Philosophical issues in scientific realism, experiments and (Dis)unity

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THESIS ABSTRACT

Biological Being: Philosophical Issues in Scientific Realism, Experiments and (Dis)Unity

The biological sciences are changing the ways in which we understand ourselves. *Biological Being* is a philosophical exploration of biology, mapping some of the features of the field that make it so important in generating these changes. Two central themes are at the heart of this exploration: biology is a science that should be grasped from a realist position, and it is a science that reveals a disunified, pluralistic world of kinds of things. After an introduction of some the issues involved, in three substantial chapters these themes are unpacked and analysed.

The first major chapter is about experimentation and biology. In it the experimental realism of Hacking is rejected, whilst the core notion of intervention and manipulation of the world as a vital epistemic tool is retained. Similarities and differences between experiments in the physical and biological science are investigated.

This comparison is continued in the second major chapter, which is about natural kinds and biology's relationship to the physical sciences. Reductionism, even in its weaker forms, is rejected along with the notion of scientific unity. Recent attempts by Rosenberg to understand biology as an instrumental science are contrasted with Dupré's realism, and a system of type-hierarchies that could support realism for biology described.

The third major chapter then looks at biology and the construction of human kinds by the social sciences. A reading of Foucault is given that attacks the idea that there can be a simple distinction drawn between those sciences that discover and those which construct kinds. Biology's role in the social sciences is explored.

A final chapter draws the components of the thesis together and seeks a general understanding of rationality underpinning the whole discussion in recent work by Putnam.

Biological Being:

Philosophical Issues in Scientific Realism, Experiments and (Dis)Unity

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Contents

0 Preface

> There is no doubt that obtaining a consensus can be important in democratic politics. But it is less central in philosophy, where, as Wittgenstein said, 'the philosopher is a citizen of no group of ideas: that's just what makes him a philosopher'; and indeed a political *dissensus* can become the source of a philosophical questioning that transforms the 'wide reflective equilibrium' in unexpected and unpredictable ways. Thus, we need some resistance to our new consensus about consensus in order to find again the fresh air to pursue the odd multiple paths of those attempts which question things. We need again sceptical, disobedient, dissident or cynical styles in philosophy, like those of Foucault or Wittgenstein.[1]

I used to think that philosophy is tremendously exciting because it shows you how things *really* are. Now I see something far more powerful—the uncovering of what is not, what is assumed and what is obscure. An undiscovered truth has nothing like the dangerous consequences of a firmly held false belief. In itself this would make philosophy a necessity to the survival of a species that evolves through its culture and its technology, but it has a further virtue. It presents us with alternatives, 'odd multiple paths,' which show us other possibilities for thinking about things. We are given new ways of understanding both the commonplace and the exotic that liberate and transform us. Often this is the origin of our seeing that the assumed fixed parts of our world are not fixed at all.

Of course there are vices too—the problem of not seeing the wood for the trees in the struggle to discover beetles in the piles of decaying leaves on the forest floor. Certainly analytic philosophers *sometimes* chop off their sense of being human when dissecting the lived, and more literary minded philosophers *sometimes* twist metaphors beyond compre-

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[1] Rajchman, J. (1995) 'Foucault Ten Years After' *New Formations* Number 25 Summer 1995, 16-17.

hension to tell us about the obvious. I am not too concerned about these vices. I believe that *doing* philosophy with a sense of its power can genuinely make a difference to us, to who we think we are, and what we think we are doing (even when we are doing philosophy ...). Philosophy then gets its hands dirty and *shows* us how things can be different, better even, more just, more humane, more beautiful.

It is from Foucault, for whom I have a great respect and admiration, that I take some of this picture of philosophy, although I have substantial disagreements with many of the general conclusions that have been extrapolated from his work. In his superb discussion of post-Kantian philosophy, 'What is Enlightenment?,' Foucault draws to a close by remarking:

> It seems to me that Kant's reflection [on the Enlightenment] is ... a way of philosophizing that has not been without its importance or effectiveness during the last two centuries. The critical ontology of ourselves has to be considered not, certainly, as a theory, a doctrine, nor even as a permanent body of knowledge that is accumulating; it has to be conceived as an attitude, an ethos, a philosophical life in which the critique of what we are is at one and the same time the historical analysis of the limits that are imposed on us and an experiment with the possibility of going beyond them.[2]

I am certainly not a structuralist, post-structuralist, nor a relativist (all labels applied misleadingly to Foucault), but this attitude to philosophy is one I embrace. Philosophy at its very best is like this. It has to be done with passion and a spirit of experimental adventuring. I believe this to be as true of metaphysics as it is of the history of the concept of madness—I know metaphysicians who would disagree! Now this really *is* tremendously exciting.

[2] Foucault, M. 'What is Enlightenment' in Rabinow, P. (ed.)(1984) *The Foucault Reader: an Introduction to Foucault's Thought* ed. Harmondsworth: Penguin Books, 50.

Such words probably seem out of place at the beginning of a thesis concerned with certain issues in the philosophy of biology. Furthermore, bold gestures often turn out to be empty, or trivial. However, I hope that this thesis is the start of a much larger project that articulates an approach to the dangers of some apparently real things, and some real things that we do not normally acknowledge. I am prepared to face the charge of being empty or trivial by indicating how the issues I confront here do have practical importance in contemporary life. We desperately need a philosophical examination of life that begins with our being situated in the biological world to a greater degree than ever before, given our scientific theories as descriptions of that world. If we are to understand what it is that we are, and what we are becoming, not only as the culture of Western thought, nor just as *Homo sapiens*, but also as individuals that are part of both of these larger groupings, this project has to recognise biology as one of the major components of what we now think we are.[3] This is the larger project.[4]

[3] I am not proposing that it is just our being 'embodied' that should be re-examined, although, as many recent studies have shown, this is a neglected area of research; see, for example, the very different approaches to bodies and human physicality in Harré, R. (1991) *Physical Being* Oxford: Blackwell; Lingis, A. (1994) *Foreign Bodies* London: Routledge; Ousdshoorn, N. (1994) *Beyond the Natural Body: An Archæology of Sex Hormones* London: Routledge; and the collection MacCannell, J. F. and Zakarin, L. (eds.)(1994) *Thinking Bodies* Stanford: Stanford University Press.

[4] I shall return to the larger project at the end of the thesis—the sketch I give is clearly *not* a detailed picture, as this would be inappropriate here. However, a further brief note is in order: Foucault spoke of the need for an analysis of biopower, the ways that our biological/medical knowledge of ourselves is used, underpins and manipulates debates involving almost all aspects of our culture, morality and society. I propose that there is now a greater need, as we face great advances in biology and its potential applications, to make explicit what we assume about biology in such talk, to prevent the misuse of this knowledge. This would be done through a new 'philosophy of life.' Understanding the philosophy of biology is the beginning of this project. It goes far beyond current concerns about genes and genetic manipulation and the undesirable re-emergent social Darwinism that one now sees in right-wing musings on social justice and responsibility.

To begin with biology then. Moving away from Foucault to a certain extent,[5] I would describe my approach to rationality and ontology as generally realist, if I could find a suitable definition of what this might mean. Crispin Wright's point that realism has two components—a modest one about the independent existence of whatever the domain one is a realist about, and a more presumptuous claim about the possibility of "getting at" this existent domain with our limited human resources, and then getting it *right*[6]—seems well made.[7] But the arguments become increasingly technical when one looks for the details of what these components might mean in practice. Some issues that are part of this debate will emerge in the body of this thesis, and at the end I shall approach the question of rationality and objectivity again. However, what I hope to present the reader with is an approach to philosophy of science that can incorporate aspects of science that recent work in biology and philosophy of biology have made apparent and which show us the necessity of retaining a realist perspective. Our motivations for finding out about the world are indeed value-laden and replete with our concerns with power, politics and social status. Politically we live a diverse world. The main point will be that disunity and pluralism about the world

[5] In his later writings there emerges a rather interesting account of how our various techniques and theories of the self should be understood in terms of their status, which cannot be just dismissed as relativism about rationality. As I shall argue throughout, Foucault's concerns are at the heart of our current worries about rationality and being. His emphasis on discussing *how* particular aspects of our supposed culture pose questions—about mental, criminal and sexual behaviour and being, for example—for our political life to attempt to resolve (but which it always fails to do completely), gives an overarching role to his philosophical thought. And in places it sounds as though *this* project has a fairly robust understanding of rationality at its core '... the work of a history of thought would be to rediscover at the root of these diverse solutions the general form of problemization that has made them possible—even in their very opposition; or what has made possible the transformations of the difficulties and obstacles of a practice into a general problem for which one proposes diverse practical solutions. ... this development of a given into a question, this transformation of a group of obstacles and difficulties into problems to which the diverse solutions will attempt to produce a response, this is what constitutes the point of problemization and the specific work of thought.' 'Polemics, Politics, and Problemizations—an interview with Michel Foucault' (1983) in Rabinow *op cit.* 389. There has been much discussion of this topic and I shall not pursue it here. See p.172 below.

[6] Wright, C. (1994) *Truth and Objectivity* Harvard: Harvard University Press. 1-2.

[7] Of course, this is not an uncontroversial position. Clearly Michael Dummett would argue otherwise; see Dummett, M. (1978) *Truth and Other Enigmas* London: Duckworth.

need not lead to an abandonment of attempts to be normative about what is best in our current methods and attitudes to that world and the sciences we use to describe it. We are finite, with limited powers in forming a picture of the world that is perspectival—only realists can make *this* claim.

Science dominates many aspects of Western culture and can quite properly inform, and be informed by the kind of reading of philosophy I have given so far. Science is, in the broadest terms, 'about' the world: there is no reason why seeing this *aboutness* as a complex thing should lead to our throwing up our hands in despair and turning to Paul Feyerabend to tell us that there is another reason for our doing science that is not part of trying to accurately understand the world.[8] The world may not be simple—why should we suppose that it is?

We stand on a threshold of startling innovations in biology.[9] Daniel Dennett's recent claims[10] for the traditional core of modern biology, evolution through natural selection, are enough on their own to suggest that it is a process of tremendous generative power. Evolution through natural selection challenges our traditional notions of mind, ontology, morality and meaning.[11] Add to this the burgeoning prospects for artificial life,[12] biotechnology,

[8] For a clear discussion of how to avoid some of the dangers of extreme views about methodological issues and the faults inherent in any complete social constructivist picture of science, see Gower, B. (1997) *Scientific Method: An Historical and Philosophical Introduction* London: Routledge, particularly chapter 12.

[9] For a speculative account of what some biologists think they might be able to do see Murphy, M. P. and O'Neill, L. A. J. (eds.) (1995) *What is Life? The Next Fifty Years: Speculations of the Future of Biology* Cambridge: Cambridge University Press.

[10] Dennett, D. C. (1996) *Darwin's Dangerous Idea: Evolution and the Meanings of Life* London: Allen Lane, Penguin Press.

[11] We need not go all the way with Dennett's account of mind to see this point. Indeed, I am fairly pessimistic that his program could ever produce a theory for the mind that was suitably robust in dealing with the vast array of human experience. My own experiences with Buddhist meditation have highlighted for me the difficulties of the so-called 'ineffable' in attempts to describe our experiences, let alone explain them.

gene therapy, cloning ... , and there emerges a new biology that is far more pregnant with changes to our understanding of what we are than the new physics could ever be. Quine, typically, suggested that '[p]hysics investigates the essential nature of the world, and biology describes a local bump ... '[13] But biology threatens to impact on our lives precisely because it is about *our* local bump.[14] Looking at one, admittedly hyperbolic claim, the conceptual transformations could be staggering:

> The human body ... is an architectonic compilation of millions of agencies of chimerical cells. Each cell in the hand typing this sentence comes from two, maybe three, kinds of bacteria. These cells themselves appear to represent the latter-day result, the fearful symmetry, of microbial communities so consolidated, so tightly organized and histologically orchestrated, that they have been selected together, one for all and all for one, as societies in the shape of organisms.[15]

In terms of current developments this lies alongside news that soon we shall be able to transplant the stem cells of human testes into animals—presenting us with the possibility of non-human animals fathering human children—and that Dolly the sheep is a clone. So one question that interests me is this: *If biology can change how we understand the basic concepts of what we are in relation to the rest of the living world and what that world of*

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[12] See particularly the splendid overview collection by Boden, M. A. (1996) *The Philosophy of Artificial Life* Oxford: Oxford University Press.

