 1978, p. 167, my emphasis).

With certain basic information concerning a species’ biology, it should accordingly be possible to predict behavior patterns based on the assumption that these patterns will be such as to maximize the inclusive fitness of the performer (Barash, 1982, p. 43).
Human behavior is flexible, of course, but this flexibility is of means, not ends, and the basic experiential goals that motivate human behavior are both inflexible and specific (Symons, 1995, p. 138).

The First Law [of behaviour genetics]: All human behavioral traits are heritable (Pinker, 2003, p. 373).

The spooky truth is dawning on scientists that they can regard behavior as just an extreme form of development. The nest of a bird is just as much a product of its genes as its wings are (Ridley, 2004, pp 139-140).

Let us try to teach generosity and altruism, because we are born selfish (Dawkins, 2006, p. 3).

Brains may be regarded as analogous in function to computers (Dawkins, 2006, p. 49).

By dictating the way survival machines and their nervous systems are built, genes exert ultimate power over behaviour (Dawkins, 2006, p. 60).

That critics focus on statements of this kind is unsurprising, given the kinds of conclusions that can so easily be drawn from them: our personal values are ultimately controlled by our genes; aggressive and selfish behaviour (or charitable behaviour or, for that matter, the ability to send text messages) is inherited through the genes; individuals are robots and computers, ‘programmed’ and dictated to by our genes; cultures (and therefore social roles) are a predictable and direct outcome of our genetic makeup. I would want to insist that critics and popularisers must take responsibility and ‘listen’ more carefully to what is actually being said. At the same time, those writing in sociobiology must also bear some responsibility for the presentation of these concepts to the broader audience – that is, to those who are introduced to sociobiological ideas through popular media, or via reviews and critiques, rather than through the texts themselves – because it is just these kinds of expressions...
and metaphors, rather than the careful and detailed explanations that surround them, that are most often picked up, in the critical literature and in the wider social arena, as ‘sound bites’ representing sociobiological thinking.\textsuperscript{149} And, of course, sociobiologists do mean to say all of these things, but within their specific framework of genes as the mechanisms of experience, and not necessarily in the ‘genes as blueprint’ ways in which they become translated into popular culture.

It is within this notion of genes as mechanisms of experience that we find the answer to the questions posed earlier; here is where we can understand what Pinker means when he directly links gene effects with individual traits and outcomes, and here too is where we can reconcile Ridley’s linear progression from gene to development to behaviour with Wilson’s statement that heritabilities are risky predictors of personal capacity. Each of them is speaking within the context of the universal behaviours contained within the social biogram, and each is placing the biogram within the framework of gene-culture interaction:

\begin{quote}
As psychologist Charles Lumsden and evolutionary biologist E.O. Wilson put it, genes have culture on a leash. Culture can wander a bit, but if it threatens to get out of hand, its genetic master can bring it to heel … Culture is on a leash, all right, but the dog on the end is big, smart, and independent. On any given walk, it is hard to tell who is leading who (Richerson & Boyd, 2006, p. 194).
\end{quote}

Despite this contextualisation, it is here, also, that we find the crux of the real problem of sociobiology for its critics, because these universal behaviours can all carry inferences far beyond the nature of evolution or of the entities called ‘selfish’ genes and into issues of personal autonomy and cultural flexibility. Thus

\textsuperscript{149} Dawkins’ ‘robot’ metaphor is a case in point, and one which he has said he would have explained more clearly if he had been thinking in terms of the common understanding of robots as ‘inflexible idiots’ at the time of writing (1999, original publication 1982, p. 17).
we are brought back full circle to arguments about the primacy of biology over

culture, keeping alive the spectre of genetic determinism.

**Mechanisms of experience – human universals**

The 1970s do seem to have been, as Dawkins puts it, “one of those mysterious

periods in which new ideas are hovering in the air” (2006, p. xvii). Although

recognised as the ‘founder’ of classic sociobiology, E.O. Wilson was neither the

first nor the only scientist working in this general area of inquiry in the 1970s,

and applying its principles to humans: John Maynard Smith and George Price’s

concept of the evolutionarily stable strategy (Maynard Smith, 1972; Maynard

Smith & Price, 1973); Robin Fox’s and Irven DeVore’s work on the evolution of

the capacity for culture and human society (DeVore, 1971; Fox, 1971); W.D.

Hamilton’s on altruism and inclusive fitness (1972), and Robert Trivers’ seminal

papers on reciprocal altruism (1971) and parental investment (1972), all pre-date

Wilson’s *Sociobiology* (1975). Even Dawkins’ *The selfish gene* was begun in 1972

and was completed in 1975, although it was not published until the following

year.\(^{150}\) All of these works, in turn, built upon a considerable body of earlier

research on evolution, in particular that of those most closely associated with the

modern synthesis\(^{151}\) (see Bowler, c1984; Dobzhansky, 1937; Fisher, 1930; Haldane,

1932; Huxley, 1942; Mayr, 1942; Wright, 1931), and on work carried out in the

1960s by George Williams and W.D. Hamilton (Hamilton, 1964; Williams, 1996,

original publication 1966).

\(^{150}\) For a more complete discussion of individual contributions to the ‘birth’ of sociobiology, see

Segerstråle (2000), chapter 5.

\(^{151}\) The modern synthesis (also called the new synthesis, the evolutionary synthesis, or neo-

Darwinism), is the unification of Mendelian and population genetics with evolutionary theory (see

Mayr & Provine, 1980).
As suggested by Ruth Bleier, however, Wilsonian sociobiology is the logical place to start when we begin to look at human universal behaviours, because his inclusion of humanity in his sociobiological inquiry sparked the controversy that continues today:

The purpose of sociobiology … is to develop general laws of the evolution and biology of social behavior, which might then be extended in a disinterested manner to the study of human beings … we expect to extend such general principles of sociobiology as can be devised to assist in the explanation of human behavior … Homo sapiens shares with other social mammals a tendency toward male dominance systems, a sexual division of labor, prolonged maternal care, and an extended socialization of the young based in good part on social play (Wilson, Foreword, in Barash, 1977, p. xiv).

Wilson’s *Sociobiology* gave the field its name and drew together many of the earlier theories under its banner. Despite these antecedents – and the fact that Wilson’s text was largely concerned with the social behaviours of insects – it is the connection between biology and social behaviour, and his explicit extension of sociobiological theory to humans in the first and last chapters of the book, that generated furious debate from the time of publication. The final chapter of *Sociobiology*, in particular, is controversial. Here, Wilson talks about the so-called human universals upon which he and others build their case for a human social biogram.

Wilson first notes that the evolutionary analysis of human behaviour is difficult because the evolutionary history of a species such as ours is “hard to reconstruct” and therefore “human qualities will be discussed *insofar as they appear to be* general traits of the species” (1975, p. 272, my emphasis). These general traits of the species include: the capacity for culture; barter (including, historically, the
use of women as objects of barter); reciprocal altruism; kinship ties; indoctrinability; aggression, and territorialism. Also included, and most controversially in a feminist context, is the predisposition to perform particular social roles as an outcome of the differing investments in reproduction by women and men. Wilson further theorises a genetic basis for homosexuality, a contentious idea revived during the 1990s by the high-profile research of Simon LeVay, and Dean Hamer and colleagues.152

Each of these hypotheses is supported by inferred hunter/gatherer practices, although Wilson concludes that to infer the presence of certain traits in early humans from their occurrence in living hunter-gatherer societies is unreliable. In this regard, he later commented that:

Many of the peculiar details of human sexual behavior and domestic life flow easily from this basic division of labor [hunting/gathering]. But such details are not essential to the autocatalysis model. They are appended to the evolutionary story only because they are displayed by virtually all hunter-gatherer societies (1978, p. 87).

Thus, ‘unreliable’ traits include: family as the nuclear unit, women gathering while men hunt,153 near-universal long term sexual bonding, and universal exogamy. Conversely, reliable traits include males being dominant over females; game playing; prolonged maternal care; and pronounced socialisation of the young (1975, p. 293). Wilson further proposes that human populations, societies and individuals are extremely variable and flexible and hypothesises that genes

---

152 Discussed in the following chapter (see Hamer, Hu, Magnuson, Hu, & Pattatucci, 1993; Hu et al., 1994; LeVay, 1991; LeVay & Hamer, 1994).

153 Wilson noted in 1975 that “there is no compelling reason to conclude that men did the hunting while women stayed at home” (1975, p. 292). It is important to note the body of anthropological evidence that it was the gatherers in any event who were of most importance to sustaining the family/group. For an overview see Burch & Ellanna (1996).
for such flexibility are “strongly selected at the individual level” (1975, p. 273).

He advises:

> It is vital not to misconstrue the political implications of such generalizations. To devise a naturalistic description of human social behavior is to note a set of facts for further investigation, not to pass a value judgment or to deny that a great deal of the behavior can be deliberately changed if individual societies so wish (Foreword, in Barash, 1977, p. xiv).

The difficulty, of course, is that such generalisations are profoundly political within a social milieu that has historically used exactly these generalisations to subordinate and demean women, their capabilities and capacities. So, despite the qualifying phrases and the emphasis on flexibility, these claims are, of course, deeply problematic for feminists and others, who not unreasonably take the view that the status quo is at least being defended, if not actively endorsed.

John Tooby and Leda Cosmides, recognised co-founders of the evolutionary psychology strand of sociobiology, and surely with an awareness of the history of negative responses to sociobiological thought, take a different approach. Where Wilson’s list of human universals is focused on knitting together ultimate causes and proximate behaviours into a causal relationship from our genes to our everyday behaviours, Tooby & Cosmides recast his specific universals into much broader behaviours:

> Anthropological orthodoxy to the contrary, human life is full of structure that recurs from culture to culture ... adults have children; humans have a species-typical body form; humans have characteristic emotions; humans move through a life history cued by observable body changes; humans come in two sexes; they eat food and are motivated to seek it when they lack it; humans are born and eventually die; they are related through sexual reproduction and through chains of descent; they turn their eyes toward objects and events that tend to be informative about adaptively consequential issues; they often compete, contend, or fight over limited social or subsistence (sic) resources; they express fear and
avoidance of dangers; they preferentially associate with mates, children, and other kin; they create and maintain enduring, mutually beneficial individuated relationships with nonrelatives; they speak; they create and participate in coalitions; they desire, plan, deceive, love, gaze, envy, get ill, have sex, play, can be injured, are satiated; and on and on (1995, pp. 88-89).

This list is both more general, and much more precise, careful, and non-controversial, than is Wilson’s. Tooby & Cosmides simply state commonsense understandings of the universal range of human behaviour and interaction that most of us will recognise as reflecting, in large part, the course of our own lives and the lives of those around us. So, while we may find Wilson’s list unpalatable, these more general (although still universal) attributes, are not so difficult to agree to. Yet, even Tooby & Cosmides then move toward more problematic specifics:

... humans have psychological adaptations that contain contentful structure specifically “about” their mothers, “about” their children, “about” the sexual behavior of their mates, “about” those identified by cues as kin, “about” how much to care for a sick child, and so on, and these contents are not derived exclusively from either a short list of drives or from culturally variable, socially learned “values” (1995, pp. 99-100).

What is interesting about this list of ‘abouts’ is not so much what it contains, but what it does not. Why contentful structure about these particular things – mothers, children, sexual behaviour, kin? There is a slippery slope here that returns us, again, to the politics of reproduction. This is what opens up suspicions about such theories – why these things? Why not other things? Why do the genes, as mechanisms of experience and as devices for extracting information from the environment, appear, in all of these accounts, to be so overwhelmingly interested in providing biological structures that so easily relate to the proximate social structures built particularly around women’s role in reproduction?
However that may be, let us for the moment assume that each of Wilson’s universal traits (from which Tooby & Cosmides’ more general list of behaviours largely stem) exist as part of our evolved human psychology. The social biogram, no matter what it is believed to contain, has been described ‘objectively’ and is itself free of morality (though the effects of some of its contents can be construed as moral or immoral) and ideology.

Clearly, humans have a facility for language and for learning generally, a capacity for culture, and individual and social flexibility. It is difficult, for example, to see how a ‘blank slate’ model can account for the acculturation into learning of individuals not already possessing the capacity to learn; but really, why should it matter if these qualities are ‘inborn’ or acquired later? Similarly, societies, and the individuals of which they are composed, are territorial, sometimes aggressive, and tied to each other through kinship. Humans do universally perform reciprocally altruistic acts (you scratch my back and I’ll scratch yours), and routinely engage in barter. And, while it may be a worrying concept for some because of its pejorative connotations, there can be no real challenge to the notion of our receptiveness to indoctrination – it is, after all, just another aspect of learning: we are indoctrinated into our cultures, our family rituals, our religions, our academic disciplines, our sporting codes, and on and on. But we are equally capable, once more through our capacity to learn, of breaking free of indoctrination and choosing another path. So, again, if these traits are part of a human social biogram that can be either consolidated or ameliorated by culture – that is, by the sociogram, and by its moral, ethical and
ideological values – why do we argue so strenuously about how they came to be?

Theorising these human qualities as presocial, within a context of gene-culture interaction and the mutability of biology, as sociobiologists do, could be relatively unproblematic. So why isn’t it?

Part of the problem, I suggest, lies in the very notion of the social biogram, as Steven Pinker observes:

> When it comes to explaining human thought and behavior, the possibility that heredity plays any role at all still has the power to shock ... Any claim that the mind has an innate organization strikes people not as a hypothesis that might be incorrect but as a thought it is immoral to think (2003, p. viii).

If it is immoral to think we may have a social biogram, then we are by definition imposing morality upon the biogram itself, not just upon its potential (or actual) effects. Further, the perception of the biogram as prior to, and therefore inevitably dominant over social influences, challenges opposing ideologies and leads to a belief that any acceptance of its existence sanctions genetic determinist thinking.

More directly, it is also because there is a perception, as we have already seen, that those who theorise about the contents of the biogram are simply not being careful enough, or even disinterested enough, in the assertions about evolved (biologically based) behaviours they extrapolate from proximate (human and non-human) social behaviours. The implied causal relationship from our genes to our everyday behaviours – without ever being able to explicate the link that allows this to happen, since no-one has yet been able to demonstrate how exactly DNA to RNA to protein to behaviour actually works\(^{154}\) – is profoundly unsettling.

\(^{154}\) Although note the brief discussion in Chapter 2 about the ways in which microarray analysis might begin to allow scientists to do so. In this regard, see also Parens (2004, pp. S19-S21).
These accounts step over an unbridged and largely unacknowledged gap between genes and culture. That is, accepting selfish gene theory does not, in itself, provide the link between a gene’s need to replicate itself and how that need translates into behaviour in any kind of prescriptive way.

And finally, of course, there is the vexed question of the ways in which many of the social biogram’s assumed contents so conveniently ‘naturalise’ and thus prop up the status quo. While the fact that they may do so is not sufficient reason to dismiss the possibility that certain evolutionary traits exist, there are very good reasons to be cautious about many of the conclusions drawn from their theorised existence. In part, the fact that more sociobiologists do not publicly critique the status quo, or fail to strongly endorse the views of those who do so, has to raise some questions about their politics. Unlike the claim that the human social biogram can be consolidated by culture, the corresponding claim that it may be ameliorated by culture remains abstract. The evidence provided by sociobiologists all points one way, and often enthusiastically so. This suggests that indeed, the real object of the enterprise is not so much to explore the interplay of biology and culture, but to assert the importance of biology to culture. In this regard, some of the discussions and conclusions about sex differentiated drives and behaviours within evolutionary psychology draw a suspiciously long bow.

155 Although many, including Dawkins and E.O. Wilson, make passing critical references to it in their works; and it may also be that such comments are made, in forums where only the more controversial comments and claims are widely reported. The media must also take responsibility for the ways in which reductionist views are conveyed to the wider society.