[13] Quine, W. V. O. (1981) *Theories and Things* Cambridge MA: Harvard University Press, 93.

[14] Many workers in A-Life would claim that is precisely because traditional biology investigates only the life of carbon chemistry that we need models of what other living systems could be like. Having a particular biochemical composition may be a very contingent matter: 'Organisms have been compared to extremely complicated and finely tuned biochemical machines. Since we know that it is possible to abstract the logical form of a machine from its physical hardware, it is natural to ask whether it is possible to abstract the logical form of an organism from its biochemical wetware. The field of Artificial Life is devoted to the investigation of this question.' Langton, C. G. (1996) 'Artificial Life' in Boden *op cit.*, 54. If such maps lead to the gold they promise then there is no grounding for the claim that biology can only be about a local bump—biology could eventually embrace *all* systems that have a universal feature, life.

[15] Sagan, D. (1992) 'Metametazoa: Biology and Multiplicity' in Crary, J. and Kwinter, S. (eds.) *Incorporation, Zone 6* New York: Zone, 365-366.

living things is, how will it affect our understanding of ourselves in relation to ourselves? And what kinds of people will there be? I am curious about how these questions might be answered through an exploration of the metaphysics and epistemology of the possibilities, rather than just the usual worries about ethics alone.[16] Which brings us back to the start of a metaphysics of the self, in the widest sense. In fact the questions that are important here are to do with understanding life *per se* where biology is but one crucial part. The following should help to lay down the framework of how I see biology's use in this respect. In dealing with three perspectives on biology as a science I hope to present a diverse science which has nevertheless real content and connections with all sorts of other knowledge gathering and theory generating activities.

I make no bones about drawing on writers and evidence from many different traditions in philosophy and biology. I hope I remain critical enough to tell the good from the bad, and open-minded enough to avoid dismissing ideas that were not originally expressed in English, or which have not yet been received into the cannon of academic analytic philosophy. I am also somewhat concerned that philosophy of science is often (thankfully not always) stylistically presented as though it were science. This is a consequence of the kind of questions that particular philosophers consider, but it does not help promote the idea that philosophy can generate new perspectives and ways of talking about important things. I have tried to avoid this 'scientific' way of writing philosophy.

Throughout the thesis I have placed short poetic pieces that I hope add to the argument, rather than distract the reader with their clumsiness. They are all my own work. Any attempt at tracing the influences on me in writing these would be a thesis and bibliography on its own. I considered using dialogue to illustrate some points, but felt that perhaps haiku

[16] *cf.* Glover (1984) *What Sort of People Should There Be?* Harmondsworth: Penguin Books.

would be more … experimental. The seventeenth century writer Basho, arguably the greatest Japanese haiku poet, instructed the writing of poetry:

> Go to the pine if you want to learn about the pine, or to the bamboo if you want to learn about the bamboo. And in doing so, you must leave your subjective preoccupation with yourself. Otherwise you impose yourself on the object and do not learn. Your poetry issues of its own accord when you and the object have become one—when you have plunged deep enough into the object to see something like a hidden glimmering there. However well phrased your poetry may be, if your feeling is not natural—if the object and yourself are separate—then your poetry is not true poetry but merely your subjective counterfeit.[17]

Basho's Zen insights are perhaps a little opaque to Western thinking, but the notion that knowledge must be more than just *intellectual* engagement, that it is genuine experience of reality, is a deeply fascinating one.

These opening remarks are intended as a polemic, a sketch of other hidden themes in this thesis, the content of which will be laid out more fully in *Biological Being* below.

i Acknowledgements

Four years seem a long time and also only a moment. Certain people have been with me all the way through this project, and without their friendship, support and encouragement I would never have completed one year. Indeed, it was only because of gentle persuasion from others, who carried my motivation for me, that I was able to return to my thesis after suffering with glandular fever in 1995. For their encouragement and help I would like to thank my parents, Pamela and John Mossley, and my friends Rachel Leonard, Jane Hunt,

[17] Basho (1966) *The Narrow Road to the Deep North and Other Travel Sketches* (trans. Nobuyuki Yuasa) Harmondsworth: Penguin Books, 33.

Chas Clapham, Ken Schenck and Mark Gay. These people are treasures in themselves, but in addition I have been lucky to be part of a superbly supportive group of postgraduate student friends in the Durham Philosophy Department. Barry Stobbart, Stuart Hanscomb and Paul MacDonald, with whom I worked on *Philosophical Writings* in 1996, together with Martin Connor, Bill Pollard, Alan Brown and Phil Diggle have all listened to cobbled together papers from this thesis with patience and enthusiasm. They have ensured that my impressionistic style did not completely negate any coherent narrative, and that I know the inside of *The Dun Cow* better than my own home.

I would also like to thank the Philosophy Department for financial backing from the Doreen Bretherton Studentship (1993-96) and money from the Alan Jemison Fund. The opportunity of teaching on the *Philosophy of Science* and *Knowledge and Reality* courses has also been important to me. Durham has a friendly and well integrated department that I shall miss very much when I do leave. Kathleen Nattrass' efficiency, good-natured patience and knowledge of *The Archers* are second to none. Similarly I would like to thank my old college, St John's. In particular I thank the Senior Tutor, Dr Margaret Masson for her understanding and support, especially in the early part of my research.

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My greatest thanks go to my partner, Lawrence, to whom this thesis is dedicated.

Haiku

DNA thinking

About DNA structure:

More alive than ever!

1 Biological Being

> It should be no embarrassment to philosophy that its conundrums often fail to engage the attention of working scientists. If philosophy is a discipline in its own right, this is to be expected. Nor should a misplaced scientism lead us to think that philosophy is barred from making normative claims by way of criticizing what scientists do. ...
>
> The idea that philosophy is an *a priori* discipline and the idea that it is simply a part of science are both wrong. Philosophy is not a unity; different philosophical problems are structured differently. Nor can one tell in advance how one philosophical problem is related to others, and to matters that arise in other arenas of thought. We should relish the fact that philosophy can be *surprising*. Understanding the nature of a problem is not something we do in advance of trying to solve it.[18]

There is no problem with biology. I am not proposing that there is something wrong that needs fixing with biology *per se*. What I address here is a small part of the philosophical picture of biology that will allow us to begin properly to embrace and assess the vast possibilities that confront us. Before we can start understanding what we are to do with the new possibilities that biology generates we need to understand, at least in some basically comprehensive way, what we can do. That is, we need to see what our contemporary philosophical approach to biology is in relation to the other sciences and our other knowledge gathering and generating practices—this is what this thesis is about. I am not trying to 'understand the nature of a problem ... in advance of trying to solve it,' merely showing how we might combine a number of important discussions about biology to clarify where we now are. Along the way I discuss some very different notions of scientific real-

[18] Sober, E. (1994) *From a Biological Point of View: Essays in Evolutionary Philosophy* Cambridge: Cambridge University Press, 3.

ism, whilst supporting scientific realism[19] as an unavoidable and largely uncontroversial consequence of taking a broader look at the philosophy of science than is usual: one that incorporates theory *and* practice. I shall examine three different perspectives on the life sciences, and show how they all should be taken as a piece. Without other perspectives there is a tendency to over-simplify *all* the special sciences and thereby make mistakes in assessing the weight to assign to philosophically significant parts of scientific practice. Roughly, these perspectives are methodological, metaphysical and (culturally) epistemological in nature. There are two linking themes to these perspectives. The first, as I have already mentioned, is scientific realism, in particular Ian Hacking's experimental realism. The second is the (dis)unity of science, focusing on debates between John Dupré and Alexander Rosenberg over biology and its status in relation to a realist approach to the physical sciences. These themes, these strands, as we shall see, are entwined in a complex way. The use of the three perspectives highlights what we can learn from a range of thought on science, biology, and the uses of scientific knowledge. But it is not necessary that we can untangle the relationship between these strands. Social and political concerns have a role but they do not negate the rational goals of the scientist. Demonstrating that from each perspective there are means for determining good, rational science and scientific practice will be enough. The dependence of the three perspectives' realism on each other supports rather than undermines realism. What I mean by this will become clearer as we proceed. As a consequence I do not consider this to be a thesis in the philosophy *of* biology, rather a thesis in philosophy *about* biology.

[19] I shall more closely address the nature of this scientific realism in the context of global realism issues at the end. I leave it to the reader to judge whether I would be better off taking the label 'pragmatist' after I have discussed the philosophical positions of Putnam and Rorty in the context of discussing 'metaphysical' realism, rationality and objectivity in Chapter 5.

i Outline

Chapter Two is about experiments and opens my discussion of Ian Hacking's philosophy of science. In it I explore some contemporary approaches to experiments in methodology to see whether they offer any substantial advances in our understanding of science practice. I offer some suggestions for what experiments tell us and how they might augment discussions about realism. I introduce the notion of metaphysical disunity in biology and contrast it with *methodological* disunity with which it can be confused. Hacking's thoughts on scientific realism emerge as important philosophical contributions to thinking about experiments and realism, but are shown to be incomplete. Without a metaphysical context in which to apply an agent perspective description for particular experiments, it is unclear just what kinds of things are supposed to be real. I conclude the chapter with some examples of experiments in biology and human genetics that illustrate a diversity of experimental techniques and some overlap of methodology with experiments in the physical sciences. Disunity of things does not imply disunity of actions and methods; but disunity of things is a stronger thesis than mere ontological pluralism.

Chapter Three then focuses on the details of the second theme—that of possible *metaphysical* disunity in science—which emerges from analysis of biology's own diverse ontology and varied taxonomies. This chapter is most obviously drawn from topics that have been at issue in the philosophy of biology for many years: reductionism, especially about how one might interpret the claims of genetics,[20] the status of essentialism and natural kinds, and attempts to place the entities that biology discusses in a reasonable relation with

[20] The philosophical debate about what genes 'mean' has some interesting parallels with the decades old discussion of how quantum mechanics might be interpreted. I leave it to the reader to fill in the detail here.

those discovered by physics. Here John Dupré's *The Disorder of Things*[21] and Alexander Rosenberg's *Instrumental Biology or the Disunity of Science*[22] are the basis for this discussion. I argue that we cannot properly decide about the metaphysical status of the kinds and entities biology postulates without additional information about their place and use in our understanding of ourselves.

Chapter Four explores further some of the issues raised earlier about biology and intervention in the world, about what we learn when we create and use phenomena to create other things. It concludes my discussion of the philosophy of science presented by Ian Hacking. He presents us with the possibility that biological kinds are constructed in much the same 'nominalist' way that the categories of social science are in his reading of Foucault. And yet they do appear to fit his own criteria for experimental realism. I present a better interpretation of Foucault that is sensitive to his own stated aims and motivations for his whole philosophical project. This gives us a solution to how we might incorporate worries about the uses of biological knowledge into a philosophical picture of real manipulation of entities in a pluralist metaphysics. I then illustrate my argument with analysis of a recent study of the history of sex hormone research.