156 But it also perhaps says something about the divide between the two ‘sides’ that they are so mutually suspicious that, even where their views coincide, they do not publicly support each other.
Mechanisms of difference – sex differentiated behaviour

Genes are not only mechanisms of experience, they are also, by extension, mechanisms of sexually differentiated behaviour. Both classic sociobiology and evolutionary psychology theorise human mating and child-rearing practices, and the social roles traditionally attached to them, as evolutionarily-based outcomes of our differential investments in and experience of reproductive process (Trivers, 1972). Parental investment theory extends across the biological and into the social. Women’s bodies ‘invest’ monthly in a single large gamete which, once fertilised, can result in a further investment of nine months gestation, an intense and potentially risky birth process, followed by an extended period of lactation and care for a dependant infant. Men, on the other hand, produce sperm on a continuing basis, and can in theory inseminate numerous women with no further involvement in the process of gestation or post-natal child care.

As a result of this greater investment of physical resources in the biology of reproduction, sociobiology theorises women will, in general, behave socially in particular ways: they will be sexually coy (less promiscuous than men); choosier in their choice of partner (looking for a man with resources who will stay around to help them provide for offspring); be tied more closely to home and children, and be less likely to engage in risky occupations (gatherers, rather than hunters). Women whose genes predispose them to engage in these behaviours would be more likely to successfully raise their infants to adulthood and, thus, be more successful in transmitting their genes into the next generation. What is so interesting here is that, in line with the silence around male biology, choice, and reproductive and child care behaviours within the modern context (as we saw in
the Summers’ controversy), very little is said in the evolutionary context about the men who, through engaging in risky occupations and dying young, or in not caring for offspring who also then die young, risk failing to transmit their genes.

Leaving that aside, these behaviours, of course, map directly onto the status quo, providing both explanation for and justification of sexual inequality in all its various guises across cultures. Evolutionary psychologists attempt to make this conclusion less threatening and more palatable in both familiar and novel ways. Like the classic sociobiologists, some evolutionary psychologists also emphasise the mutability of genetic predispositions to sexually differentiated behaviours through interaction with culture:

Unfortunately, many laypersons and members of the scientific community incorrectly assume that human sex differences that have biological origins are unchangeable. The belief that these sex differences are immutable appears to create a psychological resistance to even a thoughtful consideration of biological influences … (Geary, 1998, p. 330).

Others stress the need to avoid the naturalistic fallacy in drawing conclusions from actual or theorised differences (that is, that is does not imply ought) (for example, Buss, 1994, p. 16).

It is quite true that describing what is must not be confused with a prescription for what should be (as, I shall argue in the next chapter, vulgar reductionists too often do). It is also true that, read carefully, much of sociobiological theory cannot be said to subscribe to the notion of an unchangeable biology. Nevertheless, the presentation of current social arrangements as what is, when placed within the context of the certainty within which these kinds of sexed
behaviours are presented as outcomes of our evolutionary history, does tend to lend colour to the conclusion that, while not fixed, they are, at the very least, deeply embedded in our biology and difficult to shift.

Finally, some evolutionary psychologists have a more novel way of sweetening their conclusions by, for instance, presenting the sexually differentiated behaviours as favourable to women (for example, see Fisher, 1999 below). In relation to this manoeuvre, classic sociobiologist Sarah Blaffer Hrdy says, “Evolutionary psychologists studying mate preferences today throw the occasional sop to women lest they mind being told they should look like Barbie dolls or else despair of becoming successful breeders” (1999, p. 24).

The real problem with each of these strategies is that they leave unconsidered the truth and/or adequacy of claims derived from theories about evolved human sex differences. That is, such strategies fail to dispel, or inevitably slip into, blatant justifications of status quo differences. So, for example, anthropologist Helen Fisher claims, with certainty and without qualification, that women are better than men at relationship-building (1999, p. 101); women are more passive in the bedroom because of a “primal fantasy of surrender” (1999, p. 200); that “men are often attracted to women who show signs of youth and beauty, while women tend to gravitate to men with status and resources” (1999, p. 236), and that “[m]en tend to get more upset when a wife has engaged in a one-night stand” whereas “[w]omen become more deeply troubled if a husband has engaged in a long, intimate affair” (1999, p. 268). Fisher writes as though these differences are
universal, undisputed, and are either neutral or even favourable in their social effects for women.

Similarly, in *The Evolution of Desire*, prominent evolutionary psychologist David Buss promises to explain the “baffling contradictions” in mating behaviour by gazing “back into our evolutionary past – a past that has grooved and scored our minds as much as our bodies, our strategies for mating as much as our strategies for survival” (1994, p. 2). In this and earlier work, Buss makes claims for the influence of evolutionary pressures on proximate sex-differentiated mating strategies based on data drawn from a questionnaire administered to 10,047 people in 37 cultures “drawn from 33 countries located on six continents and five islands” (1989, p. 1), and on more than 50 subsequent studies (1994, p. 4). In general, the data support the predictions that women will value earning capacity, ambition and industriousness more than will men, and that men will value youth, physical attractiveness and chastity more than will women.

So, for example, we are told that, in Buss’s study, men were found to prefer younger wives – around two-and-a-half years younger, on average. The older men get, the younger they prefer their women to be:

In short, contemporary men prefer young women because they have inherited from their male ancestors a preference that focused intently upon this cue to a woman’s reproductive value (1994, p. 52).

As further ‘proof’ of this preference, Buss cites the historical evidence of kings and emperors ‘securing’ younger brides, and of sultans ‘stocking’ their harems with “young, attractive, nubile” women. As contemporary evidence, he notes
that ‘aging’ rock and movie stars “frequently select women two or three decades younger” (1994, p. 63).

Other health cues used to evaluate a potential mate – clear eyes, symmetry, waist/hip ratio, blonde hair – are all applied to women since, according to the survey, women are less interested in physical attractiveness, having instead an “evolved preference for a man with resources” (1994, p. 47). The rise in the numbers of high earning women has apparently not affected this preference, and this fact is provided as proof that it is an evolved preference. Further, by extension, in choosing men with resources and high status, women are complicit in the spread and dominance of patriarchy:

Men strive to control resources and to exclude other men from resources to fulfill (sic) women’s mating preferences … Men’s larger bodies and more powerful status drives are due, at least in part, to the preferences that women have expressed over the past few million years (1994, p. 47; see also pp 212-214).

Furthermore, Buss asserts that as women age and their reproductive value declines, men become less interested in ‘guarding’ their mates, less interested in sex, and more ‘distressed’ by the demands made upon them for time and attention (1994, p. 206). Consequently, more women engage in extramarital affairs as they age, because women “seem to know that their desirability on the mating market will be higher if they leave their husbands sooner rather than later” (1994, p. 207). Once they are no longer capable of reproducing, however, women “shift their effort toward parenting and grandparenting” (1994, p. 207).

Leaving aside the problems inherent in extending results from a self-report questionnaire on proximate behaviours to evolutionarily derived drives, there
are glaring flaws in the logic of these stories. The finding that contemporary men
*say* they prefer to partner with physically attractive women who are younger
than themselves in no way proves the contention that they (unconsciously) do so
because these are cues to reproductive potential. It may be so, of course, but it
may also be that it is both expected and socially acceptable, across cultures, for
men to express a desire for young, attractive, symmetrical women and that this
knowledge has biased their responses in the questionnaire. A diminution of
‘mate guarding’ by men as relationships lengthen and their partners age is just as
likely related to an increase in understanding of and trust in each other as it is to
lowered reproductive value. The further finding that homosexual men want the
same standard of youth and beauty in their partners – indeed, having
“indistinguishable mating preferences” from heterosexual men (1994, p. 63) – is
not satisfactorily explained, and must cast some doubt on the adequacy of Buss’s
conclusions in this regard.

Similarly, it is unquestionably usually more beneficial for a woman to choose a
man with resources with whom to raise her children; thus, it is not unexpected
that (even high-earning) women responding to Buss’s survey would say they
more highly value earning capacity and ambition than would male respondents.
Perhaps even more to the point, what men and women like cannot be determined
outside of cultural expectations. That women ‘like’ what men like about
themselves (the resources they achieve) and do not express unacceptable
judgments – that, for example, ugly men and old men are worthless men – can
explain the consistency of responses between high and low earning women.
Again, notwithstanding the broad cross-cultural nature of the survey, this outcome merely confirms what we would expect as a result of social expectations and arrangements without recourse to evolutionary explanations. Women, after all, still control only a very small proportion of the world’s wealth.

The existence of harems is also not a well considered argument in support of the notion of women preferentially choosing high status well-resourced men (although it does support the argument that men prefer young, attractive women) since, on the whole, men decided the fate of these women. In general, women were bartered or chosen for the harem, they did not themselves choose. Buss’s language is instructive in this case: he speaks of men who ‘select’ women (1994, p. 63) or ‘acquire’ wives (1994, p. 52). Finally, and anecdotaly of course, women know that women are not immune to male youth and beauty – they are simply more constrained by custom and culture from acting upon or even admitting to such an interest than are men of any age. This, also, may well have biased the results of Buss’s questionnaire.

In sum, the problem is not so much that the data support the predictions (although we may ask why these specific predictions?), but rather that in tying these proximate results from 37 cultures with such certainty to ultimate causes engendered in the environment of evolutionary adaptedness (EEA), Buss ignores a range of other available alternate readings. Notwithstanding that self-report of these behaviours so commonly across cultures does not in itself

---

157 According to evolutionary psychologists, the environment to which humans are adapted is the Pleistocene epoch, dated around 1.8 million years to 10,000 years ago.
sufficiently establish their genetic bona fides, in this work (and other works of this kind), E.O. Wilson’s ‘modest genetic differences’ become transmuted into sharply delineated, genetically-driven, evolutionarily-embedded, sex differentiated social behaviours.\textsuperscript{158} Any notion that ‘cultural sanctions and training’ in ‘virtually all existing environments’ significantly intervene in the psychosocial development of the sexes is elided. The possibility that Wilson proffers, of genetic differences being completely cancelled by social training, disappears entirely from this story: the data are read exclusively as evidence of \textit{biological} conformity, not of \textit{social} or even of \textit{bio/social} conformity.

Academic sociobiologists often stress the importance of clearly understanding the (often unspoken) distinction between the ultimate pressures that underpin their theorising, and the proximate human behaviours that we see in ourselves and in the wider contemporary society. However, works like these of Buss and Fisher blur this distinction so utterly that they provide fertile ground for the easy transplantation of academic sociobiology into the kind of popular or vulgar sociobiology that feminists so rightly react against, and to which I turn in the next chapter.

\textbf{Conclusion}

\ldots just as specialized, distinctively sexual anatomy exists below the neck, so the Darwinist expects it to exist above the neck (Symons, 1995, p. 143).

The reactionary and determinist vision of sex-differentiated human nature presented by sociobiology has been well-rehearsed in the critical literature. When

contrasted with feminism’s more liberal and constructionist views, it is unsurprising that engagement between the two is most often defensive, argumentative, and frequently hostile.

Still, most sociobiologists say they do not intend to endorse the status quo in their theorising – anyone who reads widely within the field will soon lose count of these disclaimers. While some sociobiological work is so unmistakably misogynistic or deeply infected by status quo thinking that any such disclaimer is rendered meaningless, my inclination is to believe that, for the most part, these are expressions of a genuine intent. I accept that the theories espoused by most sociobiologists are the outcome of a deeply held conviction that their methodologies have led them to logical conclusions that, more likely than not, accurately reflect the proximate effects of evolutionary pressures on human psychology.

What gets lost in the public understanding of sociobiological thought, however, is exactly these disclaimers and the positioning of claims as theories, as distinct from certainties, and for this sociobiologists must bear some responsibility. The task of explicitly owning the uncertainties that are evident in much academic sociobiology, or of foregrounding equally plausible alternative explanations for social phenomena, is often left undone. At the same time, there is a failure to defend the integrity of their work from determinist interpretation, which has the

---

159 Although at times careful explanation is attempted but not heeded; again, see, for example, Dawkins on Maynard Smith and the ‘gene for tying shoelaces’, and texts such as Dawkins’ A Devil’s Chaplain (2004).
effect of seeming to endorse it for the casual reader. Few academic sociobiologists publicly take issue with the kinds of popular representations of their work that are discussed in the next chapter, for example, and indeed, in some cases appear to approve and compound them. Nor, as a rule, do they enter public debate to critique or challenge the more extravagant claims of their own colleagues – although, in common with other academic disciplines, they do maintain a lively and at times self-critical dialogue amongst themselves, and they are quick to defend their theories from the public criticisms of outsiders.

These comments, and the cautionary notes I have sounded throughout this chapter notwithstanding, I do want to suggest that there are constructive aspects to sociobiology that show potential for cross-pollination with feminist discourses around determinism and difference. It seems to me that the social biogram is one such critical point where there is room for greater accord. Belief in the flexibility of the effects that genes have on our behavioural phenome – now supported within genetic research – is a constant feature of the sociobiological literature that too often gets lost in its less careful manifestations, and disappears entirely from vulgar reductionism. If the more careful explanations about gene/environment interaction, found in so much sociobiology, are foregrounded alongside the

160 I note that highly visible popularisers of science such as Dawkins and the late Stephen Jay Gould have stated that not engaging with proponents of intelligent design is a deliberate strategy (see the essay ‘Unfinished correspondence with a Darwinian heavyweight’ (Dawkins, 2004)). However, they make their position on intelligent design publicly very clear in other forums. I can find no evidence of a similar level of public disparagement of vulgar reductionism.

161 Compare, for example, the descriptions of women’s behaviour in the bedroom in the texts discussed in the next chapter, which predate those of Fisher above.

162 Again, not always – see Hrdy’s critique of evolutionary psychology, previously mentioned.

163 The long-running public debate between evolutionist Gould and sociobiologist Dawkins is a case in point, as well as the Dawkins/Midgley controversy previously discussed; and see Segerstråle (2000).
uncertainties and complexities scientists are continuing to discover in post-
genome research, then the charge of determinism cannot be sustained.

Why then continue to focus on arguing with evolutionary psychologists about
whether women are more careful about relationships (at all, or because of
evolution, or because of social environments), for example, or with sociobiology
as a whole about claims at this level? Assertions of this kind, it seems to me, are
not capable of being solved and they risk keeping the debate focused upon
unproductive ground. If they can’t be ‘solved’, at least they are capable of being
rendered trite and meaningless. All it would require is to shift the focus of the
conversation from contesting these more specific and status-quo supporting
claims, to challenging sociobiologists to be much clearer about what they mean
when they, on the one hand, speak of flexibility, and on the other appear to
endorse predetermination. What do evolutionarily ‘programmed’ mating
strategies mean when explicitly located within a science of flexibility and
interaction? Do they, in the end, mean anything at all? And where would that
leave the tacit endorsement of vulgar reductionist portrayals of sociobiological
thinking by academic sociobiologists?
Five
‘Lunch-chasers and nest-defenders’ : vulgar reductionism

[Darwinism] is rapidly emerging as the most powerful secular grand narrative available to fin de siècle westerners.

Deborah Cameron (1997, p. 81)

We have to make the advantages of this genetic revolution available for biomedical research and yet still fight what I think is the danger of a naïve biological determinism and the consequences that could have for society.

Eric Lander (1999, p. 14)

Introduction

The resurgence of academic interest in human genetics and sex difference research in recent times is shared with us all through a media deluge of information, interpretation and speculation that intrudes into our daily lives, and shapes our participation in the re-popularisation of genetic/biological determinist discourses. We have become accustomed to seeing newspaper or television headlines announcing that scientists have discovered ‘the gene for’ this disease, or ‘the gene for’ that complex human trait. Television documentaries are reappearing as books, books are discussed on radio and in newspaper columns, the Internet abounds with web pages and discussion groups, and academic journals across a broad range of disciplines carry articles that in some way touch on genetic, sex difference, or sociobiological research.
The texts conveying this research to the public generally fall into one of four categories: the scientifically credentialed ‘expert’ text (for example, Baron-Cohen, 2003; Buss, 1994; Dawkins, 2006; Dennett, 1995; Fisher, 1999; Gould, 1996; Hamer & Copeland, 2000; Hamer, 2004; LeVay, 1994; Pinker, 2003; Thornhill & Palmer, 2000); collections of commentary and critique, with contributors from a broad spectrum of academic disciplines and public interest groups (see Hindmarsh, Lawrence, & Norton, 1998; Hubbard & Wald, 1999; Kevles & Hood, 1993b; Lewontin, 2000a; Rose, Kamin, & Lewontin, 1984; Rose & Rose, 2000); speculative or ‘brave new world’ texts (see Broderick, 1999; Kolata, 1997; Robertson, 1996; Silver, 1999; Watson, 2000; Wilkie, 1993), and finally, ‘popular culture’ texts, including prescriptive ‘bestseller’ and ‘pop science’ texts (a selection of which are Argov, 2002; Duggan, 2004; Farrell, 2005, 1988; Frankel, 2004; Gray, 1992, 2008; Harvey, 2009; Legato, 2005; Moir & Jessel, 1998; Moir & Moir, 1998; Nelson & Galant, 2004; Pease & Pease, 1999, 2002, 2004; Rhodes & Goldner, 1993). In the first three categories are the kinds of texts discussed in previous chapters, some of which may reach non-academic audiences, often because of raised profiles incited by media interest or controversy. The fourth category are specifically targeted at the wider general public.

In this chapter I aim to demonstrate the dangers that follow when largely reductionist scientific theory is written into popular culture texts. What gets lost in translation, is the imprecision, the uncertainty, and the fluidity of the science that we see in genetic and (most) academic sociobiology and (some) sex

---

164 While the vast majority of texts in the first and second categories, at least, of course do not.
difference research. What is at risk, then, if practitioners of these sciences are not themselves much more proactive about policing how their views are disseminated into popular culture, is that the ‘science’ presented in these pop culture texts will become widely accepted as definitive.

To this end, I focus my examination of two texts belonging to this last category: Anne & Bill Moir’s *Why Men Don’t Iron: The Real Science of Gender Studies* (1998), and Allan & Barbara Pease’s *Why Men Don’t Listen and Women Can’t Read Maps: ‘How we’re different and what to do about it’* (1999). I choose these two texts out of the plethora of example texts available both because they are representative of the wider field (that is, texts that claim to ‘explain’ the sexes to each other), and because they are amongst the earliest of the subset of popular advice texts that rely upon and purport to translate the science emerging from genetic, sex difference and sociobiological research and package it for public consumption.

Both texts were bestsellers, attracting a great deal of media attention. In the case of the Moir book, for example, its popularity (or notoriety) was driven by its development out of a BBC Channel 4 documentary that was widely screened and repeatedly re-run over some years, before being further expanded into a panel format program.

---

165 The book has since been retitled and reissued as ‘Why men don’t iron: the fascinating and unalterable differences between men and women’ and as ‘Why men don’t iron: the new reality of gender differences’.

166 The Pease International website claims that their book has sold over 12 million copies worldwide; figures for the Moir book were not available at the time of writing.
I am aware it may be thought this kind of text does not merit serious attention, either because the arguments are old ones that have all been met before, or even that recognition gives to their arguments a legitimacy they would not have otherwise. I think this is a wrong-headed assumption. Certainly, on one level, the arguments and images are old, and could be easily dismissed. However, they have an extraordinarily long shelf life, and while there is as yet no substantive proof of many of their assertions, they can now claim authoritative scientific support for some psychological sex differences being biologically derived. Again, I suggest the danger for feminist theory (and society generally) lies not with this possibility in itself, but with the ways in which the insights of reductionist research are being disseminated and adopted into popular culture through texts such as these. While they may easily be rejected out-of-hand for lack of academic rigour and expertise, their appeal to ‘commonsense’ beliefs, their open antagonism to political correctness and to feminism, and their attempts at accessible (often humourous and simplistic) non-technical explanations of complex issues, engage a broad readership.

I will not examine the scientific evidence relied upon in the texts in great detail in this chapter, having dealt with it or similar evidence extensively in earlier chapters. However, it is important to realise at the outset that ‘science’ is represented by these authors in a very particular way. Both books favour a reverent view of science and of those who practise it. That is, they assume science

---

167 One of the authors of *Why Men Don’t Iron*, Anne Moir, has scientific expertise (a PhD in genetics), and has worked for some years in broadcasting, first with the BBC and then as European Editor for Canadian Broadcasting in Britain. Husband and co-author Bill Moir is also a television producer. Allan Pease has a background in sales and runs his own corporate training company of which his wife and co-author, Barbara Pease, is CEO. She also lectures on deportment and grooming.
is conducted by women and men who are in but, by virtue of their scientific objectivity, not of the societies from which they compile their data. They therefore present the evidence and knowledge emanating from the sciences as untainted by the beliefs, expectations and limitations of those scientists. Further, they suggest that such ‘hard science’ is not open to question (certainly not by social scientists), and that where it is not yet conclusive it soon will be. No allowance is made for the possibility that the science could be partial, discredited or disproved, nor for the fact that scientific objectivity, as has been convincingly argued, is as much a myth as Superman is fantasy. It is also noteworthy that no mention is made of the various specialisations within scientific research from which supporting data is taken and, therefore, the assumption is that all science is equally valid. Indeed, the impression is that ‘Science’ – as a single all-encompassing enterprise – is uniformly benign, altruistic and ultimately beneficial in all its manifestations.

The basic contention of both books is that:

... our minds are configured like a computer at around 6-8 weeks after conception ... so that when we are born we come pre-packaged ... Science also shows that the basic operating system and its wiring leave little room for change ... Nature versus nurture? It's a done deal. Nature had the headstart from the very beginning (Pease & Pease, 1999, pp. 60-61).

A couple of main themes are developed from this contention: first, that the ‘essence’ of male/female and hetero/homosexual lies in brain differentiation determined by genes and/or prenatal hormones acting on the foetus, and, second,

---

168 To be fair to the scientists conducting the research upon which the assertions made in these texts rest, for the most part they do not themselves claim such objectivity or certainty and indeed, some plainly declare a personal stake in the outcome of their research.

their biology predisposes men to worldly success (and women’s does not). This hardwired pre-packaging manifests itself in ways that determine our sexuality, gendered behaviours, and future prospects. Clearly, these claims are problematic. In this chapter, I look at the deployment of language which functions to construct a ‘naturally’ justified edifice of male superiority, and question why these texts make the often outrageously simplistic, sometimes offensive, and now and then entirely laughable claims that they do.

The two texts I discuss here share a number of other features. Firstly, it is noteworthy that the authors of each of these texts are a couple, and this is explicitly used in the texts to neutralise the gendered implications of the arguments. Secondly, despite their surface differences, the texts are remarkably similar in the topics they choose to focus upon. Both begin by asserting sex ‘difference’ and then, although their sequential organisation of material differs, they draw on the science discussed in the preceding chapters of this thesis in the same selective way, and to achieve the same end. Both discuss the genetics that make men different from women (in a reversal of the more usual claim that women are different from men – but in common with other projects, here men are not only different from, but improved upon, women). Both use research searching for a ‘gay’ gene to establish a genetic basis for differences between homosexual and heterosexual men. Finally, both draw upon research into sex

---

170 The ‘couple-phenomenon’ also appears in scientific work - in sex difference research, for example, Bennett & Sally Shaywitz, Kermit & Katharine Hoyenga; in evolutionary psychology, Margo Wilson & Martin Daly, John Tooby & Leda Cosmides.
hormones, in particular to discuss (endorse) the effects of testosterone on so-called ‘masculine’ traits.

For the sake of clarity, I have grouped my discussion of the claims from science into sections that loosely equate to the book chapters, and to the three themes I identified above. I then draw all the sections together to show that these are profoundly conservative books which, while purportedly basing their arguments upon the ‘latest’ scientific research, really do little more than use broad generalisations and facile explanations to support the notions that ‘boys will be boys’ and that Western society is disintegrating because women are not content to accept their genetic destiny as nest builders.

“It’s a basic model!”

Just about the only thing [men and women] have in common is that they belong to the same species (Pease & Pease, 1999, p. 3).

The vast majority of us are born either genetically female or genetically male. That is, we are born with the reproductive and hormonal makeup of one of these two sexes. Boys have a penis and testes, and the hormonal configuration which will, at puberty, initiate sperm production and the development of male secondary sex characteristics such as a deepening of the voice and beard growth. Girls have a labia, vagina, and ovaries, and the hormonal configuration which

---

171 I am not suggesting that we discount or ignore intersex individuals, born with physical characteristics of both male and female; or individuals born with chromosomal and/or hormonal makeup that differs from the norm; but for the purposes of this argument I am confining myself to speaking of the majority (for discussions of the ‘five sexes’ see Fausto-Sterling, 1985, 1993, 2000b, 2000c).
will initiate menstruation and breast growth at puberty. So much cannot be disputed.

There is, however, scientific research that suggests the differing genetic and hormonal compositions of male and female do more than this. It suggests that the so-called sex hormones are also responsible, ab initio, for hardwired cognitive differences between the sexes. I have dealt with this research in another chapter, and will not repeat myself here, except to say that where it has merit, it is still insufficiently conclusive to support the claims derived from it in the texts under discussion in this chapter.

_Why Men Don’t Iron_ states in its introduction, “men are distinct: they are possessed of the differences that make for a real difference” (Moir & Moir, 1998, p. 12). Notice these are differences of which _men_ are possessed because, “the female is the basic model and he’s the customized version” (1998, p. 108). The authors of both _Why Men Don’t Iron_ and _Why Men Don’t Listen_ pay lip service to the politically correct view that, “Men and women are different. Not better or worse but different” (Pease & Pease, 1999, p.3, my emphasis). Nonetheless, they present scientific work in a way that endorses a view of the male sex as both more complex than the female (customised/basic), and more dynamic (active/passive). Yet their evidence is very selective, for it is neither definitive nor unchallenged by other research.173

---

172 See Chapter 3 on sex difference research.
173 Moir & Moir attempt to hedge their bets on this point by stating, on page 14, that it is “insulting to the reader to qualify everything to death … So when we write ‘Science finds such and such’ it plainly means that this is the best bet: no more, no less.” As I make clear throughout this chapter, it seems to me to be more insulting to the reader to present these so-called ‘best bets’ as conclusive of the kinds of inferences the authors draw from them.
Both books, for example, assert that the default destination of all human foetuses is female (Moir & Moir, 1998, p. 108; Pease & Pease, 1999, p. 195) and that the instructions carried on the ‘Y’ chromosome alter that trajectory and cause the foetus to instead become a male. Sex difference researcher Doreen Kimura puts it this way:

Most of the differences between males and females are secondary consequences of the presence or absence of the Y chromosome. The Y chromosome, in the normal course of events, determines that testes (male gonads) rather than ovaries (female gonads) will form; the testes, in turn, help determine most of the other differences between the sexes ... it appears that no special hormonal milieu is needed to yield a female ... We can summarize the process of sexual differentiation by saying that the “default” or “basic” form in mammals is a female ... (2000, pp. 17-18).

However, within the same passage, Kimura goes on to state that testes formation is the “critical first step in the production of the male”, but that the “precise location and nature” of the “testis-determining factor” is unknown. At best (at that time, in the late 1990s), the SRY gene on the Y chromosome was a “strong candidate” (2000, p. 17). We cannot, then, be finally convinced, on the evidence that these authors had available to them, that the Y chromosome is the sole source of determining factors for the development of testes. Indeed, in light of more recent research showing that almost half of the genes related to sperm production reside on the X chromosome,174 it would seem prudent to say, rather,

---
174 The finding was made by a team of researchers led by David Page of the Whitehead Institute for Biomedical Research in Massachusetts and Howard Hughes Medical Institute in Maryland (Wang, McCarrey, Yang, & Page, 2001). “Scientists and non-scientists alike are comfortable thinking about the Y chromosome as a specialist in male characteristics,’ Page said in a statement. ‘By default, we’ve traditionally thought of the X chromosome as sexually neutral or as a specialist in female characteristics. Our findings indicate that the X chromosome has a specialty in sperm production, much like the Y chromosome does.’... The researchers found 25 genes, including 19 new ones, that were expressed exclusively in mouse sperm stem cells. They found that of these, only 3 were linked to the Y chromosome and 10 were linked to the X chromosome. ‘This was a big surprise because if the genes had been distributed randomly in the genome, we would have expected none, or at the most a couple, of these sperm-specific genes to be X-linked,’ says Page.” (Whitehead Institute for Biomedical Research, 2001).
that the available evidence points to interactions between both sex chromosomes in the production of male characteristics. Further, it is reasonable to speculate that, if the X chromosome is actively engaged in the production of *male* sex characteristics, it may also be active in the formation of *female* sex characteristics. Simone de Beauvoir anticipates this idea in *The Second Sex*, when she says “we can assert that every biological fact implies transcendence, that every function involves a project, something to be done” (1988, first published 1949, pp. 41-42).

In the case of the X chromosome, this appears to be so.\(^{175}\)

I am, of course, not disputing the fact that a Y chromosome must be present in order to engender a male foetus and, in that sense, that sperm determines the sex of the child. What *is* clear from the research is that the Y chromosome may not be the all-powerful motivating force described here; it seems clear that there are other, complicating factors involved. More importantly, development as a female has not been proved to be an inactive process, a result of the lack of a Y, as opposed to the (active) presence of two X chromosomes. As Anne Fausto-Sterling (2000c) comprehensively demonstrates, that question is far from being resolved.\(^{176}\) So what is going on here? Remembering that ‘men are distinct: they are possessed of the differences that make for a real difference’, I turn now to the arguments for innate sexual orientation.


\(^{176}\) See in particular, chapter 8, pp 195-232.
The ‘gay gene’

96-99% of men are heterosexual, and almost certainly exclusively heterosexual ... sexual preferences are overwhelmingly one way or the other (Moir & Moir, 1998, p. 36).

One thing is overwhelmingly clear from the literature on homosexuality: science has emphatically *not* proved that it is a genetically predetermined ‘condition’. However, it is no surprise to find that the authors of both texts state with some finality that the question of whether sexuality is a biologically fixed or an environmentally induced process has been answered, and biology wins.

In their first chapter, ‘He’s not part one, part another’, subtitled ‘the bisexual fallacy’, Anne & Bill Moir approach the question of homosexuality from three interrelated angles – homosexuality as an exclusive orientation; as genetically determined, and as induced by prenatal hormones. Firstly, they argue that heterosexuality and homosexuality, at least in the vast majority of men, are distinct categories of sexual orientation:

Very few men are bisexual. The vast majority of men are either gay or straight, and most men are straight ... There is no sliding scale, no continuum, no latent gayness and no universal bisexuality. There is no scary gay in the straight man’s closet (1998, pp. 36-37).

It is clearly important to the authors to establish that the overwhelming majority of men are exclusively heterosexual and they do this by quoting figures from numerous surveys conducted post-Kinsey, which variously report an upper figure of 10% down to a low of 2.3% (1998, pp. 35-36). And while they do
acknowledge the potential flaws in these surveys,\textsuperscript{177} they proceed to validate the low figure of 1-4\% of men as homosexual (including bisexual) by citing unreferenced work conducted by Kurt ‘Freud’.\textsuperscript{178} In these studies, the researcher is said to have used a machine which measured small changes in penile engorgement as men were shown “lubricious pictures of naked boys” and “luscious centrefold[s]”. The findings, according to the Moirs, “demonstrated almost beyond doubt that the vast majority of men were either exclusively heterosexual or exclusively homosexual” (1998, p. 37). Clearly, problems of unreliability and dishonesty would be minimised by this approach since, as the authors note, erections are hard to fake (although we might assume also hard to control). However, because the particular studies are not referenced, we have no direct way of determining whether there were other validation problems, such as self-selection or a statistically insignificant sample size.