Chapter Five is in two parts. Firstly, I tie together the arguments presented. No answers can be given to our questions about scientific realism and the (dis)unity of science from any single perspective. The biological sciences do not absolutely fail to fit into the philosophy of experiment that emerges from physics-centred theories, but neither do they result in a smoothly structured ontology. The methodological considerations throw us back on deep

[21] Dupré, J. (1994) *the Disorder of Things: Metaphysical Foundations of the Disunity of Science* Cambridge, Massachusetts: Harvard University Press. Referred to hereafter as *Disorder*.

[22] Rosenberg, A. (1994) *Instrumental Biology or the Disunity of Science* Chicago: Chicago University Press. Referred to hereafter as *Instrument*.

questions about our being the kind(s) of beings we are in a material world. And the consequent ontology will be complicated and disunified. Consideration of experimentation show there must be some unity, whatever minor differences there are in the methods employed, but how this is articulated is not just an issue in philosophy of science. Practice is important for realism. Metaphysics is important for practice. And an understanding of the context of application is important for metaphysics. Given this, scientific realism then emerges as a position that cannot be extracted from any of the perspectives alone. However, we are left with the questions about rationality that I take to underpin my epistemology. So the second part of Chapter Five explores what kind of philosophical context could support the position I describe for comprehending realism in science. I use the ongoing debate between Hilary Putnam and Richard Rorty to provide a context for this. Of necessity *this* discussion is inconclusive.

There then follows a concluding Postscript chapter in which I return to the themes I have stated in my Preface. With a broader philosophy of biology now a little clearer, I sketch how the debate might proceed and what will be important for future research.

ii Philosophy and Science

By now it should be obvious that I certainly believe that there is much that philosophy can contribute to discussions in and about science. Although I am not in total agreement with some of his more general claims, Philip Kitcher's *The Advancement of Science: Science without Legend, Objectivity without Illusions*[23] is a clear and well argued demonstration

[23] Kitcher, P. (1993) *The Advancement of Science: Science without Legend, Objectivity without Illusions* Oxford: Oxford University Press. Referred to hereafter as *Advancement*.

that claims about the recent abdication of philosophy and the coronation of sociology are misguided:

> ... philosophical reflections about science stand in relation to the complex practice of science much as economic theory does to the complicated and messy world of transactions of work, money, and goods. Much traditional philosophy of science, in the style of some economic modeling, neglects grubby details and ascends to heights of abstraction at which considerable precision and elegance can be achieved. We should value the precision and elegance, for its own sake, for its establishing standards against which other efforts can be judged, and for the possibility that extreme idealizations may lay bare large and important features of the phenomena. But like ventures in macroeconomics, formal philosophy of science inevitably attracts the criticism that it is entirely unrealistic, an aesthetically pleasing irrelevancy. To rebut such charges—or to concede them and to do better service to philosophy's legitimate normative project—we need to idealize the phenomena but to include in our treatment the features that critics emphasize.[24]

This I have tried to do. I address real concerns about the construction of scientific knowledge based on socio-political needs in the very area where I feel many future dangers lie, that is, in biology; I am concerned with the details of practice in the laboratory and field; and I take on board the diversity of contemporary biology itself. However, in the sense that I believe there is still a genuine conceptual task in which we must engage—that is, looking at the limits and possibilities of what we might think and do—philosophy in science and in relation to science retains, in a general sense, a modernist role, albeit in a contingent form.

iii Words

Finally a note on terminology. I shall use throughout, except where explicitly stated, 'biology,' 'biological science,' 'the biological sciences' and 'the life sciences' to mean the same thing, namely 'the study of living organisms, including their structure, function, evo-

[24] *ibid.* 10.

lution, distribution, and interrelationships.'[25] (Perhaps it would be better to say 'systems' rather than 'organisms' so as not to exclude the possibility of life not dependent on carbon compounds we find on Earth. However, on the whole, biology is about life in the form of carbon based organisms with general dependence on DNA and RNA.[26]) I include in this definition ecology, although I shall discuss some features of this field separately. I have not made finer distinctions because they would not have added to my argument as such (in part it would have pre-empted some points of issue), and in most cases (beyond general special science distinctions) the arbitrariness of divisions between fields of enquiry dissolves any philosophical value in labelling a laboratory for one purpose rather than another.[27] I do discuss some differences between theories about genes and heredity at the level of biochemistry and molecular biology on the one hand, and Classical Mendelian theory on the other; the difference here *is* one of philosophical importance, as we shall see. At the end of my discussion there will emerge a good motivation for some detailed analysis of whether the philosophical content of neighbouring fields of any domain of study really are as similar as they are supposed to be here: this will be argued for, rather than being presumed at the outset.

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[25] *Collins English Dictionary* (1979) Glasgow: Collins.

[26] Having said this, it is quite clear that defining 'life' is particularly difficult task in itself. This does not detract from biology as a serious study, but it certainly adds to its philosophical interest. Again, this is one of the motivations for A-Life research. It also connects my thoughts to Aristotle, who perhaps comes closest to exploring a philosophy of life in the way I envision: *De Anima* lurks, largely unacknowledged, in the shadows—stripped of its vitalism to be sure!

[27] Of course, field boundaries are serious and can result in peculiar miscomprehension between scientists who have been taught or developed idiosyncratic techniques and expressions specifically for one project when discussing essentially similar material with scientists from another group. Field boundaries and distinctions are also important, therefore, for understanding the sociology of science and its history.

Haiku

Picking spring flowers

To dissect their colours

I change the world.

2 Experiments

Philosophers long made a mummy of science. When finally they unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960.

It was a crisis because it upset our old tradition of thinking that scientific knowledge is the crowning achievement of human reason. Sceptics have always challenged the complacent panorama of cumulative and accumulating human knowledge, but now they took their ammunition from the details of history.[28]

i Introduction

The revolution of thought surrounding the nature of science, its status, theories, laws and methods, that occurred 'around 1960'[29] has run its course. The historical strategies employed by Hanson, Kuhn and Feyerabend, three of the heroes of the revolution, have transformed the philosophical debate about science in ways that are well known and now part of the intellectual history of the twentieth century. They made complex discussions about the logic of confirmation, of cumulative and progressive science seem, for a time, archaic and redundant in the face of uncertainty and unspecifiable change. They questioned the notion of a fixed, timeless rationality to which science could both appeal and contribute. They made us look at history as more than a 'repository for ... anecdote or chronology.'[30] Above all they

[28] Hacking, I. (1983) *Representing and Intervening* Cambridge: Cambridge University Press, 1. Referred to hereafter as *R&I*.

[29] This date seems to me to be inaccurate. Although Kuhn's *Structure* was originally published in 1962, and the fallout from Wittgenstein's work was beginning to have a profound influence on the understanding of how science talk could be understood before then, genuine debate over the consequences of the revolution, and thereby the actual crisis in thought, did not take place until the late 1960s and throughout the '70s.

[30] Kuhn, T. S. (1970) *The Structure of Scientific Revolutions* Chicago: Chicago University Press, 1.

challenged a generation of philosophers who had spent their best energies defining and refining a picture of science that could not, according to the revolutionaries, be acceptable, for it made no reference to the changing, contingent nature of scientific knowledge and practice, past and present, nor to the diversity of knowledge gathering activities human beings employ.[31]

This is how the history of the revolution is usually sketched and it serves as a beginning, however inaccurate, because the revolution over the use of *history* has ended.[32] The history of science has its own ministry in the new government. There is now little to doubt in the statement, 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind.'[33]

Consequently it was something of a surprise when Ian Hacking pointed out in 1983[34] that on further analysis there was little to choose between the two camps before and after the storming of the Bastille to "liberate" rationality from the tyranny of philosophical objectivity. Both

[31] There is a second wave to this revolution. It concerns the social construction of knowledge. My stance to the Edinburgh School and its off-spring will become clearer in what follows. Whereas Kuhn *et al.* raised serious points of issue about the historical placing of knowledge from a variety of theoretical perspectives, the contentious Barnes/Bloor reading of Wittgenstein is not sufficient to ground their claims for the social construction of all scientific knowledge. I shall discuss this point in more detail in connection with an account of Michel Foucault's criticisms of our assumptions about natural, social and medical scientific knowledge. See footnote 41.

[32] See p. 46 for discussion of how one might read Kuhn's concept of history and its role for philosophy of science and why, despite his best efforts to draw attention to his theories' application to the natural sciences, it is in the arena of social science only that he can be said to have made a genuine impact on science practice.

[33] Lakatos, I., (1970) *PSA 1970, Boston Studies in the Philosophy of Science VIII*, 91. One wonders whether anyone truly doubted it at all. Kuhn's contribution to the historiography of science was certainly important, but the editors of the *International Encyclopedia of Unified Science*, which originally published Kuhn's *SSR*, who included Otto Neurath and Rudolf Carnap, could not be accused of being historically ignorant.

[34] Allan Franklin ((1993)'Experimental Questions' *Perspectives on Science* vol. 1 no. 1, 127) has suggested that it was Hacking's 1981 article 'Do we see through a microscope?' (Hacking, I. (1981a) *Pacific Philosophical Quarterly* 62, 305-22) that 'started the process of redressing the balance between experiment and theory in the history, philosophy and sociology of science.' However, it seems to me that it was only with the more direct analysis of experiment as a whole in *Representing and Intervening* that he brought to the fore the precise nature of what actually needed to be redressed.

sides seemed obsessed with theories and their meaning, how they refer, how they change. Neither had really got to grips with the actual practice of scientists, particularly the practices in experimentation. He noted that:

> Philosophers of science constantly discuss theories and representation of reality, but say almost nothing about experiment, technology, or the use of knowledge to alter the world.[35]

He was not entirely alone in this philosophical observation[36] but pressed the point into significant philosophical use in the debate about how realism is to be understood in relation to scientific practice:

> Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be 'observed' are regularly manipulated to produce a new phenomena (sic) and to investigate other aspects of nature. They are tools, not for thinking but for doing.[37]

This has seemed a second minor revolution and has prompted a number of philosophers to re-examine experiments. Hacking considered this a 'Back to Bacon' campaign, but the philosophy that has emerged has been of a different kind than Francis Bacon's attempt to lay down the foundations of a fixed epistemology of experiments, recipes for experimenters and fact gathers. Theories about the epistemology, ontology, logic, history and sociology of experiments have abounded, produced by philosophers, historians and sociologists alike. So many older, well-established issues have found new life in the context of the philosophy of experiments that this revolution itself seems to have lost some of its force. But quite clearly experi-

[35] *R&I*, 149.

[36] Nancy Cartwright's work on the phenomenological nature of scientific laws dovetails with Hacking's thoughts to articulate an anti-Humean attitude to causality and probability, and to separate talk of the truth of theories from the reality of entities, see Cartwright, N. (1983) *How the Laws of Physics Lie* Oxford: Clarendon Press. As will also become apparent, I share many of Cartwright's worries about the universality of fundamental physical laws, not to mention biological and psychological ones, but I have serious doubts about a simple divide between theory and entity realism, for any domain.

[37] *R&I*, 262.

ments are a fundamental part of much of science, and I want to be open to the suggestion that philosophically significant issues are being missed by ignoring or dismissing them. In looking at some parts of the current state of the debate about experiments, I focus on the role of experimentation in science as a metaphysical and epistemological issue, to show how it is at the heart of a wider notion of intervention and manipulation, which can be *part* of a defence of a form of scientific realism, but which is inadequate on its own. Ultimately this will demonstrate how new tools and methods can be accommodated in philosophy and philosophy of science without the end of anything, be it history, epistemology or philosophy itself. Indeed, since part of my thesis is an examination of fresh claims about the disintegration of the metaphysical framework for an assumed unity to science that arises from science itself, *something* does need to be said about how these arguments are manifest in practical terms.[38] Add to this the importance of biology, and biology as an experimental and world manipulating science, and such an examination has become imperative, and long overdue in biology. On their own these seem to be good enough reasons to look again at experiments, but they need clarifying.