In any event, a major problem with this argument is the large body of evidence that exists about the variety and mutability of same-sex desire and practise, throughout history and across cultures. Anne Fausto-Sterling (2000c, p. 18 ff), for example, surveys the research literature on this matter and also takes issue with the presentation of human sexuality as a gay/straight dichotomy, saying:

\begin{center}
There are many gradations in sexual orientation. What do you call men who have sex with their wives while fantasizing about men? Or guys who are mostly straight who pick up male prostitutes, or transsexuals, or serial bisexuals who may switch between exclusively gay and exclusively straight relationships? How
\end{center}

\textsuperscript{177} That is, that they used unreliable samples, or that respondents are dishonest about their sexual experiences.

\textsuperscript{178} Although the Moirs cite Kurt ‘Freud’ in both text and index, and do not reference the relevant studies, we can be reasonably certain they are referring to the work of Czech sexology researcher, Dr Kurt Freund, developer of the penile plethysmograph, a device to measure penile blood flow (see, for example, Freund, 1991; see also Furr, 1991).
do you count sexual behaviour that changes over time in different circumstances? (quoted in Nimmons, 1994, p. 68).

It is necessary for the Moirs to establish exclusivity of sexual orientation in order to argue that it is fixed in the womb, by a combination of genes and hormones. For this claim they rely in part upon the 1993 research led by Dean Hamer, which was reported in Science as the discovery of the ‘gay gene’. This gene is first acknowledged by the Moirs as controversial, but is then validated as fact:

Dan (sic) Hamer’s ‘gay’ gene might not cause gayness at all, but merely predispose its possessor to the real causes of homosexuality. If that is true, then for the gay gene to be expressed requires further biological action, and that seems most likely because, confusingly, some homosexuals do not possess the so-called ‘gay’ gene at all. Something else, either an unidentified gene or a biological process, made them gay (1998, p. 41).

There are a number of claims here: the ‘gay’ gene exists, although it is not always expressed; those homosexuals who don’t possess the particular gene identified by Hamer must have another one; or there is another ‘real’ cause, still a biological process, which ‘makes’ them gay.

While the Moirs attempt to cover all possibilities consistent with their argument, Allan & Barbara Pease offer no alternatives to the straightforward conclusion that:

Those carrying the ‘gay gene’ as it is now called, have a 40% to 70% chance of becoming gay ... the X928 (sic) region of the X chromosome ... has been determined to be the approximate location ... The likelihood of the gene

---

179 See Hamer and colleagues (Hamer, Hu, Magnuson, Hu, & Pattatucci, 1993). This research found that 33 pairs of gay brothers, out of a sample size of 40 pairs, carried an identical strand of DNA on the long arm of the X chromosome, Xq28.

180 Other researchers have been unable to replicate these studies. George Rice and a team from University of Western Ontario conducted a study similar in method to Hamer’s using 52 gay brothers, and "found no evidence of linkage of sexual orientation to Xq28" (Rice, Anderson, Risch, & Ebers, 1999). Further, an unpublished study from the University of Chicago, studying 54 pairs of gay brothers, also failed to find a statistically significant link with Xq28 (discussed in Sardar, 1999). Hamer himself has many times denied that there is “a single, all-powerful ‘gay gene’” (Gallagher, 1998, p. 32).

181 The region in question is Xq28.
becoming penetrant appears to be largely dependent on the presence of the testosterone hormone six to eight weeks after conception (1999, p. 204).

The hormonal environment within the womb is the other ‘real’ cause identified also by the Moirs, and this claim rests on Simon LeVay’s controversial work on brain physiology.

Specifically, LeVay conducted a study into a cell cluster known as the third interstitial nucleus of the anterior hypothalamus (INAH3). In the study, LeVay autopsied the brains of 19 homosexual men, six women, and 22 heterosexual men, and demonstrated that there was a measurable difference in the size of this cell cluster between straight and gay men and, further, that the cell clusters in gay men were in the same size range as those in the women (1991). 182

LeVay’s results immediately attracted criticism, primarily that all 19 homosexuals included in the study had died from AIDS, as had six of the 22 heterosexual men, and there was no non-AIDS gay group as a control (see Byne & Parsons, 1993). 183 The Moirs dismiss this criticism with the news that it ‘lost its force’ after LeVay investigated the brains of homosexuals who had not died of AIDS, and once again discovered that they presented a “typical female pattern” (1998, p. 45). They conclude that LeVay’s findings not only support their construction of gay men as far more like women than like heterosexual men, but also their contention that gay men are ‘born that way’. However, LeVay himself

182 Anne Fausto-Sterling also disputes that LeVay found a ‘demonstrable difference’, remarking instead that he had found a ‘distributional difference’ (in Nimmons, 1994, p. 68).
disagrees with this interpretation of his work. In an interview reported in 1994, he said:

I did not prove that homosexuality is genetic, or find a genetic cause for being gay. I didn’t show that gay men are ‘born that way,’ the most common mistake people make in interpreting my work ... Since I looked at adult brains we don’t know if the differences I found were there at birth or if they appeared later ... The differences I found could have developed after a person was born (in Nimmons, 1994, p. 66).

Both Why Men Don’t Iron and Why Men Don’t Listen present Hamer’s and LeVay’s findings as though they have conclusively proven the fact of genetic/hormonal causes of homosexuality, when in fact they have done no such thing. So we are left asking: why do they do this and, more importantly, who benefits from such a conclusion?

To answer these questions we need to examine the other conclusions the authors draw from this claim of a biological cause for homosexuality. When we do this, we find that it works on several levels. Firstly, while carefully disavowing any homophobia – by claiming homosexuality as ‘natural’, for instance – the authors are also able to assert that homophobia itself is ‘natural’ in heterosexual readers (”‘What I don’t understand,’ Anne asks, ‘is why you men are so rude about gays, calling them bloody shirt lifters...’ ‘We can call them much worse than that,’ Bill says.” (Moir & Moir, 1998, p. 33)). Thus, they play to the prejudices of the reader (if so inclined) and reassure them that they are ‘normal’ to think this way. Further, the Moirs advise homosexuals that “the best way to reduce society’s antipathy towards homosexuality is ... to practice more restraint”, thus shifting the burden for ‘straight’ homophobia to the gay community (1998, p. 33).
The Pease book goes further than this, recommending that, since it has been ‘proven’ that stress and illness suppress testosterone production, which in turn produces a hormonal environment in the womb which can result in a gay child, “if you intend to become pregnant, it’s a good idea to start thinking about having a break and to monitor your surroundings for undue stress” (1999, p. 209).¹⁸⁴ This both pathologises homosexuality, placing it firmly within the category of a congenital defect that all prospective parents should (and would) seek to avoid at all costs, and blames women for their failure to do so.

Next, the arguments reassure the heterosexual reader – both male and female – that there is no gay lurking in the (particularly male) subconscious. A heterosexual man is a *real* man; a homosexual man, somehow, is not. This leads inevitably to the ultimate conclusion that gay men are not really men, *but are actually more like women*. Gay men have “a female brain structure” (Moir & Moir, 1998, p. 45) and, “boys will be boys, but not always” (Pease & Pease, 1999, p. 193), the implication being that *some* boys (gay boys) will be *girls*.

In terms of the political effect of these books, however, the really important point is none of these things. Within a framework of difference, this careful exposition on the causes of and reactions to homosexuality is most important because it works to strengthen the argument that *real* men are *not* like women.

---

¹⁸⁴ A claim possibly based upon work such as that by Lee Ellis & M. Ashley Ames (1987), who theorise that maternal stress is a factor (but only one among many other possible factors), in homosexual orientation.
Testosterone rules

“... and it isn’t only his sexual orientation that is customized. Even a man’s level of aggression is fixed in the womb. The more testosterone a foetus gets, the more aggressive he’ll be as an adult” “A hot rod, eh?” (Moir & Moir, 1998, p. 108).

According to these texts, then, ‘real’ men, that is heterosexual men, aren’t like women; they are both more customised, and more dynamic. The so-called ‘male’ hormone, testosterone, is not only responsible for effecting (what might be called) the fundamental differences, but is also the primary ingredient for a cascade of secondary differences that have wide-ranging effects on our behaviours, choices, and possibilities. Men are said to have a stronger sex drive, to be both more aggressive and more competitive than women, and to score higher on tests of spatial and math aptitude. It is important to consider the significance attached to each of these claims by the authors, because of the implied personal and social consequences.

Both texts argue that men have a considerably higher sex drive and are more easily sexually aroused than women. I concern myself here only with the knock-on effects the authors identify as resulting from the fact that, “The sexual drive in both women and men is fuelled by testosterone and men have 1,000%...

185 See Robert Sopolsky’s The trouble with testosterone (1998), and also his ‘Testosterone rules’ (1997), the latter essay discovered some years after the title of this section of my chapter had been chosen.
186 Other hormones have been implicated as well, for example serotonin and dopamine levels in risk-taking behaviours; cortisol in competitive behaviours. However, cortisol – which “provokes anxiety and counsels caution” is said to be a disadvantage to competition and is tagged as “much more the women’s hormone” (Moir & Moir, 1998, pp. 172-173).
187 It is useful to note here that a primary source for many of the claims made in the Moir text on T effects acknowledges the cultural effects on difference and the change over time given new cultural circumstances: “In the past, it was assumed that women had a weaker drive, that women were more difficult to sexually arouse ... Some research finds women to be less sexually arousable than men, but only because of cultural expectancies and because women have had fewer sexual experiences ... Women are culturally expected to be less arousable ... Until the last three decades, before marriage, men were more likely to be more sexually experienced than were women. This has changed” (Hoyenga & Hoyenga, 1979, pp. 382-383).
more” (Moir & Moir, 1998, p. 221). There are three main elements: the links between high levels of testosterone and sexual aggression, sexual novelty, and pornography. Let us look at the last element first and recognise at the outset that pornography is a somewhat vexed subject, even amongst feminists. Not all women are opposed to pornography, and nor are all men drawn to it. That said, I suggest that most women, and many men, no matter what their stance is on pornography per se, would still be disturbed and insulted by the assertions made by these texts in regard to it.

The authors of Why Men Don’t Listen argue that men need their pornography at least partly because of their testosterone-induced superior spatial skills. That is, they need to look at visual depictions of curves, lines and shapes; looking at centrefolds satisfies their “biological urges” (Pease & Pease, 1999, p. 252). Their partners should not feel threatened by this, since “When he looks at a naked woman, he doesn’t wonder if she has a nice personality, can cook or play the piano... For him, it’s not much different to admiring a leg of ham” (1999, p. 241).

The Moir book, though, attributes men’s need for pornography to more than the satisfaction of an innate need for visual stimulation. These authors say that pornography is not, as feminists and others have argued (and, we might be excused for thinking, the Pease book has just rather convincingly demonstrated), an objectification of women, but rather a response to men’s unfulfilled need to be objectified as an object of lust by women:

He worships the woman who sees him as a sex object. It is the male dreamland in which he can have all he wants, without jealousy, effort, or risk. It is a fantasy (not a fancy) woman that is the object of his adoration. And that fantasy is
deliberately fed as much by real women as by their cyber-sisters (1998, pp. 232-233).

Real women, we are told, feed the fantasy by ‘leading him on’. They wear tight jeans, bikinis, revealing clothes, to provoke and titillate. Or alternatively, they veil themselves, wear the chador, which also “only encourages the male’s fantasy life. The intimate is hidden, prompting visions of the unclad houri in Paradise” (1998, p. 233). In short, then, men’s need for pornography is hormonally induced, and deliberately provoked by women; and, further, it is something for which men should bear no responsibility because it is, after all, not a degradation of women but is rather men seeking to enjoy “a fantasy of perfection” (1998, p. 239).

Moving on to the second element, sexual novelty, perhaps it is also the quest for a fantasy of perfection which leads men to “fool themselves into believing they have a harem of different women by dressing their partners in a range of sexy clothing and lingerie. It is, in effect, his version of putting a bag over her head to provide a variety of different appearances” (Pease & Pease, 1999, p. 235). Both texts state that men have an evolution-driven biological imperative to impregnate as many women as possible.188 Since this is unacceptable behaviour in today’s modern world, civilised men in monogamous relationships subvert this urge by pretending variety and creating novelty with their partners. On the other hand, women don’t put bags over their men’s heads because our biological imperative is not for variety, but for a male who will stay around long enough to help us rear our children. It is so important to us to have a man to fulfil this role,

188 Again, this is a much simplified version of this concept drawn from evolutionary psychology and confuses proximate with ultimate causes. See chapter 4.
the authors seem to be suggesting, that we are more than happy to model our ‘paper bags’ in the bedroom. After all, in “denying himself sex outside marriage he makes a biological sacrifice comparable to her having the children” (Moir & Moir, 1998, p. 230).

Finally, high testosterone levels (T levels) in the male are also said to be closely linked to levels of aggression, specifically sexual aggression. Curiously, despite this claim, neither text has much to say on the question of sexual aggression, other than to note that most sexual assaults are committed by males between the ages of 17-25 years, and that the incidence of them declines with age and lower T levels. Further, the Moir text suggests that while “[v]iolent rape is associated with higher T levels ... the non-violent rapist [that is, a rapist who does not beat or injure, other than to sexually and emotionally injure by the forced sex act itself] generally has an average level” (1998, p. 222).

More space is devoted to men’s higher levels of non-sexual aggression, which, it is said, are implicated in their greater propensity for risk-taking and competitiveness. In turn, this leads to their better positioning for worldly success. It certainly seems to be true that men and boys do participate in riskier, more dangerous sports and occupations, and that they are more aggressively competitive than girls and women. The texts variously cite drag racing (Pease & Pease, 1999, p. 137) and Formula One racing (Moir & Moir, 1998, p. 159), flying jumbo jets (Pease & Pease, 1999, p. 139) and F14 Tomcats (Moir & Moir, 1998, p. 189).

Though figures on marriage, divorce and single motherhood tell a somewhat different story. In saying this, the Moirs seem to lend some colour to Andrea Dworkin’s claim that all men are potential rapists (1988).
and share trading and futures trading (1998, pp. 158, 162), as risky occupations dominated by men.

In the area of competitive sport, they state that “males have a 10% advantage” over women in track and field events, “and nature will keep it that way” (1998, p. 165). Men run faster, play harder, and compete always to win. Women may compete to win, but will forego the podium, the big corporate deal, or the important conference, in order to be home for a daughter’s birthday party (1998, p. 167).

It is possibly true that the majority of men will continue to have a 10% advantage over the majority of women in track and field sports, but this will be purely as a function of their average height, weight, and muscle distribution advantage, not because of any hormonal differences which give them a biological will to win which women lack. Despite the arguments of these authors for the male’s aggressive ‘edge’, it is not true that females are not aggressive, and there is good evidence to suggest that, although the style of aggression may differ, women are just as aggressive as men.191

It is by far more likely that all of these differences, from drag racing to winning marathons, to not missing children’s birthday parties, are a response to socially accepted roles, and socially expected emotions. Men are expected to take risks, be tougher, run harder, and sacrifice family for work commitments; and as Larry

191 The question of male/female differences in aggression is discussed in chapter 3 of the thesis, but also see Kaj Bjorkqvist & Pirrko Niemela (1992) for a full discussion on this point, chapters 6 to 8 in particular.
Summers’ comments quoted in chapter 3 of this thesis made very clear, it is
expected that women will put family first, and their own ambitions and interests
second. Females are not expected, nor is it acceptable, for them to be aggressive.
However, as the authors of a seminal text on gender differences note, “If we
change our gender stereotypes, gender-related differences should also change”
(Hoyenga & Hoyenga, 1979, p. 3).

This brings us to the last of the claims attributed to testosterone – spatial and
mathematical aptitude. Moir & Moir state that boys, “are much more likely to
excel at higher mathematics, at physics and at economic studies, indeed at any
area where mathematical reasoning is needed”. Males have an “inbuilt
advantage”, and there are, at the top end of the scale, “thirteen mathematically
gifted males to every one female” (1998, p. 118). The increasing percentage of
female math teachers (and of females generally in math-related fields) is
explained by Pease & Pease in this way:

... women are more suited to teaching, interaction and organising groups, and
are more committed to learning the basics than men. Because they are teaching
the same material over and over, they can do an excellent job with teaching most
subjects, including maths. This would also explain why in the field of
accountancy there is a general increase in the number of female accountants

However, boys’ competitive and aggressive ‘natures’ are identified as
shortcomings in today’s ‘feminised’ schooling systems in the Western world,
where boys are placed at a disadvantage by the adoption of cooperative and non-
confrontational teaching methods which favour the naturally preferred learning
styles of girls.
There have indeed been changes to teaching and classroom management methods over the past three decades. Some of the changes, of course, have been aimed specifically at making the learning environment more girl-friendly; these, though, have often been seen as most necessary in those areas of math and science teaching in which the authors of these texts admit boys have historically been advantaged – although they claim the advantage is biologically-driven. Many of the changes in pedagogical practice have come about, not with a direct goal of making classrooms more or less girl- or boy-friendly, but in response to a recognition of just the kind of thinking that these authors espouse: children are now credited with individual needs, prior experiences, and talents. These changes are a rejection of what Paolo Freire labelled the ‘banking’ style of teaching and learning which has dominated our classrooms for most of the history of formal education (1993). Far from being an attempt to feminise the classroom, they are an attempt to cater to the needs and strengths of each child (and, of course, different systems, schools, and individual teachers do this more or less successfully, but that is another argument, and not relevant here).

Nature versus nurture

Men and women should be equal in terms of their rights to opportunities to exercise their full potential, but they are definitely not identical in their innate abilities (Pease & Pease, 1999, p. 7).

“Hard science”, say the Moirs, “demonstrates substantial biological differences between the sexes; differences that are not, and cannot be, culturally engendered” (Moir & Moir, 1998, p. 13). In support, both the texts being

192 See chapter 2.
discussed here stress the point that in three decades of ‘equality’ these “fundamental sex differences” have hardly changed. I would argue, rather, that it is clearly not biological sex differences but rather culturally engendered values and belief systems that these texts are all about defending.

Boys, these authors tell us, are disadvantaged at school and in society in general, because their natural tendencies toward competition and aggressiveness aren’t catered for in the ‘feminised’ school system, and furthermore, that today’s parents are too quick to medicate for “normal boyish behaviour” (1998, p. 141). The education system needs to be ‘masculinised’, and teachers need to re-learn the particular skills of teaching boys: lots of tests instead of continuous assessment, competition instead of cooperation, work that allows for a real sense of ‘action’ and for a much shorter attention span.

We are told that, because of their unique genetic and hormonal makeup males also have a “near monopoly of the untalented extreme” (1998, p. 179) and, since there are so many more males at the lower end of success (women have less geniuses, but also less abject failures), the ‘workplace revolution’ has been disastrous for men. This is a problem created, maintained and accelerated by women, because women will accept less congenial working conditions, make less trouble, and work for lower pay than men. In the professions, such as law, women are preferred because they are not as “obsessed as the male with pay and status” and work just as long hours and as productively as men but, rightly, are not paid as much as a male employee because, in the long run, he is “putting in
more effort ... she is much more likely to be doing unprofitable legal aid work because it makes her feel useful” (1998, p. 186).

In the boardroom, because of their differing biological equipment, “his chances of sitting on a major board are only one in 55,555. Hers are one in a million”. This is not a social barrier – “there are no real barriers to a woman’s success” – but simply a reflection that “women are not taking up the challenge because it does not suit their biology” (1998, pp. 191-192). Men are more competitive and aggressive and, in consequence, they succeed more often than do women in the corporate world, the political world, and the world of highly paid sport. Women, who don’t have the biological advantages vouchsafed to men, should accept that this is so, and leave the men to it.

On the other hand, we are told that boys and men don’t know what to expect or how to behave in today’s world because:

We move from a time of sharply drawn lines to a time where the line drawn is against the drawing of lines. Fifty years ago a man was expected to play the dominant role ... Today the expectation is of a sexual parity at home and at work ... Lines of demarcation, present for millennia, are being blurred (1998, p. 19).

And, according to the Pease book:

Things were simple: he was the lunch-chaser, she was the nest-defender ... Her self-worth came from the man’s appreciation of her home-making and nurturing skills (1999, p. 13).

And that’s the argument in a nutshell, despite the politically correct disclaimers sprinkled throughout both texts: women are much happier taking care of others, men are much happier when women are busy taking care of them (and keeping the children quiet), and society as a whole would be much better off if women
would only accept their biological limitations and go back to their kitchens, or nurseries, or volunteer caring work.

So in effect, the argument is that there are, and always have been, lines drawn between the sexes by biological forces which are as “ineradicable as the leopard’s spots”, which are natural, and are therefore not to be meddled with. Nature has so organised the sexes that there are “constant masculine values” (Moir & Moir, 1998, p. 121) which ensure that ‘real’ men will always hold the top jobs, make the most money, eat red meat, read maps the ‘right’ way up, largely ignore their children’s bodily needs, and never iron a shirt if there’s a woman around to do it for them.

It is a demonstrable nonsense to invoke a socially constructed and disseminated belief system such as ‘masculine values’ and claim it as a biological given, almost as nonsensical as claiming that, “When you learn a new skill, you pass it on genetically to your children” (Pease & Pease, 1999, p. 17). Even if we were to adopt a generous interpretation of what these authors may have meant here, it is a serious misinterpretation of the theory of memes, which broadly suggests a means for the transmission of social knowledge (see Dawkins, 2006, p. 192 ff).

In sum, all of these arguments clearly commit the naturalistic fallacy that because it is natural (whatever that may mean) it is also right. As I read them, the animating purpose of these works is not to interpret science for us but to use science to defend the status quo against the criticism of feminists in particular. Their message is that the way society has been constructed in the past is ‘natural’,
‘right’, because it suits our evolutionary development; and today’s feminised society, today’s feminists, are responsible for all the ills of the Western world consequent upon the overturning of this ‘natural’ order. The way it was when it benefited men was the way nature intended it to be. It is pure coincidence that men’s personal, financial, sexual and political interests all manage to coincide with their biological imperatives, while those of women allow for them to again be subsumed into personal compromise and domestic drudgery. In effect, in their arguments from difference, these texts do much more than avert necessary social change – they actively set out to reverse it.

Conclusion

Language is a powerful mediator of belief, and widely held beliefs influence social practice. We see the evidence of this on our televisions every day, in the preponderance of busty women (most men like big breasts) and blonde women (blondes have more fun) on our screens; in the thinness of women on the catwalks (fat is ugly); and in images of men as stronger, more protective, tougher than women (boys don’t cry). While none of these issues is trivial, we can see the more serious effects of the kind of thinking exemplified by these texts in, for example, the high incidence of eating disorders, such as anorexia and bulimia, primarily in young women in western countries; or when we think of certain sex-biased cultural preferences and practices in some non-western countries,

---

193 Or, in evolutionary psychology, blonde hair signals youth and fecundity.
194 Although clearly cultural emphasis on body image is not the only cause of eating disorders. One Victorian study found that nearly 9% of female adolescents suffered from an eating disorder (see http://www.eatingdisorders.org.au/media/key-statistics.html).
periodically revisited in the western media and to some extent now part of the western literature around damaging sexed practices and beliefs.

Each of the books discussed in this chapter, though perhaps the Moir book more explicitly, sets itself up as a corrective to address the supposed imbalance and inequity experienced by men because of the so-called feminisation of Western society. The pendulum of inequality, they assert, has been set swinging by feminism and has swung too far the other way; men and boys are now the disadvantaged sex. If this were true, and if the purpose of these authors was to achieve a state of true equality between the sexes, that would be a laudable aim – one with which many feminists could perhaps find common ground. But I believe that equality for women and men is not what they seek, and that despite the rhetoric, these books are not even-handed in their evaluation of the sexes. The balance and harmony they profess to seek is based upon an interpretation of an essential male and female difference, if there be any, which is profoundly disadvantageous for and contemptuous of women. The true subject of these books is ‘man’, their purpose to return him to his ‘rightful’ place, and their titles are only the first indication that these books play dirty – men don’t, women can’t. This is a very convenient position from which to ensure that men retain choice and agency, whilst women do not.

Texts of this kind feed off and feed into the desire for easy answers to difficult questions in an increasingly complicated world. They fit neatly into the ongoing
conservative backlash against feminism and the increasing interest in ‘masculinity’ studies, by presenting a popular oversimplification of the messages of the new genetics and sociobiological theory, effectively propping up so-called ‘commonsense’ assumptions about the essential ‘nature’ of femaleness and maleness, masculinity and femininity, in order to reinforce the status quo. The late Stephen Jay Gould was not speaking of these books when he said that theories are most successful when they let us believe that our “social prejudices are scientific facts after all” (paraphrased in Baxter, 1994), but he could well have been. Nature versus nurture may well be a ‘done deal’, but contrary to the arguments put forward in these and other texts like them – and for which some of the reductionist science discussed in earlier chapters of this thesis must bear some responsibility – we are not yet able to say with any degree of certainty what that deal might be.

I believe these books are dangerous in their use of broad generalisations and facile explanations of complex theoretical concepts, patronising in tone and patriarchal in intent, and offensive to both sexes. Their conservative and prescriptive character enhances the possibilities for a regressive realignment of opportunities and societal expectations based on perceptions about gendered abilities and behaviour. Given their penetration into popular culture and everyday belief systems, neither feminist theorists nor the scientists upon whose work many of these claims are based can afford to dismiss these books as mere

---

195 For a thorough examination of the backlash against feminism in Western cultures, see Susan Faludi (1992).
'pop culture'. Nor can feminists hope to overcome their broad appeal by taking issue with them on the basis of their oppressive effect upon women.

Both sides of the campus must come together to disrupt the effects of vulgar reductionism by holding it to account to fairly represent the uncertain and fluid nature of much of the research upon which it relies. It is perhaps wishful thinking that the media would also play their part by foregoing sensationalism in the interests of accuracy; but at the very least they must be encouraged to provide greater opportunities for scientists and others to inform, educate, debate, and to correct public misinterpretations of their findings. At the end of the day, the status of science being what it is, scientists themselves must take the lead in averting the threat to the position of women in society by paying sufficient attention to the wider social effects of their theories, and more vigorously policing and disputing the interpretations placed upon their ‘truth’ claims by whatever means possible. That they don’t do so, and do so vigorously, makes them vulnerable to/opens them up to the suspicions/recriminations discussed in previous chapters. For undeniably, where science is not careful or vigilant enough, vulgar reductionism is what we are left with.
Part three: “The organism is predecessor to our theories about it”

We have to be more careful not only with our own theorising but also with what we think to be true, since we can theorise without giving our theories the truth status.

Catherine Stimpson (quoted in Bacchi, 1990, p. 228)

Modern conceptions of scientific truths can be seen as lying on a continuum ranging from claims that “all human knowledge is relative” to the positivists’ claims that “science can produce solid truths about the real world if coupled with logic and reason” … both of these oppugnant positions are problematic. It is, however, a tricky task to locate an intellectually defensible position on the slippery continuum between these antipodes of “no truth” and “absolute truth”.

John D. Baldwin (2008, p. 1)
Because gender is premised on the expulsion of biology, and because gender has indeed become feminism’s “central explanatory and organizing category”, the possibility of thinking biology as other than an excluded, distant, and foundational ruse has been foreclosed in the majority of feminist projects. Feminist critiques of the stomach or hormonal structure are not merely unlikely in this critical environment; they have been rendered unthinkable … biology becomes the excluded, unthought, simple ground from which feminist analysis is fashioned.


Rejecting the biological explanations merely because they are biological is to give them more power than they deserve.

Marlene Zuk (2002, p. 190)

Introduction

Almost fifteen years ago, Patricia Gowaty organised an ‘Ev and Fem’ symposium, with the intention of facilitating a “new dialogue at the frontiers created by the interfaces of feminisms and evolutionary biology”. The role she found herself undertaking, instead, was “as interpreter and apologist for both at the contested, old boundaries between them” (1997b, p. 1). The preceding chapters in this thesis demonstrate that the old boundaries continue to exist. But so, albeit in a still limited and under-utilised way, do the frontiers and new

---

196 Evolutionary biology, in this context, encompassing both classic sociobiology and evolutionary psychology. The symposium, the Society for the Study of Evolution – University of Georgia State of the Art Symposium on Evolutionary Biology and Feminism, was held at the University of Georgia’s Institute of Ecology on 13-14 June, 1994.
dialogue for which Gowaty was hoping. This chapter is concerned with the interesting dialectic relationship between critique and construct in the highly political, and politicised, environment of mutual suspicion and misunderstanding within which we habitually debate the biology/culture split.

A number of feminists, writing from within their diverse disciplinary homes in psychology, philosophy, corporeal theory, and the natural sciences, have attempted to work within this inhospitable environment in an attempt to draw together feminist and reductionist approaches to (socio)biology. While all of these works share a common interest in bridging the gulf between these two endeavours, and in this sense are to be celebrated, they differ in approach and in what they are hoping to achieve. That is to say, there are distinctions to be made in the animating logic of these works and, as a result, they are more or less successful in advancing a feminist agenda.

The critical dialectic

The empirical research undertaken by the sociobiological feminists informs the critical reinterpretation of existing theory, acting to promote the inclusion of a female perspective within sociobiological models and reductionist discourse. Sarah Hrdy ascribes the particular insights of this research in part to feminism, but largely to the fact that she, and other women fieldworkers, were more likely than their male counterparts to pay attention to the ‘unexpected’ behaviour of the (non-human) females within the groups they were observing (1999, original
publication 1981, p. xviii).\textsuperscript{197} These ‘unexpected behaviours’, that had in the past been dismissed as merely atypical, form the centrepiece of much feminist sociobiological work, and the basis for extending their theories into the sphere of human female behaviour. Hrdy’s fieldwork with langur monkeys in the 1970s led her to theorise that the female promiscuity she observed was at least in part an evolved strategy to counter infanticide – that is, males who had mated with an individual female were less likely to kill offspring they had potentially sired – as well as to provide extra assistance through the creation of ‘alloparents’ (1977).\textsuperscript{198} Hrdy expands on this insight by observing that similar strategies have been found among women in a number of traditional societies, and uses it to sound a cautionary note for modern societies:

Forget the image of promiscuous women having “fun.” At stake is a serious endeavor: mothers making do under difficult circumstances. Mother-centered models force us to rethink long-held assumptions about the nuclear family. Not long ago, a Wall Street Journal editorial entitled “Feminism isn’t anti-sex: It’s only anti-family” complained that feminism and especially birth control are responsible for the contemporary breakdown of families in America, with special reference to what is going on in America’s inner cities. But given that polyandrous mothers probably predate by thousands of years that most modern of postindustrial luxuries known as “feminism,” we would do better to focus instead on demographic and economic realities constraining maternal choices: high rates of male mortality, imprisonment, and defection and job prospects that translate into poor “hunting” prospects, making it impractical for a mother to rely on one man (2001, p. 93).

In other work, she convincingly dispels the idea that dominance and competition are male-only traits, demonstrates that in many primate societies multiple sexual partners and same-sex sexual encounters are common (1999, original publication 1981), and takes on the ‘myth of the coy female’ (1986; see also Wayne, 2000).

\begin{itemize}
  \item \textsuperscript{197} ‘More likely’ but not exclusively - Meredith Small, for example, notes the insights and contributions of male sociobiologists – in particular John Maynard Smith and Robert Trivers – to theories of evolution driven by female-choice (1993, see chapter 4).
  \item \textsuperscript{198} ‘Allo’ from the Greek for ‘other’; any person other than a biological parent who acts as a caretaker of the young.
\end{itemize}
Returning to this latter theme again a decade later, Hrdy (1997) specifically locates the origins of patriarchy within male control of the resources required by females, as a mechanism for the restraint of female sexuality. Hypothesising from the premise that females have an evolved adaptive predisposition to mate with multiple partners, for reasons ranging from sexual pleasure, to genetic quality, to the creation of paternity confusion, Hrdy proposes that the problem for men then becomes one of how to restrain women from acting on this predisposition in order to ensure certainty of paternity (1997).