1 The importance of practice

Understanding science as more than just its conceptual product, knowledge, has been the motivation behind the various fields of what has come to be called 'science studies,' whether this is seen as a philosophical, sociological or historical exercise. As noted, concern for science as a practical enterprise has produced interesting, diverse and controversial results that have influenced our understanding of science-as-knowledge and the traditional ways that philosophy has engaged with science, both descriptively and normatively. Similar stress on practice can

[38] Dupré does not discuss experiments at all in *Disunity*. In a recent paper presented to the *British Society for the Philosophy of Science* he suggested, rather more as a hope than a direct claim, that philosophical examination of experimentation would reveal a disunity of practice to match his theoretical analysis. As I show below, combining current analysis of experiments with examples from the biological sciences, provides only partial fulfilment of this hope. This does not undermine the notion of ontological disunity in science.

be found in the works of other philosophers of science, of course, most notably Ian Hacking, Andrew Pickering and David Gooding[39] and I draw on their work heavily, but I hope that my approach will be distinguishable from these others. Of experimentation David Gooding has said:

> Analytic philosophy views the relationship between theory and experiment as a logical relationship between propositions. So experiment must be a means of generating observation statements which bear a logical relationship to statements derived from theory. ... This focus on explicitly represented knowledge implicitly proscribes consideration of the *other sorts of stuff* with which science is made: instruments are invisible or feature at best in a subsidiary way, as merely practical means to theoretical ends; observers' agency figures not at all.[40]

This is true of traditional descriptions of practice in science generally. Before going further we need to make some preliminary distinctions. When we speak of agency there are two possible points of view, the human and the non-human. The sociology of scientific knowledge (SSK) movement championed by, amongst others, Barry Barnes, David Bloor and Harry Collins,[41] has been at the forefront of science studies' concern with viewing *all* of science practice from the perspective of *human* agency. The questions that these sociologists ask are of the form "How do we explain this or that practice in terms of the interests and motivations of the people involved?" But this seems to imply that the complex machines that test, measure, record and manipulate the stuff of the world are invisible and entirely fluid in their behaviour—to say nothing of the material world itself. Machines need tuning and fixing and understanding. They can play a large role in just how an experiment is conducted. If the experimental set-up has to

[39] See Hacking, *R&I*; Gooding, D (1990) *Experiment and the Making of Meaning* Dordrecht: Kluwer Academic Press; Pickering, A. (1995) *The Mangle of Practice: Time Agency and Science* Chicago: Chicago University Press.

[40] Gooding, *op. cit.* 9.

[41] See, for example, Barnes, B. (1977) *Interests and the Growth of Knowledge* London: Routledge; Bloor, D. (1976) *Knowledge and Social Imagery* London: Routledge; Collins, H. (1985) *Changing Order: Replication and Induction in Scientific Practice* Beverley Hills: Sage.

be changed, even slightly, to accommodate the particular X-ray detector a biologist is using to track potassium ion flow through a semi-permeable membrane, then what has been called 'material agency' has a role to play in correctly analysing the experiment. There has been some discussion about the coherence of material agency, SSK supporters claiming that only human agency can make any sense of understanding science from outside science's own interests. Material agency, they say, is in the realm of science itself, so no self-respecting science critic should call on it. I shall not pursue the details of this position here. However, as a sketch of a simple reply, let us note that the argument already presupposes there is a known, predictable and largely transparent quality to machines, scientific apparatus and the world. As Andrew Pickering points out:

> The contours of material agency are never decisively known in advance, scientists continually have to explore them in their work, problems arise and have to be solved in the development of, say, new machines. And such solutions—if they are found at all—take the form, at minimum, of a kind of delicate material positioning or tuning, where I use "tuning" in the sense of tuning a radio set or car engine, with the caveat that the character of the "signal" is not known in advance in scientific research.[42]

So when I speak of agency I am referring to both human *and* non-human agency in science. Human interests and concerns for social, political and cultural ends in science practice should be considered alongside the ways that the materials and apparatus of research imposes its own "interests." There aren't just "people doing things," but *people doing things to stuff that does things*.

But the question remains: why should we, as philosophers, be looking at practice at all? Are not the conceptual and cognitive components of science, their derivation, inter-relationships, epistemological and metaphysical consequences the essence of philosophy of science? It is

[42] Pickering, A. (1995) *The Mangle of Practice* Chicago: University of Chicago Press, 14.

worth going over some of the arguments in favour of practice to make explicit the premise to which I have laid claim: namely, that experiments[43] are a philosophically significant aspect of contemporary scientific research.

Firstly,[44] what scientists do is interesting—at least to me it is. Through work on the philosophy of biology, I have come to see that the detail of what scientists do and what they think they are *doing* is as worthy of study as what they *think*, especially if one is worried about science and public policy. If we are truly concerned with the ethical consequences of the Human Genome Project, for example, it seems only proper that we should be as concerned with what is done, and what we are in fact capable of doing with DNA and genetic material (in some detail), as we are with the supposed knowledge that such an investigation claims to produce. There is a significant gap between the plethora of suggestions for what the Genome Project's final knowledge bank might mean for us as individuals in the future, and what, in fact, tax-payers' money is spent on now.[45]

Secondly, it is possible to look at many of the important philosophical positions and puzzles about science from the point of view of practice. Throwing aside our Humean prejudices—as Nancy Cartwright advises in the context of understanding science as a process of measure-

[43] That experiments are complicated and very diverse is an important consideration, but stating this is not to prejudge an answer to the question 'What is an experiment?,' merely an informed observation

[44] I am closely following Pickering's own reasoning in the Introduction ('From Science as Knowledge to Science as Practice') to *Science as Practice and Culture* but with additional arguments and examples.

[45] For a cogent account of what the researchers in the project are actually investigating see Kitcher, P. (1995) 'Who's Afraid of the Human Genome Project' *PSA 1994 Volume Two* Philosophy of Science Association, 313-321. Whatever the ethical difficulties that arise in the end, the funding governments are not told that all the important information obtained in the short and medium term will be about the genomes of the yeast *Saccharomyces cerevisiae* and the nematode worm *Caenorhabditis elegans*. At current rates of progress it may well take a century to completely map the human genome. For a good overview of worries about the application of the information from the project and related gene technologies see, Kitcher, P. (1996) *The Lives to Come: The Genetic Revolution and Human Possibilities* Harmondsworth: Penguin Books.

ment and quantification[46]—we see that agency can be made to work well in describing with considerable accuracy and with notable explanatory power, the causal connections and commitments that underpin our investigations of the parts of the world we find interesting. Connections have been made in the opposite direction too. For example, Alan Franklin[47] has carried out a number of studies which apply a Bayesian analysis to experiments in high energy particle physics in order to demonstrate how rational decision making by experimenters can be reconstructed to reveal a path from theory and experimental set up to results and conclusion.

Thirdly (again I am following Pickering's discussion here), there is much to be said for a consideration of practice as a point of dialogue where traditional discipline boundaries begin to dissolve. Pickering argues this point in relation to philosophy, sociology and history, but I think it may also serve as a corrective within philosophy of science itself. Too often it can appear that philosophy of science is becoming a kind of "*ersatz* science" where philosophers are struggling to make a contribution to the actual sciences they investigate.[48] Overly technical involvement with the *theoretical* problematic of a science can quickly lead one away from the central philosophical problems that emerge in higher level, more abstract discussions of that science. This might seem to contradict what I have already said about a need for philosophers to get involved in the details of actual practice. There is, of course, no conflict here. While there may be a known quality to the kinds of technical problem that physics or biology throws up for the philosopher of science from the perspective of theory, there is no reason at all to suppose that an examination of practice will be most successfully carried out using *any* of the concepts familiar to the scientist. So that, although details of experimentation may be the be-

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[46] See Cartwright, N. (1989) *Nature's Capacities and their Measurement* Oxford: Clarendon Press.

[47] See, for example, Franklin, A. (1986) *The Neglect of Experiment* Cambridge: Cambridge University Press. See p. 57 ff. below for an outline and discussion of his use of Bayesian methods in analysing experiments.

[48] I am in debt to the *History of the Philosophy of Science* discussion group on the Internet for focusing this thought.

ginning of a philosophical discussion, one very quickly runs out of resources without reference back to fundamental issues in epistemology, philosophy of mind and action, phenomenology and metaphysics. This will help to revive what is becoming a fragmented area of philosophical research.[49]

Finally, related to the previous point, the discussions that are taking place about practice are addressing some very deep matters that are of concern to anyone engaged in contemporary thinking in its widest sense: how to comprehend and perhaps dissolve the relationship between subject and object, between nature (Nature), environment and society, between individuals, communities and institutions.

2 Hacking and scientific realism

If a belief in the importance of experiments is my first premise, my second premise or commitment is that talking about scientific realism is also meaningful and worth doing. My initial comments should make it clear that only later will I be able to present an acceptable defence of this position. But in one sense *scientific* realism just is the dull thesis that realists are always saying it is. That is, the scientific realist is simply affirming that when a scientist says a particular theory is a good description of the world, or that a particular entity or property of an entity in fact exists, we should initially interpret the scientist as literally meaning what she says. It is the responsibility of the anti-realist to make their case against this obvious interpretation. This is just being a scientific realist *in* science.[50] In this sense we can quite coherently accept that there might be any number of ways that science could get at the causal structure of the world, and even that a single discipline could contain within it a number of different scien-

[49] I am not suggesting that *all* philosophy of science is "ersatz science," of course, just that, at present, the majority of papers at the philosophy of science conferences I attend seem to be of this kind!

[50] For the distinction between realism *in* science and realism *about* science see Hendry, R. F. (1995) 'Realism and Progress: Why Scientists should be Realists' in *Philosophy and Technology* ed. Fellows, R. (Royal Institute of Philosophy Supplement: 38) Cambridge: Cambridge University Press.

tific descriptions of an entity or phenomenon that need not be reducible to another. Dupré's promiscuous realism provides such framework for realism about the entities described by biology and I shall look at his position in some detail.[51] What is important is finding a way to defend scientific realism *about* science—that is, to say something about the way that science is conducted, something about method and its justification.

Since it will be important throughout the rest of the following discussion, let us lay out the commitments in Hacking's experimental realism.[52] There are certain key distinctions to be drawn. The first is between experimentation and observation. Crudely put, 'experimenting is not stating or reporting but doing—and not doing things with words.'[53] There is a basic non-theoretical engagement with the material stuff of the world that separates experimentation from observation. A second important aspect of experiments for Hacking is that they are used to make new phenomena. In *Representing and Intervening* he says:

> *One* role of experiments is so neglected that we lack a name for it. I call it the creation of phenomena. Traditionally scientists are said to explain phenomena that they discover in nature. I say that often they create the phenomena which then become the centrepieces of theory.[54] [My stress.]

Far from this just being only one of the roles of experiment it soon becomes clear that Hacking regards it as a necessary condition for experiments:

> There is no more familiar dictum than that experimental results must be repeatable. On my view that works out as something of a tautology. Experiment is the creation of phenomena; phenomena must have discernible regularities—so an experiment that is not repeatable has failed to create a phenomenon. ...

[51] Chapter 3, p. 88 ff..

[52] Critical discussion will come later. See pp. 31 ff., 74 and 190 ff. in particular.

[53] *R&I*, 173.

[54] *ibid.* 220.