Most recently, Hrdy has continued her analysis of the selection pressures on mothers, arguing against any straightforward notion of a ‘maternal instinct’ or of maternal exclusivity in child care, but concluding, rather, that humans have evolved to be cooperative breeders (Hrdy, 1999, 2001, 2009):

> Whether living in the Pleistocene, in eighteenth-century Europe, or in the United States right now, mothers have sought help rearing their children from fathers, other males, female relatives, or anyone else they could rely upon. As a consequence of this dependency, the emergence of maternal commitment, always a complicated process in mammals, is unusually contingent on social circumstance (2001, p. 82).

Others have similarly expanded upon or contradicted conventional theories about ‘human’ nature, by paying greater attention to the selection traits operating upon the behaviours of non-human females. Barbara Smuts, for example, has built upon her work with baboons to elaborate on the effects of male aggression and coercion on female behaviour (1992; Smuts & Smuts, 1993) and, like Hrdy, has placed the evolutionary origins of patriarchy within men’s need to control women’s sexuality (Smuts, 1995). Meredith Small has also studied the sexual
behaviour, mating, and reproductive strategies of female primates (1993). Beginning from Darwin’s ‘misogynist’ theory of female mating strategies, Small reviews the research on primate mating strategies, and her own observations of the mating behaviours of Barbary macaque females, and extends her findings to theorise about these same behaviours in humans. She concludes that “selection for female sexual assertiveness, sexual pleasure, orgasms, and the clitoris as an organ of pleasure occurred parallel to, not as a by-product of, selection for male sexuality” (1993, p. 148). Moving away from primate studies, Patricia Gowaty has studied ‘apparent’ monogamy and breeding season aggression in female bluebirds (Gowaty, 1981a, 1981b, 1985; Gowaty & Wagner, 1988), the latter research apparently attracting considerable media attention. Marlene Zuk presumes this was because it confounded stereotypes: “Killer bluebirds are bad enough, but female killer bluebirds seemed to be the stuff of nightmares” (2002, p. 38, my emphasis).

The particular interests and individual foci of these and other feminist sociobiologists/evolutionary feminists have made a substantial contribution towards positioning sociobiology as a less male-biased discipline. Their work operates at the level of critique of malestream sociobiological theory to correct bias, but also argues with it in a positive way to actively amend and extend it. Whether we agree with their conclusions or not, their theories have undoubtedly made significant inroads into upsetting stale perceptions about biologically determined gender roles and restrictions. In the process, both feminism and
sociobiology have benefited from this important crossing and shifting of the boundaries between the two disciplines. As Sarah Hrdy notes:

By the last quarter of the twentieth century ethologists (scientists who study animals in their natural habitats) and sociobiologists (who seek to understand the biological basis for social behaviors and rely on comparison across species to help them do so) were moving away from moralistic projections toward systematic study of what animals in the natural world were doing. In particular, females were being studied as individuals, leading to a new awareness of the extent to which one female differed from another. A female of the same species, even the same individual at different times in her life, might behave very differently according to her circumstances … Unnoticed by social constructionists, there had been a sea change in how females were conceptualized by sociobiologists (2001, p. 65).

In short, although female agency is not completely overlooked by traditional (malestream) Darwinian and sociobiological theory, feminist-informed sociobiology has been able to propose and promote an evidence-based evolutionary view of females as active agents whose interests and desires have shaped – rather than merely being the passive recipients of – evolutionary history. Thus, the knitting together of a feminist agenda and sociobiological theories of biology has been most successfully accomplished within this work. Despite the doubts expressed by some of their feminist sisters about their situation within (and possible seduction by) the sociobiological model, its conventional views about female social and sexual behaviour have been revisited and revised over the last three decades in large part because of the insights of this relatively small group of researchers.

The work of the sociobiologist feminists speaks to us all. Unlike many of their feminist colleagues, though, they are not endeavouring to question the very existence of a rich and complex social biogram which is the fundamental
assumption of the field they share with their sociobiologist colleagues, who are their primary audience. Their work is still largely a story about the (re)interpretation of evidence and the acknowledgement and correction of bias and, in this sense, sits squarely within the realm of a critical dialectic.

Feminist sociobiologist Marlene Zuk is no exception, but in her work, she does something slightly different in explicitly setting out to examine and critique bias in scientific questions and answers, and anthropomorphic bias in our understandings of animal behaviour. Side by side with the scientific publications that emerge from her field and laboratory work (primarily with crickets and red jungle fowl), Zuk takes on debates within sociobiology around whether specific human functions and behaviours – in particular, homosexuality, female orgasm, spatial ability and menstruation - are by-products or adaptations (2002). Female orgasm, for example, is variously hypothesised, by sociobiologists and others, as an artefact or as an adaptation. As an artefact, it is merely consequential, and often understood from a male-centric bias. Zuk notes that Stephen Jay Gould, for example, proposes that the female orgasm “is a by-product of the clitoris developing from the same embryonic tissue as the penis” and, thus, “females have orgasms because males do” (Zuk, 2002, p. 142); and anthropologist Donald Symons agrees that, “orgasm may be possible for female mammals because it is adaptive for males” and, further, the “ability of females to

---

199 Or, in Stephen Jay Gould’s term, ‘spandrels’ – that is, something that occurs as a by-product of a selected trait, but which did not itself arise from natural selection. The term was borrowed from the architectural spandrel, generally the spaces between two arches. See Gould & Richard Lewontin’s classic paper, ‘The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme’ (1979).
experience multiple orgasms may be an incidental effect of their inability to
ejaculate” (quoted in Zuk, 2002, p. 144). As an adaptation, more positive
theorisations propose that it is a mechanism to predispose females to seek
multiple partners and copulations, as a device to upset paternity certainty, or for
pleasure (Hrdy, 1999, original publication 1981; Small, 1993), or as a device for
testing consideration in lovers as a measure of the likelihood that they are good
positively, other theorists, also acting on the assumption that female orgasm is
adaptive, variously hypothesise that this is so because it promotes conception –
either because of the ‘poleax’ effect which keeps females lying down postcoitally
(a distinctly silly notion when placed in the wider context of sex in other species,
let alone the universalising assumptions it makes about human females), or via
the ‘upsuck’ effect, where orgasm assists with the movement of semen up the
reproductive tract. Still others hypothesise that female orgasm is more likely to
occur with more symmetrical males who, in turn, are assumed to be more
genetically fit (Zuk, 2002, pp. 146-148). With the exception of the adaptation
scenario proposed by Hrdy, what is notable about each of these suggestions,
according to Zuk, is “the focus on Where’s the Penis. What females experience is

In fact, what Zuk makes very clear is that the majority of these premises, whether
they support artefact or adaptation, account for a female’s ability to experience
orgasm by reference to male sexual behaviour. In reviewing the artefact position,
she asks:
Why is it less “efficient” for women to have orgasms before, after, and not always during intercourse? Why do they have to have one (or more) every single time a man does? ... A problem with viewing female orgasm as a trait that arose only through selection on males is the same one I discussed earlier, that our biases sometimes cause us to assume that males are normal, and females are variants. What is so perfectly efficient about male orgasm, after all? (2002, pp. 144-145).

And, in concluding that human female orgasm “is at least as much of an adaptation as male orgasm” she writes:

The point is that it is foolish to puzzle over why women can conceive without orgasm, over what possible function this trait should serve in females, when we do not wonder why males evolved the same trait ... the assumption that the male way is the only way has kept us from asking some obvious questions ... All of us, however, would benefit by abandoning the male model; female orgasms can be adaptive without being exactly like male orgasms (2002, p. 152).

Whilst maintaining that “biology can extend the boundaries of our thinking about gender as it can for so many other ideas”, in stepping back to analyse and examine these and other examples of bias, Marlene Zuk supports her contention that “feminism has more to say to biology than biology does to feminism” (2002, p. 17). If we now shift our gaze away from science, though, it is clear that bias complicates the issue on both sides of the campus.

My discovery of philosopher Griet Vandermassen’s text (2005) left me downcast for some weeks, thinking from its title (Who’s afraid of Charles Darwin? Debating feminism and evolutionary theory) and accompanying promotional material that, because of unplanned breaks leading to an overly long gestation, my thesis had in the end been written by someone else. Vandermassen also sets out to

---

200 Also see Meredith Small’s discussion on this point and her comment that there is a “sneaking suspicion that male scientists are trying to take credit for female sexuality”. In common with Zuk, she notes that the “males-got-it-first orgasm theory is shaky on several levels” (1993, p. 139). For a treatment of the various approaches to female orgasm, also see Elisabeth A. Lloyd (2005), who leans to the artefact explanation; but see also a sociobiologist’s review of Lloyd’s text (Barash, 2005). Both are excellent examples of the gulf that lies between many feminists and sociobiologists.
demonstrate that there are missed opportunities for conversation between feminism and sociobiology; in her case, specifically with evolutionary psychology. Her book does traverse some similar ground to this thesis, and we agree that an evolutionary paradigm has already been shown to be useful for feminism in the work of the sociobiological feminists, and has potential for much greater exploitation by feminist body theorists. But I finished the book feeling unsettled by the extent of her investments in evolutionary psychology and her treatment of feminism’s reservations, despite herself being a feminist.

Vandermassen sets out to explore the relationship of feminism with science, and with sociobiological science in particular. In the process, she takes on board and endorses many of the theories that are espoused by ‘classic’ sociobiologists and by evolutionary psychologists. For example, she explains and approves William Hamilton’s theory of inclusive fitness (2005, pp. 76-77), Robert Trivers’ theory of parental investment (2005, p. 81 ff), and Sarah Hrdy’s challenge to notions of the ‘coy’ female (2005, p. 125 ff). She commends Richard Dawkins’ careful separation of values from facts in his works, commenting that “most sociobiologists” do so (and noting that E.O. Wilson does not) (2005, p. 124). She clarifies the different approaches to sociobiology taken by various theorists, and carefully distinguishes proximate from ultimate causation arguments. Yet, despite being made by a feminist, and thus perhaps less likely to raise suspicion from the outset, some of the arguments she makes are not so different from the arguments made by the sociobiologists themselves. Thus, while her explicit commitment to feminism can work to disarm our reflexive suspicion long enough for us to more
clearly see the strengths of those sociobiological theories which are already strong, by itself it cannot work to improve the weaker of the theories that she also approves. For example, notwithstanding her careful attempts to qualify and temper evolutionary psychologist David Buss’s research on mating strategies, the concerns expressed earlier in this thesis about work of this kind remain. Vandermassen seems to accept, in a relatively uncomplicated way, claims for such things as an evolved male preference for youth and beauty as an indicator of fecundity; a female preference for high-earning males; differing sexual strategies as an evolutionary outcome of unequal investments in the processes of reproduction; the complicity of women in the perpetuation of patriarchy, and the ‘unique vulnerability’ of women as an evolutionary certainty. It seems to me that while these things may be true, alternative explanations and inquiry into the robustness and politics of this research cannot be easily dismissed as reflexive feminist biophobia.

In counterpoint to the feminist sociobiologists’ empirical work that underlines malestream bias in sociobiological thought, within her explication of sociobiological theory, Vandermassen devotes considerable space to interrogating feminist responses to it, and to detailing the ‘female bias’ inherent in this body of critique. She identifies the major issues – the nature/nurture debate, genetic determinism, the naturalistic fallacy – where feminism and sociobiology are at odds, and convincingly re/presents the counter arguments to them. As

---

201 See p. 197 where Vandermassen agrees with Robert Wright’s assessment that “women are in some ways uniquely vulnerable” and that this vulnerability stems from the “specificities of the female mind”.

228
well, she identifies a number of misreadings and misrepresentations of sociobiological work in feminist critiques. For all of this, I have some sympathy, and while she may be correct in saying that criticism of feminism “is not a risk-free activity” (2005, p. 85) (is criticising any discipline risk-free?), if there is valid criticism to be made, it should be made. Defensiveness is to be expected, but so also is robustness. In my view, her text makes an important contribution to the conversation that must be had between feminism and sociobiology.

That said, a major limitation of Vandermassen’s own story lies in how she casts the terms of that conversation. Her thesis is that there are problems on both sides – with feminism’s ‘biophobia’ and evolutionary psychology’s ‘youth’ – but that “feminism and evolutionary psychology need each other” (2005, p. 195). That she believes feminism’s need is greater is apparent in her claim that evolutionary psychology can provide the ‘unifying framework’ that feminism is lacking, and her emphasis on the need for feminists to accommodate (socio)biological narratives. While my own conviction of the need to be more open to the possibilities for feminism within such accounts was a motivating factor in this research project, clearly there must be a corresponding openness in the sociobiologists’ accounts to take seriously and accommodate the valid concerns of feminists. Vandermassen is both more forgiving, and more optimistic about the legitimacy and usefulness of some theories from evolutionary psychology for feminism, than I am. Importantly, I think, in explicitly recognising the problems inherent in critique undertaken from a political or ideological stance, she appears to forget that this difficulty cuts both ways. In characterising feminism as
‘phobic’ and evolutionary psychology as merely ‘young’ (still to mature), Vandermassen is in danger of forgetting that feminism does have valid concerns. In shifting the critical gaze to feminism, she risks being too much of an apologist for some of the less useful and more dubious evolutionary psychology projects and, in so doing, demonstrates exactly the problem of standpoint bias that Marlene Zuk has observed.

A more successful attempt to traverse the same difficult terrain can be found in the work of Janet Radcliffe Richards. In philosophical works published more than two decades apart, Richards demonstrates a longstanding interest in identifying the obstacles in the way of a profitable interaction between feminism and reductionist theory. In her earlier work, *The sceptical feminist* (1982), Richards’ critical focus was on feminism – which she defined as the belief that “women suffer from systematic social injustice because of their sex” (1982, pp. 13-14). Endorsing the feminist position that women have historically been subject to such injustice (justified by their ‘difference’ from men), in her early work she exposed the flaws in ‘traditionalist’ arguments that purport to know the nature of women. Taking her reader through a discussion that ranges across issues such as work, fertility, mothering, social justice, and the ‘proper place of nature’, Richards is very clear that feminism has cause for concern.

At the same time, she was critical of those feminists whose arguments exclude any evidence which does not fit within their own social theories. In adopting a

---

202 The title of Chapter 2 in *The sceptical feminist.*
position which says that we “need to know about the nature of the world we are dealing with, to as great an extent as possible”, she noted that “the knowledge we have of the natures of things in no way dictates what use should be made of the raw material” (1982, pp. 84-85). Put another way, knowing the nature and extent of any difference between men and women in no way carries with it an obligation to make no effort to change it:

Even though the two camps [traditionalists and feminists] reach different conclusions, both are making the fundamental mistake of thinking that if you know the nature of something (what it is like) you ipso facto know what is natural for it (what it ought to be like, how it should develop). This is false … If we know that men and women are different at birth that does not show that they should be kept different; if they are more or less the same that is not enough to show that they should be kept the same. Why should we not increase or decrease the differences? (1982, p. 82).

As the earlier chapters in this thesis make clear, the dangers of the naturalistic fallacy are by now well-rehearsed on both sides of the feminist/reductionist divide. In company with much of the rhetoric around the necessarily entwined qualities of nature/nurture, this response to the difference/sameness problem still tends to evoke a ‘yes, but’ argument. It is very easy to begin with the rhetorical gesture – just because (even if) it is this way, that doesn’t mean it has to remain this way – but it is commonly followed with an argument in defence of establishing our own particular concerns and investments. Typically, this defence is advanced by one of two routes: to either disprove that it is this way in any event; or, to more firmly establish that it most certainly is this way, and, that although change is possible, for one reason or another it is not at all probable.

Richards foresaw this kind of difficulty:

While it would be ideal if everyone could just assess each controversial problem on its own merits as it arose, what actually happens is that people usually start by deciding whose side they are on, and from then onwards tend to see everything that is said or done in the light of that alliance (1982, p. 15).
In this early work, then, Richards explicitly addresses the problem of bias identified by Marlene Zuk: the sheer difficulty within a polarised critical environment of not taking up a position at the outset and prejudging the result.