> To experiment is to create, produce, refine and stabilize phenomena.[55]

The refining and stabilising of phenomena are crucial here. If there is no stability, there is no repeatability and hence no experiment at all. But the creation of a phenomenon is not enough justification for the experimenter to believe in the reality of whatever entities are postulated to explain the phenomenon. According to Hacking the experimenter must *use* the entity in further experimental conditions to create yet more new effects. That is, the experimenter needs to engage with the causal properties of the entity. To repeat the quotation from earlier:

> Experiment work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be 'observed' are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing.[56]

Important in all this is Hacking's attempt to distance scientific realism from its 'traditional,' representational roots.[57] What is interesting in his account of experimentation is that he claims that it is *only* by looking at what we do that a sense of the real can be gained. Roughly speaking, he thinks that the attempts to extract realism from theory alone are always doomed to failure. He says:

> By attending only to knowledge as representation of nature, we wonder how we can ever escape from representations and hook-up with the world. ... The harm comes from a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment.[58]

And that:

[55] *ibid.* 229.

[56] *ibid.* 262.

[57] The whole *Representing* part of *R&I* is a about the failure of philosophical discussions about the truth of scientific theories considered only as theories.

[58] *ibid.* 130-131.

> There is an important experimental contrast between realism about entities and realism about theories. Suppose we say that the latter is belief that science aims at true theories. Few experimenters will deny that. Only philosophers doubt it. Aiming at the truth is, however, something about the indefinite future. Aiming a beam of electrons is using present electrons. There is in contrast no set of theories one has to believe in. If realism is a doctrine about the aims of science, it is a doctrine laden with certain kinds of values. If realism about entities is a matter of aiming electrons next week, or aiming at other electrons the week after, it is a doctrine much more neutral between values. The way in which experimenters are scientific realists about entities is entirely different from ways in which they might be realists about theories.[59]

So Hacking has already dismissed the possibility of salvaging realism from the wreck of theory, and despite his warnings about obsessive behaviour, he is over-keen to tie scientific realism solely to practice. However, a simple divorce of scientific realism from its theoretical origins will require more argument and evidence than Hacking has offered to date—if it is possible at all.[60]

His more recent work has produced a detailed breakdown of the components, human and material agency included, that go into making physics experiments work. For example, in 'The Self-Vindication of the Laboratory Sciences'[61] he identifies fifteen different elements of experiments which he lists under the headings 'ideas,' 'things' and 'marks.' He is concerned to demonstrate the extent to which practice and theory are tailored to each other to provide a stable context for experiments to *work at all*. Having said this, he still insists that our ability to intervene in, and use parts of the world, to utilise the material agency of entities for our own ends, is as much an indication of that entity's reality as anything. However, the concept of manipulation/intervention that underpins his account of realism remains largely unexplained,

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[59] *ibid.* 263.

[60] See 190 ff. for my conclusions about this point.

[61] Hacking, I. (1992) 'The Self-Vindication of the Laboratory Sciences' Pickering, A. (ed.)(1992) *Science as Practice and Culture* Chicago: Chicago University Press. 29.

except in so far as it requires a belief in a real causal nexus. Since this is just the kind of larger philosophical issue with which I think philosophy of science should become engaged, I shall need to spend some time looking at this idea, with special reference to agency.

Furthermore, from this we also see two forms of scientific realism. One from the perspective of theory that says we should support the scientists' claims for the truth of their theories or the reality of their entities as a starting position, and a second from practice that says we should accept the reality of entities that are regularly used and manipulated to produce new, interesting or useful effects.

3 History and further thoughts

The history of how the state of affairs concerning experiments has been understood is not uniform. Philosophical analysis has varied greatly at different times since the sixteenth century,[62] and certain figures stand out for simply addressing the issue of how experiments are to be understood, when other thinkers and practitioners were happy to continue with their work without reflection on the methods and tools they used. In raising the possibility of finding out about the world by direct question and intervention, Francis Bacon *is* significant. It is easy to discount the apparent superiority of experimentation over 'passive' observation as obvious, given the wealth of evidence now available to us that experiments are useful and insightful: Bacon had comparatively parlous accounts of the empirical advantages experiments could afford.[63] Similarly, Claude Bernard in the field of physiology and 'experimental medicine' in

[62] It is interesting to note, however, that Roger Bacon says in the thirteenth century: 'There are no lectures given in experimental science either at Oxford or at Paris and this is a shameful thing because experimental science is the mistress of the speculative sciences, it alone is able to give us important truths within the confines of the other sciences, which those sciences can learn in no other way.' *The Opus Majus of Roger Bacon* trans. Robert Belle Burke quoted in Ayer, A. J. and O'Grady, J. (1992) *A Dictionary of Philosophical Quotations* Oxford: Blackwell, 34. This suggests that the current history of experiments that is only now being explored will need to include analysis that looks much further back than is usually assumed.

[63] See, for example, Gower, B. S. (1997) *op cit.* chapter 2.

the nineteenth century must be highlighted for his concern for the methodology of his field.[64] However, it is beyond the scope of my aims here to trace these threads in detail.

There now also exists a growing body of literature about the philosophy and history of experiments from late twentieth century writers. Here, however, as suggested earlier, other matters are entangled with the debate; it is sometimes hard to separate talk about experiments *qua* experiments from the use of such philosophising for other means. One area of philosophy of science that has had a minor renaissance through experiments is confirmation theory and the analysis of probability—*pace* the Kuhnian revolutionaries. This is largely a product of the apparent relative ease of access to the decision procedures and actions of scientists in experimental situations. It would seem that the use of extremely complex (and expensive) apparatus in modern science requires the operators to specify at every stage of their work *exactly* what it is they are doing; the ubiquitous nature of modern research groups (and international groups of groups such as constituted the Human Genome Project at its inception) make this doubly imperative.

From the contemporary literature it might seem that almost all of experimental work is restricted to physical science. As soon as any attempt is made to examine 'real life' experimentation one is overwhelmed with the diversity of experiments, *not just in science* but in our everyday talk and activity. It is necessary, therefore, to ask whether there is a single unified account of experiments available at all, and whether such an account is *desirable*. Doing this will help clarify and judge any general thesis about the unity of science; we will have a new focus, apart from the separate technical questions that arise from examination of theory alone. This generates further ripples into the metaphysical underpinning of our thoughts about science in our culture, ripples which reveal some disturbing tendencies to misunderstand the

[64] Bernard, C. (1957) *An Introduction to the Study of Experimental Medicine* New York: Dover.

power and possibilities for change that science can generate, particularly in the biological sciences.

4 Introduction summary: theory and experiments

In examining the work of a selected range of thinkers on experiments I intend to follow two linked theses through different approaches to science and experiments. The first is that intervention/manipulation is a key epistemic tool that is embedded in science and explored in science through experimentation. This picture is not unique, having antecedents in many thinkers some of whom will be discussed below. David Gooding says something similar when he writes:

> According to the received philosophical view, natural phenomena are bounded by theory. I shall argue that natural phenomena are bounded by human activity.[65]

There are no revolutions here, only a weaving together of threads from recent philosophy of science that produce theories and perspectives of genuine contemporary concern. It may seem that this neglects, or at worst obfuscates, the more obvious questions about the direct relationship between experiment and theory in science from which Hacking wants to move away. How does a scientist frame the questions she wishes to 'put to nature'? Just how do experiments confirm or falsify theories? And so on. These questions *are* important, but they are questions that will have to be reassessed if content cannot be given to methodological unity that is supported by material from the philosophical examination of experiments. So it is worth looking at this notion's foundations first. In any case, it is partially from such questions and the *variety* of answers (and the diagnoses of neglect of other issues involved) now available that my current concerns have arisen.

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[65] Gooding, D. (1990) *Experiment and the Making of Meaning: human agency in scientific observation and experiment* Dordrecht: Kluwer Academic Publishers, 9. I am not in agreement with Gooding on a number of points concerning what this might mean however.

Philosophical work more obviously dealing with the relationship between theory and practice has been used to provide a background to some of this chapter so that the shift away from the traditional view is visible and the outcome mapped. Even the more abstract aspects of talk about experiments can be seen to have political and moral components and implications. Simply put, to begin weighing the merits of actions, knowledge is required of what, why and how. That knowledge is not inaccessible to our rational processes for understanding the world, but neither is that access necessarily neutral in the roles it is given for reasons that stem from what we think is going on. Producing a framework for how we can assess the goals and uses of science should be part of an examination of its more obvious and traditional theses. That framework should be one that allows judgements to be made if it is to be of any value.

Having highlighted these loci of interest we now need to clear some ground by looking at a more comprehensive account of experimentation.

ii What an experiment is

Hacking's definition of experiments as the sites of phenomena creation in physics starts the discussion about experiments in the wrong place since there are many everyday cases of experimentation that are not scientific and do not create phenomena.[66] Let us broaden the picture a little.

1 Lexicographies

In *The Oxford Dictionary of Philosophy* Simon Blackburn says of 'experiment':

[66] Alternatively, everything I do can be said to produce phenomena—even if it is the phenomenon of nothing changing, in which case the claim is vacuous.

> A controlled manipulation of events, designed to produce observations that confirm or disconfirm one or more rival theories or hypotheses. To experiment is to put questions to nature, and the experimental method is contrasted with the passive acceptance of whatever observations happen along. The method is characteristic of modern natural science. However, a discipline (such as history) may be pursued with greater or less objectivity and success without being able to avail itself of the experimental method.[67]

According to Blackburn, experiments involve control and manipulation of events (not things). They are essentially tied to the production of data or observations for the testing of theories. Their design presumes the priority of theory over action or accident. The definition excludes the possibility of just trying something 'new,' or the generation of situations where there is no formal way of stating which theories are under test, or even the lack of an agreed way of describing the phenomena observed. He speaks of *the* experimental method, and thereby implies a unity in the method of 'modern natural science,' since experimental method is so characteristic of it in this picture. This seems to fit unexamined intuitions about experiment, even if some of the relativising effects of social constructivist theories of science are included in the outlook.

Hacking directly questions whether this kind of description of the work of experimenters can be correct.[68] He draws on several sources to show that it is possible to make observations and perform experiments without this clear framework of theory and hypothesis. He suggests, in

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[67] Blackburn, S. (1994) *The Oxford Dictionary of Philosophy* Oxford: Oxford University Press, 131.

[68] Hacking quotes Popper on the received view of the separation of theory and experiment:

> 'The theoretician puts certain definite questions to the experimenter, and the latter by his experiments tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude … . It is a mistake to suppose that the experimenter [… aims] 'to lighten the task of theoretician', or … to furnish the theoretician with a basis for inductive generalizations. On the contrary the theoretician must long before have done his work, or at least the most important part of his work: he must have formulated his questions as sharply as possible. Thus it is he who shows the experimenter the way. But even the experimenter is not in the main engaged in making exact observations; his work is largely of a theoretical kind. Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory'. Popper, K. (1959) *The Logic of Scientific Discovery* London: Hutchinson, 107; quoted in *R&I*, 155.

discussing a fundamental difference of opinion about the role of experiment in scientific investigation between Humphry Davy and Justus von Liebig, two great pioneering chemists of their time, that there are two versions of the theory dependence of experiments: a strong version and a weak one. The strong version requires a clear and explicit statement of the theory under test and a complete account of the theory behind your apparatus.

> The *weak version* says only that you must have some idea about nature and your apparatus before you conduct an experiment. A completely mindless tampering with nature, with no understanding or ability to interpret the result, would teach you almost nothing.[69, 70]

However, it is obvious from what Hacking says that he deliberately intends to leave 'some idea about nature and [the] apparatus' as a loose notion. Elsewhere he has been more specific about the kinds of considerations that make up experimentation once the received view is dealt with.[71] Note, that although Hacking wants to ditch the theoretical components from the justification of belief in particular theoretical entities, he retains it, admittedly in this 'weak' form here. I do not think this is a contradiction as he presents it.[72]

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[69] Hacking *R&I* 153.