Returning to more recent times, in her latest work (2004), Richards’ critical focus is more evenly divided between the content and effect of neo-Darwinian theories, and the coherence and strength of feminist, among other, criticisms of them. Richards follows Daniel Dennett in characterising Darwinism as a ‘universal acid’ that threatens to ‘leak out’, spreading “all the way up, dissolving the illusion of our own authorship, our own divine spark of creativity and understanding” (Dennett, 1995, p. 63). Her main aim in this work, then, is to examine “the extent to which these threats are all that they seem” (Richards, 2004, p. 23). To achieve this, she interrogates those elements of sociobiological theory that are identified as problematic by feminists and other critics - questions of free will and individual responsibility; altruism and selfishness; ethics and morality; and the very application of such theories to human nature – using philosophical techniques. These generally take the form of logically considering certain conditional statements and their implications (‘If current theories of evolutionary psychology are right’ these things will follow).

Richards utilises widely criticised texts from sociobiologists Robert Wright and Richard Dawkins, to determine whether notions of ‘biology as destiny’, ‘blameless puppets’, and the (im)morality of selfish genes can be logically
deduced from their evidence, and to reflect on whether the implications inherent in what she calls the ‘blank paper’ view of human nature are any more comforting. She decides, in the first case, that none of these conclusions of determinism can hold under logical scrutiny; and, in the second, that when subjected to a comparable series of philosophical techniques, the ramifications of the blank paper view may well be similarly unwelcome (2004, in particular pp. 118-125). Neither set of arguments escapes problematic nuance or repercussion. She claims, in the end, that “the different depths of Darwinism have in themselves no implications for most ideas about ourselves and our situation” (2004, p. 270, my emphasis). It is, again, the assumptions underlying our different preconceptions and positions – in effect, which ‘side’ we are on before we begin to consider the arguments – that require the most rigorous unpicking.

Richards, like Vandermassen, is intent on explaining evolutionary psychology to feminism and, to some extent, on rescuing it from its determinist implications. Both attempt to make evolutionary psychology more ‘user-friendly’ for feminists. Despite sharing with Vandermassen an evident sympathy for evolutionary approaches to human nature, Richards is more successful in doing this for two reasons. Firstly, in reaching her conclusions, she does not forget, regardless of the contingent nature of sociobiological theories, that feminists have legitimate concerns about their real world applications. Importantly, in positioning herself as interrogating philosophically the logic of the arguments from both sides, and despite in the end dismantling the logic of some specific feminist concerns, she does not fall into the error of dismissing feminist fears as irrational. And
secondly, while, like all of us, Richards begins her journey from her own individual location, she is consistent in foregrounding her recognition of this, and of the influence of standpoint bias on the outcome of critique.

What is very clear from all of this is that critique is always already locked into an oppositional strategy. No matter what kind of critique we set out to encompass – whether it be the unqualified rebuttal critique of outsiders (such as were discussed in chapter one); the insider’s qualified critique that offers counter-examples and alternate explanations (such as the challenges to mainstream theory by the sociobiological feminists discussed earlier in this chapter), or the kind of critical endeavour that seeks to explain opposing positions to each other with the intention of dispelling suspicion and misunderstanding (Vandermassen and Richards) – our politics intervene. There is no innocent place to stand. While each critical approach has its place in shifting the conversation forward, we do not seem to shift very far by continuing to debate what the science shows us. Implicit in the critical debate, however, is the construct that can emerge from it; those moments where we shake free of the limited options available to us and attempt to reshape how we think about what the science says about our bodies. So, if we consider the move from critique to construct, and look for instances where feminists on either side of the campus try to talk about how bodies work in a way that gives biology legitimacy without making it over-determining, how do we fare?
The construct dialectic

Much of the work that is being done by the sociobiologist feminists is confined to observing and reporting on the exception to the rule, and in this way poses alternatives to existing theory about behaviour. The nature of the science they are doing stops short of pursuing a new way of thinking about bodies, since this is not their primary concern. Nor, perhaps, do they feel an urgent need to do so, having already married the social and the biological to their own satisfaction within the sociobiological paradigm (while, as with all successful marriages, remaining open to continuing compromise and adaptation).

Nevertheless, we have seen that, within the sociobiological model, their work has added to an accretion of knowledge that has, piece by piece, acted to nudge their male colleagues out of their comfortable, largely unexamined and unreflexive beliefs about female natures. This work, without explicitly setting out to do so, has in places moved from a reformulation of existing theory to a retheorisation of the representations of women within sociobiology. Sarah Hrdy, in particular, out of her observations, and her consequent critique of notions of mothering over three decades, constructs new theory that has both positively impacted sociobiological thought, and confounded status quo thinking about biologically determined social roles.

If theory emanating from within sociobiology is accused of being too focused on what is believed to be under the surface – on the behavioural biology contained
within the social biogram – feminist body theory has been accused of too often *stopping at* the surface. Feminists on both sides of the campus have noted that the renewed feminist focus on the body does not encompass an interrogation of the body’s interior. Biologist Lynda Birke records her “frustration at the gap between feminist cultural analysis and [her] own background as a biologist” (2000, p. 3), and, as I mentioned in chapter one, psychologist Elizabeth Wilson is forthright in confronting what she calls ‘feminism’s antibiology’, observing that “[d]espite an avowed interest in the body, there is a persistent distaste for biological detail” (1998, p. 15). Both attempt to remedy this situation in works that explore the inner processes of our physiology, with the explicit intention of fashioning new ways of thinking about the biological body for feminism.

In *Feminism and the biological body* (2000), Birke again takes up her earlier concern with thinking about non-reductionist approaches to biology and the gendered construction of bodies. She is intent on exposing what she calls the ‘black boxes and tedious universals’, particularly in feminist conceptualisations of the body where the “anatomical, internal, body … seems to disappear, except as a set of signifiers” (2000, p. 27). As well, Birke draws together illustrative themes from representations of the body to show how understandings of our physiological processes are immersed in, and interpreted through, the language of science. As one might expect, she brings her training as a biologist to a feminist examination of scientific readings of the body. Thus, she delves into the meanings that are read onto bodies through the medium of technological visualisations of our

---

203 The title of Chapter 2 of ‘Feminism and the biological body’.
internal anatomies and processes, through classificatory and other systems, and through metaphor. For example, she discusses and likewise rejects Donna Haraway’s reading of the organism as an information system, as techno-cultural cyborg, arguing that its “potential to damage through fragmentation” (2000, p. 155) conceals a slippery slope to genetic reductionism.

Birke notes that while reductionism is a useful scientific method, “much has been lost in the stories that we might tell of how our biology works” (2000, p. 140). The reductionist view, she argues, leads to a perception of bodies as fragmented, an assembly of parts without intrinsic value (2000, p. 170). In place of these understandings of the body, Birke proposes instead a vision of:

the body becoming, as transformative. I want to question the notion of the biological body as bedrock. I do so partly because I consider that it gives a limited understanding of our biological bodies, and partly for more political reasons, because such a view of biology leads to determinism (2000, p. 136).

In pursuit of a theory of ‘the body becoming’, Birke constructs an alternate story of “organismic agency and transformation” which both works against notions of fixity and reductionism, and allows us to conceptualise the body as fluid, dynamic, and changeable. In so doing, she writes approvingly of Elizabeth Grosz’s use of the Mobius strip motif, and proposes that we need:

… an understanding of the biological body that links inner and outer, rather than presupposing a singularity to the body. We need, moreover a biology that is not determinist, nor is seen as foundational or presocial (2000, p. 174).

In sum, Birke refuses fixity and instead insists that feminism should struggle to “rename nature through complexity and transformation” as a strategy to challenge “persistent dualisms” (2000, p. 155). However, Birke seems not to
notice that she herself, in proposing a theory that ‘links’ inner and outer, is both thinking and proposing a model of the body that risks rehearsing such dualisms. As well, the positive connections that could be drawn with genome and sociobiological themes of the mutability of biology and its inextricable link to environment are evident here, but Birke does not draw them. Instead, she challenges those scientists who “too glibly” trot out their belief in gene/environment interaction (2000, p. 139). Recognising that this, in some cases, is a more than fair accusation, it also falls back into old critical habits and misses an opportunity to imagine or outline a new project that rethinks feminist and evolutionary/genetic theory.

Elizabeth Wilson also attempts to disrupt feminism’s ‘compulsive antiessentialism’ in two full-length works, theorising the body through its various biological processes and, in this way, opening feminist body theory to the notion of a bio/social body. In the first (1998), she undertakes a critical reading of the relationship of feminist studies of science to the fields of neurology and psychology, and, secondly and more recently, she examines the detail of the neurological body (2004).

In her earlier work, Wilson explicitly objects to the ‘critical habits’ of an exclusionary feminist psychology that, as a result of its political commitments and epistemological practices, neglects to examine (among other areas) evolutionary psychology. This exclusion, she says, is ‘enforced’ by the operation of a series of familiar dualisms – among them, human/animal,

Also in this earlier work, Wilson counters the prevailing acceptance of neuropsychology as an “antidualist account of the mind” (1998, p. 121), claiming that it does not, in fact, resolve the Cartesian dualism. She explicitly deals with the tripartite distinction between mind, brain and body, and identifies as a “brain-body reductionism” (1998, p. 123), the ‘decapitation’ of the neuroscientific brain from the body. She says:

Through this decapitation, the disembodied brain (and often the head in general) becomes the seat of the intellect, and the headless body becomes the home of sensation and the guardian of the passions … traditional mind-body dualism has been displaced and disguised through embodiment … Contemporary neuropsychology dissolves a superficial mind-body opposition via the materiality of the brain but redeploy its cognate couplings (intellect-sensation, reason-emotion) in the division of brain and body (1998, pp. 124-125).

Wilson evaluates this notion of decapitation in conversation with Simon LeVay’s work on the neurological/genetic basis for homosexuality. In doing so, unlike other corporeal theorists, she steps across the campus to directly engage with an example of scientific reductionism; she challenges the utility of the dualistic thinking inherent in LeVay’s approach, arguing it ‘quarantines’ the body and its organs from the sexuality he locates within the biology of the brain (1998, pp.

---

204 See chapter five.
Wilson theorises that the “materiality of neurocognition” is not locatable within an “inert and originary location”, arguing instead for what she calls “neurocognitive mobility” (1998, p. 203), which potentially disrupts the brain/body dualism, and forces a reassessment of our critical habits.

In her second work, Wilson takes up Vicki Kirby’s challenge that feminist theories about biology should include “the peristaltic movements of the viscera, the mitosis of cells, the electrical activity that plays across a synapse”, to theorise about a body “whose tissue includes all the oozings and pulsings that literally and figuratively make up the differential stuff of the body’s extra-ordinary circuitry” (Kirby, 1997, p. 76). In Psychosomatic (2004), Wilson argues, again, that neurological theory is useful to feminist body theory, returning to notions of “connectivity/connectionism”. She examines this ‘entanglement’ of nature and nurture through a series of examples. She again discusses Simon LeVay’s work on sexual orientation, and also examines a Darwinian account of the phenomenon of blushing, which she concludes is an outcome of a ‘material entwinement’ of the biogenetic and the psychocultural (2004, pp. 75-77). In a move that appears to breach the boundaries between evolutionary psychology’s theory of the modularity of mind and feminist body theory, Wilson approves ideas about neurological modularity, of “emotional and cognitive worlds as a series of interfacing systems” (2004, p. 94). Wilson thus expands and extends Kirby’s program, providing a model for the sort of interactionist work that opens feminist body theory to new understandings of biology.

She returns to LeVay’s work in greater detail in her later work (Wilson, 2004, pp. 49-62).
Wilson and Birke share a vision (along with many reductionist scientists in all three enterprises discussed in this thesis) of the body’s biology and the social environment as interactive, interconnected and mutually constitutive. Both are very clear about the need to fill the gap within feminist theory of the body between the social body and the material body, the ‘flesh’ beneath/including the skin. Both are doing good things in the attempt to define or recreate feminist body theory. I would argue however, that of the two, Wilson more clearly addresses the question of how do we achieve that? Partly, this is because Wilson more directly sets out to construct new theory, while Birke’s theory still largely emerges from a critique of existing theory. Partly too, it is because, within their readings of the interactionist nature of biology and society, Wilson is less committed to arguing for the pre-eminence of one or other than is Birke – Birke’s work, in other words, still invokes, for me, a ‘yes, but’ response in favour of culture, in answering the question of which of the two is the more important. This is, of course, my individual response and here I admit the possibility that my own bias against political positioning plays a part in my conclusion. If I am to maintain my role as an interested observer (although clearly not an impartial one, despite my best intentions), I must confess that my concern about Birke’s framing of her opposition to the body as ‘bedrock’ as partially politically motivated, is itself partially ‘politically’ motivated. As I have said in relation to a number of themes throughout this thesis, I strongly believe that simply because something may be politically unpalatable is no reason to reject it as a possibility, or even as a probability.
Given Birke’s dismissal of a “biological bedrock onto which experience and environments write” (2000, p. 29), it is not surprising that her work does not converse with, for example, that of Sarah Hrdy. Wilson, on the other hand, with her focus on the neurobiological body, might be expected to have opened a dialogue with Hrdy’s and other feminist sociobiological readings of behavioural biology. That she has not, indicates that underneath the schism between feminism and science, lies a still deeper schism between scientific/sociobiological and philosophical ways of thinking about the body, and this continues to prevent alliance, even between otherwise like-minded feminists, across this boundary.

**Conclusion**

To end where I began, Patricia Gowaty notes that the first Darwinian Feminists meeting, in 1993, consisted of just thirteen women (1997a, p. xv). In the intervening years, greater attention has begun to be paid to the possible value for feminism of insights from the evolutionary sciences, and, conversely, the benefits that accrue from applying a feminist viewpoint to these disciplines. There are also now many more feminists convinced of the need to in some way encompass the insights of biology into a more coherent theory of the body. The examples I have drawn from the literature in this chapter expose a range of approaches to encompassing (socio)biology from within feminism on both sides of the academy. Still, where feminists position themselves along the nature/nurture divide dictates their starting point and, in many ways, also their ending point.
On the nature side of the boundary, the sociobiological feminists, in a sense, add or apply their feminism to a view of biology that proceeds from an acceptance of a complex social biogram. As a result of the evolutionary predispositions contained within that biogram (which may or may not be related to biological sex), individuals certainly have the capacity and the flexibility to alter both their behaviour and their proximate environment, but this capacity for change is not without biological limits. Both critique and construct are thus constrained by working within a framework of ultimate causation, where culture is recursive – both the outcome of adaptation, and what is being adapted to. Whatever occurs within this paradigm, and in the great deal of exciting feminist work being undertaken within it, it begins and ends with that bedrock of biology.

On the nurture side of the boundary, the social biogram is a vigorously contested site. Our evolutionary predispositions, such as they are, are limited to sex-neutral generalities like eating, excreting and sleeping\textsuperscript{206}. Despite the growing recognition of the need to encompass biology, and the innovative ways in which new theory is being framed, for feminists on this side of the boundary, it still begins and ends with culture. Biology, as materiality, as the “differential stuff of the body’s extra-ordinary circuity” (Kirby, 1997, p. 76), is now theorised as mobile, fluid, and agentic – all of which are consonant, as we have seen, with scientific understandings of biology. However, the agency of a behavioural biology remains largely unconsidered here and, apart from obvious limitations imposed by our specifically human physicality – such as flying or breathing

\textsuperscript{206} See chapter 4.
under water – it is not biology but proximate culture which constrains
behavioural change.

For one side, then, it is very clear that biology is important, and that it somehow
or other shapes culture; for the other, it is very clear that culture is important,
and biology is not at all over-determining of behaviour within culture. And
tentatively attempting to balance at the boundary, but not wholly successfully,
are a few feminists, who primarily seek to put biology into feminist body theory,
largely by re-interpreting and making more palatable theories of biology drawn
from the social biogram.