[70] One might believe that western industrial development has resulted in 'a completely mindless tampering with nature, with no understanding or ability to interpret the results,' it certainly seems to apply in the case of genetics and the claims made for the proposed tampering with the human genome: a future possibility that, despite current claims to the contrary from interested parties, looks increasingly likely through the commercialisation of the whole process of gene research and the Human Genome Project in particular, despite the hidden time scale for the whole project. But this is to prejudge the meaning of both the experimental situation and the theoretical background—it is just not clear that even 'simple' analysis of behaviour to link behaviour to genetic makeup are going to produce contexts for manipulation.

[71] As already noted, Hacking talks about three elements to experimentation, 'ideas' (theories), 'things' (entities and equipment) and 'marks' (output from equipment, laboratory notes, experimental accounts, scientific papers) in 'The Self-Vindication of the Laboratory Sciences'. Franklin (1993) has, rightly, suggested that in trying to specify the exact nature and role of 'ideas' Hacking is in great danger of loosing track of the idea of experiments 'having a life of their own', independent of theories, one of the central components of his argument in *Representing and Intervening*.

[72] Once the criteria of *what* we can use and manipulate become apparent we will see that there is tension here. Experimental realism may not require a commitment to specific scientific theories, but that does not mean that there is an entirely neutral assumed background to what one does things to.

One merit of Blackburn's sketch does stand out. Even if something can be said about what is epistemically good in experiments on the world, there are ways of gaining knowledge other than doing things directly to the object of study. History, astronomy, botanical taxonomy, primatology all make claims about knowledge gathering, without the need to intervene or experiment as a central activity. Linking intervention to science though experiments does not exclude other activities, including simple observation,[73] from the canon of possibilities for gathering empirical information.

The *Oxford Dictionary of the History of Science* is a little more useful in stating that:

> An experiment, unlike an experience, is a designed practical intervention in Nature; its upshot is a socially contrived set of observations, carried out under artificially produced and deliberately controlled, reproducible conditions. At an experiment's core is the notion that the conditions for producing a given effect can be separated into independently variable factors, in such a way as to demonstrate how the factors behave in their natural (i.e. non-experimental) state.[74]

This definition introduces a role for the social organisation of experimental science—although I think we need to be very careful about what 'socially contrived' means without being trivial (or just plain useless). This definition also mentions the need for experiments to produce observations that are reproducible, although this immediately needs adjusting to accommodate the fact many experiments are *not* repeatable (in principle or practically), *pace* Hacking for reasons that include: fundamental changes in natural conditions affecting the experiment; ethical considerations; and expense. Furthermore, if an experiment is acceptable, it is more likely

[73] In the philosophy of physics intervention has been a problem for the interpretation of quantum theory, in that observation can been interpreted as an intervention associated with the resolution of the inherent indeterminacy of the states in physical systems considered at the quantum level. I have deliberately not discussed this issue. It is a separate matter for a particular part of philosophy of science and physics. Later chapters will discuss matters relating to biology and medicine where the interpretation of quantum theory is materially irrelevant.

[74] *The Oxford Dictionary of the History of Science* (1981) Oxford: Oxford University Press, 136.

that the experiment will be changed in some way to help confirm the initial results—to what extent this is repetition of the prime experiment as such, is not obvious. Taking the isolation of 'independently variable factors' influencing a phenomena or effect as a necessary component for experimentation can be found in discussions elsewhere,[75] and is a product of the three metaphysical theses underlying most contemporary philosophy of science, which Dupré attempts to demolish in *Disorder*, *viz.* strong commitments to essentialism, reductionism and determinism.[76]

2 *What an experiment is not*

Both of these simple definitions share intervention and manipulation as the central feature of experiment—something is done to the world by the experimenter, research group or community to gain knowledge. This feature explains, to some extent, the vast array of uses we have for talking about 'experimenting' both inside and outside experimental science. Across a range of human activity we can find talk of intervention, manipulation, test and measurement—some examples: putting mayonnaise in the chicken soup recipe for a change; restricting the supply of money in the economy through a monitoring of the circulation of cash; building a Viking boat to test its seaworthiness and exploratory range; observing the effects of drought on the migration patterns of swallows by a process of ringing and sampling; using a new drug to help stave off AIDS in people tested HIV seropositive; changing toothpaste brand to discover which keeps plaque to a minimum; creating and/or discovering a new particle in an accelerator. But we need to be very clear about what these interventions are. Are they really all the same kind of thing? Do they all involve the same commitment to what is existent, to what we

[75] Roy Bhaskar, in a somewhat obfuscating style, says that an experiment is, 'an attempt to trigger or unleash a single kind of mechanism or process in relative isolation, free from the interfering flux of the open world, so as to observe its detailed workings or record its characteristic mode of effect and/or test some hypothesis about them.' Bhaskar, R. (1986) *Scientific Realism and Human Emancipation* London: Verso, 35. I leave the reader to unravel Bhaskar's prose!

[76] Chapter 3, below.

want to know, or to the methods involved in intervening? Yet in each case we may speak of an *experiment* being performed. It is not simply a matter of using a figure of speech that puts all these examples together, they all involve some form of action and seem to have informative consequences. Even if we restrict ourselves to purely academic concerns, I do not believe there currently exist any clear ways of spelling out what the implications of this are, nor of saying how we are to fit experiments in the social, biological and medical sciences into the same epistemological framework. Perhaps a detailed framework cannot be constructed without doing damage to these intuitions, but three theses can be recognised in this.

Experiments necessarily involve intervention/manipulation which is an important epistemic tool.

The growth of interest in the practice of science is paralleled by a growth of interest in human practice in philosophy generally. With naturalised epistemologies[77] comes the general supposition that there is no obvious point where the things that we do to find out about the world stop and justification of those things begins. What we *do* can be understood in different ways, for example, sociologically, politically and psychologically, but it can also be understood as an expression of a belief in a rational order (or several sorts of order) that can be explored. My claim is that intervening and manipulating can be understood in this way, that is, as rational and having epistemic value. Sometimes *more* can be found out by doing things, by acting on the world, than not doing things. Of course, the big question is *why* this is so. It is possible to say how this acting on the world fits rationally into a set of epistemic goals believed to have real value, and without the notion of intervention there can be no notion of experiment—intervention is wider than experiment and contains it.

[77] See, for example, Kitcher, P. (1992) 'The Naturalists Return' *Philosophical Review* 101, 53-114. There is now a wealth of literature on naturalised epistemology, which I shall not list here.

There are, of course, experiments and experimental situations that involve only minimal intervention in the world. For example, an experiment in the use of a new statistical model in an ecological study of the distribution of newt species in England and Wales does not seem to involve any intervention or manipulation of anything other than the mathematical model used. However, that is exactly what is being experimented on, not the newts themselves—again this does not seem to be the creation of any new phenomena in the way Hacking discusses it. This shows that intervention/manipulation can be applied as a conceptual tool to things other than material objects, entities or events involving such entities, which accounts for the value of such a tool and its diverse applications. But it means we have to look more carefully at Hacking's claims.

Intervention/manipulation is the exercise of agency.

There could of course be great difficulty, even an impossibility, in rationally deciding when one should intervene and when only make an observation. For example, to read the motto that runs round my mug, I can pick it up and turn it round—thereby intervening in the world with the intention to move the mug's position in space relative to myself—or I could get up and move myself round to the other side of the desk to get a better view. Moving the mug is not an experiment, but it is an intervention/manipulation. It is an action informed by my having the capacity for agency in the acquisition of knowledge. We are left with the remaining problem of what other criteria have to be fulfilled for an observation from intervention of this kind to be viewed as an experiment. One easy suggestion might be that experimentation involves the revelation of some hidden material agency.[78] Unfortunately this will not do. Certainly *some*

[78] This turns Hacking's argument around. An experiment reveals aspects of the world to be real *just because* that is what an experiment is. The main argument for this claim is the coherence it lends to the whole of the position I am presenting here.

experiments are of this sort, but it is not clear that all could fit this model. What is clear is that all experiments involve some agent perspective description to be meaningful.[79]

That there are no sufficient conditions for experimentation.

This is part of my claim regarding the plurality of the ontology (ontologies) of world and our access to it, in partial support of Dupré's position. The minimal consistency of experiments in their all being a revealing intervention of some sort, tells us little in detail about how this category of intervention technique(s) is applied. Experiments need not be the same in anything other than this similarity. The examples I discuss at the end of the section help to illustrate this point.

There are obvious cases of rational intervention that are not experiments, as the examples of the mug and the new toothpaste demonstrate. As the account stands scientific experiments are a form of rational intervention in a scientific context. This links the discussion to the problem of the demarcation of science from non-science.[80] But on the whole the experiments do not seem to have a unique characterisation without further clarification of their context of application.

Let us now turn to how *scientific* experiments have been treated by philosophers in the past.

iii History and experiments

According to Hacking, there were no good philosophical accounts of experiment between Francis Bacon and himself. This claim is false. Although the history of philosophy of experi-

[79] Again, this is one of Pickering's points in his (1995) *op cit.*

[80] See my reading of Foucault on this issue, p. 169 ff.

ments is patchy, it is untrue to say that nothing was written about the nature of experimentation from Bacon's death in 1626 until *Representing and Intervening* in 1983. Hacking *is* right to stress that little was said about practice in the majority of twentieth century philosophy of science, and it is worth, for a moment, pursuing the reason for this silence. Hacking wrote in 1984:

> No field of philosophy is more systematically neglected than experiment. Our grade school teachers may have told us that scientific method is experimental method, but histories of science have become histories of theory. Experiments, the philosophers say, are of value only when they test theory. Experimental work, they imply, has no life of its own. So we lack even a terminology to describe the many varied roles of experiment. Nor has this one-sidedness done theory any good, for radically different types of theory are used to think about the same physical phenomenon (e.g., the magneto-optical effect). The philosophers of theory have not noticed this and so misreported even theoretical enquiry.[81]

Hacking almost seems to say that such neglect can only be understood through recognition of the profound stupidity of such philosophers. This point of view is, of course, untenable; so one must look for other reasons for the neglect in this century. The first thing to do is to see what lies this side of the Kuhnian revolutionary fire break that opens *Representing and Intervening*, to see whether the answer lies in different approaches to the traditional worries about meaning, change, rationality and reference. To begin with the revolutionary himself.

1 Kuhn, experiments and history

Thomas Kuhn[82] has suggested that there is a tradition stretching back to early Greek thought in which a clear distinction may be drawn between the experimental and mathematical sciences, born of the mystical conception of mathematics; that this distinction truly comes into full operation in the seventeenth century with the generation of Baconian sciences; that it con-

[81] Hacking, I. (1984a) 'Experimentation and Scientific Realism' in Leplin, J. (ed.) *Scientific Realism* Berkley: University of California Press.

[82] Kuhn, T. S., (1976) 'Mathematical versus Experimental Traditions in the Development of Physical Science' *Journal of Interdisciplinary History* 7, 1-31.

tinues throughout the eighteenth and nineteenth centuries; and that it can be clearly marked out in the seventeenth century by Newton's supposed different strategies in *The Optics* and *The Principia*.[83]

Even if the controversial claim about Newton could be resolved, this distinction will no longer fit into the naturalised philosophy of practice now emergent in philosophy of science. Kuhn's thesis is about supposed methodological concerns over how theories are to be put to the test. The line he draws divides the sciences into those which rely on trial-and-error, inductive, Baconian procedures, and those with a more rigid deductive, aprioristic system. In other words, he is still concerned with examining how theories are *considered testable* within the two traditions he highlights, not how they are in fact tested and what this might reveal about these methods of testing, or the world. He still submits to the view of his contemporaries and those he aimed to displace. Not only does he not take on board the variety of practices employed in theory testing, but implicitly allows that there would only be a simple single relation between theory and observation in any particular field of science, even though that relation may be paradigmatically determined.