In fact, it seems to me that the two sides are not really all that far apart on the
details. But, caught within the oppositional trap of being either too suspicious, or
not suspicious enough, it is the promise of a new theory of the bio/social body
that suffers. And in our struggle to convince each other to cross the boundary
that divides us, what we all risk forgetting is that “the organism is predecessor to
our theories about it” (Henry, 2001, p. 91). If we could somehow explicitly
recognise and put aside the tension between proximate and ultimate causes, and
truly suspend this pointless debate about which of nature or nurture is dominant,
we may be able to shift the conversation from the level of disagreement about
our individual starting points, or about the evidence, and ask instead: as
feminists, what is our collective objective in asking these questions? What are the
questions we really want to ask about how our bodies intersect with the cultures
in which we live? And how can we utilise what is being done across the board to
reframe the agenda? The condition for a way forward is a more self-critical approach to our own disciplinary commitments; a more interdisciplinary approach; and a more self-conscious (real) commitment to the project of how the biology and society complex actually works.
Conclusion

Enough! A century of misunderstanding, the drawn-out Verdun and Somme of Western intellectual history, has run its exhausting course, and the culture wars are an old game turned stale. It is time to call a truce and forge an alliance. Within the broad middle ground between the strong versions of the Standard Social Science Model and genetic determinism, the social sciences are intrinsically compatible with the natural sciences.


The self is a byzantine bureaucracy, and no gene can push the buttons of behavior by itself. You can attribute the ability to defy our genotypes to free will, whatever that means, but you can also attribute it to the fact that in a hundred-trillion-synapse human brain, any single influence can be outweighed by the product of all of the others.

Steven Pinker (2009, p. 8)
Conclusion

The conundrum is still one about how human beings take their biological endowments and turn them into scripts for social roles. It is still important to disentangle the biological imperative from the social mandate, and still essential to observe at the beginning, in the middle and at the end of all such discussions that description and prescription are not the same thing.

Ann Oakley (1985, Foreword, p. 2)

[The whole incoherent opposition between socially determined (or culturally determined) phenomena and biologically determined (or genetically determined) phenomena should be consigned to the dustbin of history.


The past two decades have seen an explosion of information about the ‘biological imperative’, emanating from a range of sources. In particular, the undertaking of the Human Genome Project, with its grand vision of capturing and deciphering our genetic code, has placed biology firmly back on the agenda, and reinvigorated open debate around questions of nature and nurture within the academy and the wider society. The 21st century is indeed shaping up to be the ‘century of the gene’; we live in an era where the ceaseless pronouncements of reductionist science are assiduously studied, broadly disseminated, and routinely embraced. No matter where we stand in relation to this new knowledge, and the
extensive theorising that it engenders, we cannot escape its implications, its possibilities, and its dangers.

In this context, my motivating concern in this work has been the risk to the relevance of feminist body theory if our projects on ‘the body’ continue to ‘think’ dualistically in pursuit of a social body that is not also a biological body, rather than to think of and theorise about the body as bio/social. Accordingly, I have adopted Marlene Zuk’s premise that to view science as a “tool for ideology” or as a “weapon in the gender wars” (2002, p. 160) is unhelpful. Instead, I have attempted to work through the concerns that feminists have expressed about reductionist stories of human nature by taking a walk through the three interrelated and highly visible scientific endeavours of the genome project itself, sociobiology, and sex difference research. I then paused and directed my gaze away from the academy to the world outside, where this research is re-presented and mass-marketed in texts which, no matter what disavowals the scientists might make, are the popular face of their research efforts. In all but the last case,207 I positioned myself as a women’s studies scholar intent on understanding and finding the positive aspects of these reductionist enterprises, rather than on traversing critical ground already well represented in the feminist literature. In general, I have not been interested to pursue the validity or otherwise of this research or of its critique, but have instead been concerned to follow a path through the history, hypotheses, arguments and outcomes of each that would disclose the general shape and purpose of the enterprise in order to understand how

---

207 Where, of course, there being no usefully positive aspects to this work, I was justified in approaching the material much more critically from the outset.
concerned, and how interested, feminism should/could be. The methodology I have followed to guide my walk through these unfamiliar byways has been dictated in large part by the sheer mass of available research within and about them. Inevitably, much has been overlooked or mentioned only in passing that could, with a more detailed focus on each individual discipline, be usefully discussed. Nevertheless, I believe that the drawing together of these disciplines in such a way that the links between them are made more visible is an important task. It is also necessary for my broader enterprise of interrogating how we are placed to bring culture and biology, feminist body theory and sociobiology together.

In Part One of the thesis, I laid the groundwork for my overarching concern with the problem of dualistic thought, and the trend within feminist theory to reflexively distance itself from biological explanations for human nature. Chapter One is a brief excursion through the various ways in which feminists have attempted to deal with the seemingly intractable problem of sex difference, in conversation with each other and with reductionist science. Utilising the device of a loose taxonomy of the positions taken by feminists on these issues, I discussed five different modes of approach to biology that are variously situated around the essentialist/social constructionist, sex/gender, sameness/difference and nature/nurture splits. I concluded that, while each approach is a necessary and valuable addition to the feminist toolbox, none manages to solve the dualisms, or to itself escape the trap of dualistic thinking, because each, in its own way, resolves by returning to an investment in one side of the binary.
In Part Two, I crossed the campus to walk through the unfamiliar territory of those reductionist sciences that have, individually, been the focus of so much feminist critique and which I believe, collectively, have the most to say, to us and to each other, on questions of biological sex difference. In Chapter Two, I focused my attention on the Human Genome Project, situating it first within the rhetoric of control that is clearly expressed by the choice of metaphors used to promote and describe it. I then outlined its history and antecedents, its objectives, its outcomes and real and expected benefits, before moving to a discussion of the controversies that surrounded the project itself, and the anxieties that still exist about the post-genomic research resulting from it.

I concluded that both the claims made for, and the fears expressed about this endeavour have been tempered in its realisation. The early promise of control has unravelled in the face of the limitations imposed by genetic complexity, while concerns are partially met by the investment in interdisciplinary research around various ethical, social and legal issues. It does seem likely that we will see a gradual increase in health benefits through the alleviation of disease. It also seems to me likely that a continuing investment in behavioural genetics will eventually succeed in establishing a more robust link between genes and behaviour than has thus far been the case. That being so, feminist theorists can take heart from the recognition of the contingent, flexible and fluid character of ‘nature’ that is a visible outcome of genome and post-genome research.
In Chapter Three, I moved on from the science of human nature to the science of sex difference research. I first placed the roots of this research enterprise within an Aristotelian intellectual tradition that constructs women’s reproductive capacities as the essential feature of ‘woman’, and as the key factor in female cognitive process and ability. Following the debates around sexual dimorphism in the brain from ancient times through the Victorian era to modern times, I exposed the continuity of habits of thought and approach that link current research to past endeavours. I demonstrated that within sex difference research, ‘woman’ is still confronted by the false question of her difference from ‘man’, while men continue to escape the practical consequences of biological constraints. While there are encouraging indications of more moderate voices within the discipline to challenge the worst of the presuppositions and implications being drawn out of this research, I found that sex difference research is still often reactionary, with many of its pursuits deeply embedded within outmoded notions of ‘science’ and of the natures of women and men. In sum, I concluded that, of the three areas of academic research I walk through in this thesis, sex difference research is the least useful for feminists wanting to utilise reductionist science towards a more positive theorisation of biology’s role in the construction of sexed bodies.

Chapter Four of the thesis explored issues within sociobiological theory of an evolved human nature and of sex-differentiated behaviours. In asserting that the conversation between feminist theory and sociobiology had become unproductive, I located my interest in this research around a belief that
sociobiology may have more to offer to (and in common with) feminism than our separate ideologies and mutual suspicion allow us to recognise. Accordingly, noting but not dwelling on the problem for sociobiology of the misogynistic claims made by some of its practitioners, I began my inquiry by explaining the differences between diverse endeavours that I then, for the sake of convenience, largely drew back under the umbrella term of ‘sociobiology’.

Having first accepted, for the purposes of argument, the fact of a social biogram, the elements of sociobiology that I identified as most central to feminist concerns are those of ‘genetic determinism’, and human and sex-differentiated behaviours. I investigated notions of genetic determinism by reviewing discussions around genotype/phenotype interactions in a selection of works, and by describing the various meanings given to the gene within sociobiology, focusing in particular on the confusion around the determinist implications of labelling the gene as ‘selfish’. I concluded that in the main those writers I survey clearly express a belief in the uncertainty and unpredictability of gene/culture interactions that is fully consonant with the information arising from the post-genome work, but that there are also places in their work where inferences of inevitability can be drawn. Finally, I returned to the difficulties inherent in the very notion of a social biogram, evidenced through the sociobiological literature on human universals, and sex differentiated behaviours. I argued that while the behaviours that are alleged to be contained within the social biogram carry some baggage that will inevitably raise questions about the care with which such arguments are framed, and the politics of those who espouse them, the fact that many of these
behaviours tend to prop up the status quo is not a sufficient reason to dismiss them. In relation to sex differentiated behaviours, however, I concluded that certain researchers in this area of evolutionary psychology, by expressing their readings of the data with such certainty and ignoring alternative available interpretations, blur the boundaries of the naturalistic fallacy to the point where determinism becomes an almost inescapable inference.

In Chapter Five, I took a short walk away from campus to look at what happens to the research in these academic fields when it is translated into popular culture. On the basis of their popularity, longevity, and the pattern they have provided for later texts, I compared two of the earlier examples of this kind of work across the major features that they have in common. I claimed that it is important to undertake analyses of these kinds of texts because they draw on the authority of ‘Science’ to establish the authenticity of their conclusions, and because they present these conclusions as the ‘accepted’ science in these matters to a broad readership.

I argued that the two texts I analysed in this chapter follow a distinctive pattern of, first, establishing that not only do men and women differ from the beginning, but that men possess a superior difference; second, pathologising homosexuality, and constructing arguments that lead to a conclusion that gay males are more like women than they are like straight men; third, utilising hormone research to confirm men’s natural predisposition to occupy positions of authority and women’s natural role as society’s caretakers. I concluded that these texts, and
texts of this kind in general, are dangerous in their facile explanations for complex biological and social issues. I suggested that feminists, the media, and most particularly the scientists upon whose work they draw to support their claims, must police, dispute and disrupt the easy slide into vulgar reductionism that these texts represent.

In Part Three of the thesis, I returned from my observations of the ‘other side’ to look briefly at some of the ways in which feminists, from both sides of the campus, have begun the work of positive intervention in, and interaction with, reductionist science. In Chapter Six, I noted that the earliest and most successful of these contributions, in my view, have come from feminists situated within sociobiology itself. More recently, Darwinian feminist philosophers have attempted to explain arguments from evolutionary psychology in a sympathetic light, with limited success. And finally, some feminist body theorists have attempted to deflect feminism’s reflexive distancing of biology, by constructing theories of the body through examination of various biological processes, but still, I have suggested, largely without engaging directly with the reductionist notion of a complex social biogram.

This thesis had its beginnings in Elizabeth Grosz’s ‘throwaway’ line, and the renewed flowering of genetic and sociobiological research in the 1990s. The conjunction of these two things caused me to more carefully consider the content and tenor of feminist work on the body, and set me on a long path through the unfamiliar, sometimes difficult, and often seductive territory of the ‘other side’ of
campus. My fundamental presumption was that biology cannot be left out of any account of the body, and my approach encompassed a desire to accept that (most) scientists act in good faith, and that reductionist science has valuable things to say to feminist theories of the body.

I remain convinced that it is a nonsense to deny the body’s biology. I remain optimistic that proponents on both sides of the nature/nurture divide in general act in good faith and that there is more in common philosophically between them than either often admits or, indeed, seems to recognise. I believe that we need not and should not be so nervous about much of the research that is going on in these reductionist sciences; nor should we be too quick to make assumptions that it is biologically or genetically determinist in intent, even if it is in effect. I am strengthened in my belief that feminist body theory can no longer skirt the question of difference; we must deal with it directly, and not as an outcome of some quantifiable choice between biology and society. As a feminist, I remain equally convinced that feminism has valuable things to say to reductionist science about the way out of determinism. I suggest we set aside Ann Oakley’s ‘enduring questions’, since there are clearly grounds for imagining that neither one of nature or nurture is more important. Instead, reductionists and feminists alike, we should all be – some already are – theorising a bio/social body, the entire package, rather than arguing over or attempting to tease out the bits that might be nature, and the bits that might be nurture.
So, in endorsing the importance of the corporeal project, I conclude that one useful way forward towards overcoming the problem of dualisms and dualistic thinking is to adopt an explicit agenda that acknowledges the complex social biogram. I believe that the notion of an evolved social biogram that treats body and mind (and in some ways society itself) as an indissoluble whole sits well with the feminist project of overcoming the dualistic Cartesian model of body/mind and thus is potentially very useful for feminist body theorists. That being so, instead of investing in a notion of irreconcilable differences, it may be useful to adopt a more open, less confrontational approach which says, “Well, this might be true – we may not at this point concede it one way or the other – but what if it is? So what? What does this knowledge really mean for how we might think about our bodies? Where might this productively lead us in our theorising (of the body/of individual and social relations)?”

At the same time, in engaging with reductionist theory, we must not be apologetic or defend them. There is still ample evidence of sloppy thinking, gross oversimplification, and lack of rigour in the ways some scientific findings are being interpreted and in the conclusions being drawn from them, and so we must remain vigilant in holding individual practitioners to account for what they say. We must still insist on precision in ensuring that the claims they make are hedged about with the requisite levels of uncertainty and caution that are evident within much of their own work, but which too often gets lost in translation into the public arena. More broadly, we must continue to challenge the enterprise
itself to be more intellectually and socially responsible – to be more rigorous and far less speculative when it engages with the complexities of human social life.

Although, because of my focus on the ways in which feminism tends to deal with biology and ‘read’ reductionist discourse, this thesis has primarily been addressed to other feminists, of course the conversation needs to be progressed from both directions to be productive. This requires a commitment on the part of the scientists who work in these fields to more explicitly acknowledge that the context within which they work is never scientifically neutral, but is politically, philosophically, socially and, increasingly, economically bound up with particular systems that come with their own expectations and demands of science. There are hopeful signs that this is occurring already, most notably in the interdisciplinary focus of much of the work in behavioural genetics. But it also requires a commitment on the part of feminist body theorists to abandon the attempt to make biology comprehensible in our terms, and to instead undertake the difficult task of making our terms meaningful in light of biology. Again, there are encouraging moves in this direction. For my part, it was well worth the walk.

We can engage more positively with reductionist science. I would like to invite ‘them’ to walk across the campus and join ‘us’ in that project.
Bibliography


Duggan, Mike. (2004). *Men fake foreplay ... and other lies that are true*. US: Rodale.


Fisher, Helen. (1999). *The first sex: the natural talents of women and how they are changing the world*. Milsons Point, NSW: Random House Australia Pty Ltd.


Lynn, Richard, & Irwing, Paul. (2008). Sex differences in mental arithmetic, digit span, and g defined as working memory capacity. Intelligence, 36, 226-235.


Patai, Daphne. (2000). Do they have to be wrong? . Gender Issues, 18(4), 74-82.


dissertations. Uppsala, Sweden: Faculty of Social Sciences, Acta
Universitatis Upsaliensis.


Feminism and evolutionary biology: boundaries, intersections, and frontiers (pp. 116-150). New York: Chapman & Hall.


UCI Medical Center, University of California, Irvine. (2002). Protein that halts Huntington’s disease found. Study could lead way to preventing progressive neurological disorder. Retrieved 17 November, 2004, from [http://www.ucihealth.uci.edu/News/Releases/ProteinHaltsHuntingtons.htm](http://www.ucihealth.uci.edu/News/Releases/ProteinHaltsHuntingtons.htm)