That scientists view different theories in only two ways is Kuhn's conclusion, but there seems nothing particularly enlightening or radical in that. Furthermore, Kuhn's distinction does not tell us why experimental practice itself is neglected in the philosophy of science of the early twentieth century and in his own writing. Theory bias has dominated the philosophy of science to such an extent that the agenda set by logical empiricism was taken up by the more historically informed philosophy that superseded it. Kuhn's apparent attempts to talk about experi-

[83] Newton, I. (1934) *Mathematical Principles of Natural Philosophy* trans. Motte, A. Berkeley LA: University of California Press; (1979) *Opticks, or A Treatise of the Reflections, Refractions, Inflections and Colours of Light* New York: Dover.

mental science fall short of what might be taken to be Hacking's bench mark of concern because of this bias.

It may be that a nod towards the dominance of theory in general philosophy, seen as the Rortian targets of epistemology and mind as theatres for reference, will help to resolve some of the apparent difficulty in explicating the lack of interest in experiment in modern philosophy. But perhaps this is putting things the wrong way round. It is explicit in Hacking's thesis that there is a gap to be filled by a philosophy of experiment, and that the subject has always been interesting and important. This is all very whiggish. He is constructing a misleading history for the philosophy of science. The conceptual work and logical analysis pursued by the logical empiricists was not mistaken, given their interests. Attacks on their lack of insight into experiment is short-changing the efforts of past philosophy of science. Experiment as a topic for philosophical investigation was not pursued in this century because it was not interesting to the philosophers concerned. Carnap puts it this way in *The Unity of Science*:

> In the first place I want to emphasize that *we are not a philosophical school and that we put forward no philosophical theses whatsoever.* ... Any new philosophical school, though it reject all previous opinions, is bound to answer the old (if perhaps better formulated) questions. But we give no answer to philosophical questions, and instead *reject all philosophical questions*, whether Metaphysics, Ethics or Epistemology. For our concern is with *Logical Analysis.* If this pursuit is still to be called Philosophy let it be so: but it involves excluding from consideration all the traditional problems of Philosophy.[84]

This bold statement of the Vienna Circle's aims has echoed through much later writing and work on the philosophy of science, until quite recently, in fact. The concern with theory was not an oversight—it was deliberate. Practice itself does not open up to logical analysis in a pure form. It is only with the return of messy metaphysics—with the attendant problems of

[84] Carnap, R. (1934) *The Unity of Science* London: Kegan Paul, Trench, Truber and Co. Ltd.

dealing with human ontologies—and untidy epistemology that the agenda of appropriate questions for philosophy of science can include experiments and experimentation.

2 Historical studies, science studies, instruments

In the last fifteen years, along with these 'untidy' interests, there has been an explosion in historical studies of experiments and their relevance to both the history and philosophy of science. It is beyond this study to survey all the material available, but it is worth noting some of the more interesting paths that these historical accounts have taken. The key to understanding these historical studies must be that of embracing diversity, at least in terms of the uses to which these studies are put. Near the opening of their collection of essays Gooding, Pinch and Schaffer write:

> Our case studies include Galilean mechanics, Newtonian Optics, early Victorian electromagnetism, experiments on insects, on clouds and thunderstorms, on quarks and on the accuracy of nuclear missiles. This sample hardly exhausts the fields in which experiment matters. Generalisations across such a range are likely to be provisional and we do not imply that there is some essential and unchanging activity called experiment. Yet there are some important lessons about the way this kind of human activity has developed and the uses it serves. These include human agency and skill, the role of persuasion and of rhetoric, and the significance of the site of experiment and of instrumentation both to learning and persuasion.[85]

One feature of their boasts about the size of their editorial net is immediately obvious: except for 'experiments on insects' the whole selection is drawn from the physical sciences. This bias becomes more apparent from surveying the contents proper. Echoing my earlier remarks, there must be a suspicion that if diversity is so vast in the physical sciences, then even more significant consequences could appear if the biological (and medical) sciences are included. Further

[85] Gooding, Pinch and Schaffer (eds.)(1989), *The Uses of Experiment: Studies in the Natural Sciences* Cambridge: Cambridge University Press, xv. References marked with † are to this volume.

extension to the psycho-social sciences is another issue that I shall mention again later in connection with Foucault.[86]

Instruments and the acquisition of the skilled use of instruments have been one focus for discussion. For example, in the Gooding, Pinch, Schaffer collection there are three essays on the co-development of instruments and techniques by Hackmann,[87] Schaffer[88] and Bennett[89] and there are other less direct treatments of the role that instruments play in experimentation. The establishment of the use and significance of particular instruments is not simple, as Schaffer's discussion of the Newtonian prism shows. There is a dynamic interplay between the scientific theory that the instrument demonstrates and the development of the instrument itself—and it is indeed demonstration that is the key here. Acceptance of theoretical claims, which can also be read as acceptance of experimental results, depend, to some extent, on the experiment being seen to work and to be independently verified. This adds a further factor to any picture that tries to establish a rational, naturalised account of science practice.

One suggestion might be that the route is not rational, that the reconstruction of the experiment later is simply a construction through other pressures (market forces, psychological influences, authority) given that the experimental apparatus has already been embedded in the theory and has become 'transparent' (Schaffer's term). Such a suggestion is too simple. The apparent need to understand what a particular piece of apparatus is, before assessing the experiment it is used for can be understood, or assessed, does not of necessity exclude the possibility of placing this transparency within a rational account of the experiment, even at the cost of introducing some vagueness and ambiguity into the experiment, as talk of agency does.

[86] See p. 190 ff.

[87] † Hackmann, W. D. 'Scientific Instruments: Models of Brass and Aids to Discovery', 31-66.

[88] † Schaffer, S. 'Glass Works: Newton's Prisms and the Uses of Experiment', 67-104.

[89] † Bennett, J. A. 'A Viol of Water or a Wedge of Glass', 105-114.

After all, talk of manipulation or intervention has remained unexamined in the literature, but is crucial to current phenomenological and naturalised theories and stories about experiments. In practice this means that acquisition of skill in this regard remains unclear. This does not mean that the procedure has no rational nature even though many reconstructions may take place to get to any coherence of theory and practice. Thomas Nickles has argued otherwise,[90] a tactic that has much in common with sociological reconstructions of the practice of science.[91]

3 Sociology

The Sociology of Scientific Knowledge (SSK) programme has tried to show how science as practice, seen as the interplay of the dynamic groups composing the scientific community, can and does generate knowledge. The highlighting of neglected interests and of the political and personal motivation for particular projects can only be for the good, since it is not always obvious that science is the open, free and democratic enterprise that it claims to be. However, the stronger thesis that sociological relations are all there can be to science needs much more support than demonstrations that scientists have and are motivated by other interests apart from rational, objective knowledge seeking. Dupré correctly criticises this kind of approach for its blanket inclusion of all science under one theoretical framework:

> … being quite unrestricted in [its] scope, [it] provide[s] no motivation for questioning the specific epistemic credentials of particular scientific projects. That is [it] typically assume[s] that the domain to which … analyses apply is given in advance, tacitly presupposing some kind of scientific unity. … By asserting that all scientific beliefs should be explained in terms of the goals, interests, and prejudices of the scientist, and denying any role whatever for the recalcitrance of nature, it leaves no space for criticism of specific beliefs on the grounds that they do reflect such prejudices rather than being plausibly grounded in fact.[92]

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[90] † Nickles, T. 'Justification and Experiment,' 299-334.

[91] This agent perspective was discussed earlier, p. 26 ff.

[92] *Disorder* 12.

It is exactly because nature is recalcitrant—that it resists certain manipulation and intervention by humans in particular ways—that experiments can have any significance at all, as was noted in the quotation from Pickering.[93] If the reply is that science ultimately has no significance, at least as "objective" knowledge, one may legitimately ask the critic why she should be believed more than the scientist. How is her position to be rationally justified? To claim that she is *merely engaging in a dialogue* that we all participate in being culturally specific humans, a typically Rortian response,[94] is not good enough if one happens to be in a minority group and suffers because of the application of prejudices through science and technology. At least, it is not good enough if one wants some aspect of rational justification as part of one's understanding of science.

But this is a caricature and cannot stand as a full analysis of SSK. We need to take care to separate the general point that scientists are social beings—and that therefore their beliefs are a partial product of this state of affairs—from the stronger program that aims for a completely socialised epistemology of science.[95] Later, again in connection with discussions of Foucault, we shall see how 'sociological' issues are important when placed in a proper context, where there are a multiplicity of interests. For now, let us carry forward the observation that the application of sociological analysis to all parts of science practice in all disciplines is questionable and ultimately incomplete. This is as true of current analyses of experiments as it is of other aspects of science.

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[93] See p. 28 ff.

[94] I admit that this does over-simplify Rorty's position. His overview of the place of rationality and philosophy will be discussed later in Chapter 5.

[95] See Fuller, S. (1988) *Social Epistemology* Bloomington: Indiana University Press; Rouse, J. (1987) *Knowledge and Power* Ithaca: Cornell University Press; Latour, B. (1987) *Science in Action* MA: Harvard University Press; Longino, H. (1990) *Science as Social Knowledge* Princeton: Princeton University Press.

So it is clear that the history of the philosophy of experiment needs to be sensitive to the philosophical interests of the time. Hacking has not succeeded in showing that there is a missing aspect to past philosophical musing about experiments. Similarly, attempts to reconstruct historical and present accounts of experiments as only the product of sociological interests lack many motivations that lie in philosophy. Attempts to go beyond the common criteria for *all* experiments either result in frameworks that lack the power to assess the epistemological value of particular experiments, or are not wide enough in their scope to include fields that we would want to assess as experimental.

iv Probability and the detail of rational practice

Putting socio-historical matters on one side for the moment, let us now look at what happens when rational considerations of experiment are stressed. One of the most conspicuous ways that experiments have been used and analysed by philosophers of science involves a re-emergence of attempts to characterise the rationality of scientific practice. Unlike most other areas of our intervention and manipulation of the world, experimentation is often recorded in detail, and the methods by which experimenters correct their procedures to minimise error and difficulties are usually open to inspection. Part of this re-assessment is of course related to a dissatisfaction with the over-emphasis on the socially constructed component in experimental work discussed above and will help to put back some of the other missing elements of interest to the experimenter (and the philosopher).

1 Different perspectives—Kitcher's dynamic model

To begin with let us look at the possibility of re-articulating a role for the concept of rationality in philosophy of science generally. In *Advancement* Kitcher has tried to incorporate rational and group interests into a coherent epistemology. One of his claims is that the running down of the possibility of an epistemology of science—through past implicit stress on the static nature of rationality—has failed to show how a dynamic model of rationality might be significant in such an epistemology. Kitcher discusses this change of model and its importance.[96] He highlights how the rational status of single beliefs is not interesting or apposite in looking at how science is conducted in a modern context.

What is much more interesting is the rationality of belief change, that is, noting what evidence (and how it is weighted) is used to alter the understanding of particular theories. Kitcher gives his own account of how sense can be made of the inductive practice of scientists that addresses the fundamental issue of underdetermination, one of the key sites for Kuhn's criticism of the earlier, highly logically structured picture of science. Kitcher calls this pre-*Revolutions* school of philosophy 'Legend.'[97] Kitcher goes a long way to salvaging rationality from the use of studies of the social and political influences on scientists—stories that generate stories about science that exclude even the possibility of the activities people engage in being explicable and assessable through reasons—that fit the epistemic goals that they have as individuals or as part of a community. He stresses that science is done by people with real lives and real histories embedded in traditions of research, but points out that it is still open to us to understand the nature of science in a philosophical way, involving analysis of the epistemic rea-

[96] Kitcher *Advancement*. Chapters 6-8.

[97] Of Legend Kitcher says: 'Legend celebrated science. Depicting the sciences as directed at noble goals, it maintained that those goals have been ever more successfully realized. ... Successive generations of scientists have filled in more and more parts of the COMPLETE TRUE STORY OF THE WORLD (or, perhaps, of the COMPLETE TRUE STORY OF THE OBSERVABLE PART OF THE WORLD). ... Inspired by the work of Gottlob Frege and Bertrand Russell, the architects of modern mathematical logic, logical empiricist philosophers of science proposed to uncover the logic of confirmation, the logical structure of theories and the logic of explanation, thus formulating with precision those canons and criteria that they took to be tacitly employed by scientists in their everyday work. References to logic reverberate like drumrolls through the classic works of logical empiricist philosophy of science, works that, because of their clarity, rigor, and attention to a range of considerations, belong among the greatest accomplishments of philosophy in our century.' *Advancement* 3-5.

sons people as scientists have for their beliefs in the area of science in which they are working.[98] He says:

> I conceive of rationality as a means-ends notion. Concepts of rationality are generated by thinking of entities (people, groups of people, science as a whole, science and its relations to society) as meeting some criterion of good design (maximization of expectation, expectation of positive modification, high expectation with respect to rival entities) relative to a set of goals (epistemic goals, practical goals, both.)[99, 100]

He recognises that this characterisation of rationality could well be criticised from a number of positions. Traditionally there could be no way of taking goals/ends, into account when drawing up epistemic rules for rationality because of their supposed psychological source.[101] Given his account of what we ought to be doing in examining the rational procedures of science, it is no surprise that Kitcher considers 'apsychologistic' epistemologies of science to be 'flawed.' Only by the introduction of such notions as the epistemic goal(s) of the subject (the scientist, or research group) can a destructive relativism be avoided and the study of epistemology have any purpose.[102] This accords well with my earlier sketch of experimentation that urged that we recognise the human agency involved in rational intervention in the world, and hence in experimentation. Kitcher's analysis of rationality in science is part of his wider pro-

[98] Perhaps this is the actual difference in strategy employed by the opponents on either side of the *SSK* break. Those obsessed with the sociological aspects of science practice discuss the reasons for belief change from the perspective that we must examine 'scientists as people,' in the sense that people are prone to all sorts of 'irrational' influences; whereas there is always the possibility of attempting to grasp 'people as scientists,' which allows for the fact that people are also rational and can make decisions based on reasoned judgement.

[99] *Advancement* 179.

[100] There is an issue, a very large issue, about how to reconcile the tendency to generate big pictures about knowledge and the world with the local details of rational practice. Again, we shall encounter this again in connection with Bachelard and Foucault, and Putnam and Rorty.

[101] Kitcher notes Strawson, P. (1952) *Introduction to Logical Theory* London: Methuen; and Carnap, R. (1951) *Logical Foundations of Probability* Chicago: Cambridge University Press, in this context.

[102] Richard Rorty has argued that there is nothing left for epistemologists to do, perhaps most notoriously in *Philosophy and the Mirror of Nature* ((1979) Oxford: Blackwell), esp. chapters VII and VIII.

gram that gives a role to philosophy, and avoids many of the past problems of trying to draw lines around the 'good,' rational, Legend-like theories and the 'bad,' irrational, social constructivist theories about science, exclusively.[103] Again, this seems to be another indicator that the Kuhnian revolution has been completed and absorbed back into issues that are genuinely important in trying to say *anything* coherent about science.

However, despite Kitcher's stress on the rational practice of scientists, he does not go too far in looking at that practice in experiments and experimenting. Although experiments can be, for him, the crucial point where theories really are put to the test, altered, rejected, constructed and so on, and a number of different ways of doing this could be devised and considered, the 'how' of this testing is left somewhat vague. Scientists can be rational, even in their attitudes and interactions with each other over acknowledging authority in their field, but there is a gap in what Kitcher says about the rationality of the actual practical measures that experimenters take in the performance of complicated experiments with complex equipment; how, for example, experimenters distinguish artefacts from genuine results. As with the logical positivists, this is not because he is missing something, but that his analysis is operating at a more general level. But it is disappointing that he does not take his analysis further because the detail of experimentation might provide very good evidence of the kind of dynamic rational processes he wants to highlight.

Let us now look at work on the details of experimentation by philosophers with *some* similar aims as Kitcher, the re-articulation of philosophy of science that gives robust content to notions such as rationality, rational theory change, expectation, prediction and success where the details of experimentation have been central. Work has been done here that focuses on the use of Bayesian probability analysis, that is, the conversion of statements of degrees of belief into

[103] *cf.* Newton-Smith, W. H. (1981) *The Rationality of Science* London: Routledge.

statements of probabilities for these beliefs, and the treatment of them through Bayes's Theorem and the probability calculus. Kitcher notes the apparent potential of pursuing this kind of approach:

> Bayesianism has many virtues. It offers a unified account of the confirmation of hypotheses that can resolve some issues of underdetermination, can yield solutions to traditional logical puzzles about confirmation, and can explain the differential force of different types of evidence in *some* historical cases of scientific reasoning. ... Bayesianism is clear, precise, and unified: there are results about proper reasoning and there is a single perspective from which these results flow.[104]

He does, however, have a number of reservations about Bayesianism that include the observation that there is evidence that people do not in fact reason probabilistically.[105] There are other criticisms too that will arise from what we have considered so far, but before looking at these in more detail it will be useful to lay out some of the work of Bayesian analysis of experiments. Whatever objections can be raised to Bayesianism, in some of its forms it is, to date, the most structured thinking on experiments. Furthermore, the unity of practice it presents will have to be examined if any claims about disunity of *method* in science are to be assessed thoroughly.

2 Bayesian analyses

At the beginning of *The Neglect of Experiment* Alan Franklin raises two questions about experimentation:

> 1. What role does, and should, experiment play in the choice between competing theories or hypotheses or in the confirmation and support of hypotheses?

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[104] *Advancement* 291-292.

[105] Kitcher notes Tversky, A. and Kahneman, D. (1973) 'Availability: A Heuristic for Judgement Frequency and Probability' *Cognitive Psychology* 5, 207-232; (1974) 'Judgement Under Uncertainty: Heuristics and Biases' *Science* 185, 1124-1131; Nisbett, R. and Ross, L. (1980) *Human Interference: Strategies and Shortcomings of Social Judgement* Englewood Cliffs, NJ: Prentice Hall; and Goldman, A. (1986) *Epistemology and Cognition* Cambridge, MA: Harvard University Press, chapter 15.

2. How do we come to believe rationally in the results of an experiment, or how do we separate results, obtained by use of apparatus to measure or observe a quantity, from an artefact created by the experimental apparatus?[106]

The first is not a neglected question. It has been the stuff of Legend for a long time and continues to exercise many thinkers. However, it is the second question that has become a locus for much contemporary work for probability theorists. Franklin admits, 'I do not have a general answer to the first question concerning the role of experiment in theory choice.'[107] He goes on to describe in quite involved detail three 'episodes' in physics that required the use (discovery and refinement) of difficult experimental techniques. These are experiments on the non-conservation of parity, the discovery of CP (combined space inversion and particle-antiparticle interchange) violation and the experiments carried out by Millikan to determine the size of *e*, the basic unit of electric charge, and its quantum nature. He says that this is a varied survey of experimentation, a claim that seems hard to support given the narrowness of the field from which all three studies are drawn, namely physics (and a limited part of it at that). Nevertheless, within his own Bayesian outline, there are differences between the experiments in the sense that different strategies are employed in the conduct of the experiments in calibrating the equipment, grasping the significance of the output of the equipment, determining the relevance of the results so obtained, and so on.

Bayes's Theorem can be expressed by the identity:[108]

$$P(h|e) = \frac{P(e|h)P(h)}{P(e)}, \text{where } P(h), P(e) > 0$$

[106] Franklin, A. (1986) *The Neglect of Experiment* Cambridge: Cambridge University Press, 3.

[107] *ibid.* 5.

[108] The notation used is standard logic and probability notation. The majority of the formulae used here are taken from Franklin (1986) *op cit.* and Howson and Urbach (1989) *Scientific Reasoning: the Bayesian approach* London: Open Court.

('$P(h)$' is the probability of the hypothesis under test, '$P(e)$' the probability of the evidence, '$P(h|e)$' the probability of the hypothesis given the evidence under consideration, and '$P(e|h)$' the conditional probability of the evidence given the hypothesis.)

This is easily obtained from basic axioms in probability theory.[109] Simply put, Franklin believes that we can successfully isolate strategies by which the validity, the acceptability of experimental results are tested and evaluated, and demonstrate that they are justifiable through the use of the theorem. He highlights nine such strategies:

1. Experimental checks and calibration, in which the apparatus reproduces known phenomena.
2. Reproducing artefacts that are known in advance to be present.
3. Intervention, in which the experimenter manipulates the object under observation.
4. Independent confirmation using different experiments.
5. Elimination of plausible sources of error and alternative explanations of the results.
6. Using the results themselves to argue for their validity.
7. Using an independently well-corroborated theory of the phenomena to explain the results.
8. Using an apparatus based on a well-corroborated theory.
9. Using statistical arguments.[110]

Each of these strategies does have an immediate appeal and examples can be found for their application to actual scientific practice. For example, strategy 5. is called the '"Sherlock Holmes" strategy' by Franklin: Holmes once remarked to Watson, '"How often have I said to you that when you have eliminated the impossible, whatever remains, *however improbable*, must

[109] See Howson and Urbach *op cit.*

[110] Franklin, A. (1990) *Experiment, right or wrong* Cambridge: Cambridge University Press, 104.

be the truth."'[111] He proposes that there is a rational analysis in formal Bayesian terms for the elimination of plausible sources of error in the obtaining of experimental results. Thus this gives a measure to their validity. It seems that this is a key strategy for checking this validity, and provides a stronger criterion than some of the others he lists. It could, I suggest, subsume strategies 1-3, in that they are themselves only specific strategies for the removal of potential sources of error in the apparatus used. If there are no results, or wild results, one source of error in your experiment is that the apparatus is not working properly. As with the whole of this approach there is a built-in sense of expertise. The experimenter must be able to say beforehand which processes and events are possible and then must assign a prior probability to them. 'A Bayesian approach shows quite clearly why this strategy works.'[112]

If H = the hypothesis believed to be the explanation for e.

e = the results obtained,

h_i = the 'plausible sources of error' (a possible explanation for e), then,

$$P(H|e) = \frac{P(e|H)P(H)}{P(e)} = \frac{P(e|H)P(H)}{\left[P(e|H)P(H) + \sum_i P(e|h_i)P(h_i)\right]}$$

If the experimenter then obtains further information that eliminates h_j, that is, she effectively falsifies the theory under test, $h_j \rightarrow \neg e_1$ so that $P(e_1 \mid h_j) = 0$, then,

[111] In fact the nearest I can find to the quotation Franklin gives is "Eliminate all of the factors and the one which remains must be the truth" Conan Doyle, A. (1985) 'The Sign of Four' *Sherlock Holmes Selected Stories* Oxford World's Classics, Chancellor Press, Oxford University Press: Oxford, 73.

[112] Franklin (1990) *op cit.* 109.

$$P(H|e \wedge e_1) = \frac{P(e \wedge e_1|H)P(H)}{\left[P(e \wedge e_1|H)P(H) + \sum_{\substack{i \\ i \neq j}} P(e \wedge e_1|h_i)P(h_i)\right]}$$

gives the result that,

$P(H|e \wedge e_1) > P(H|e)$.

This result says that the probability of the hypothesis (H), given the evidence to support it and the evidence that eliminates the competing hypothesis, is greater than the probability of the hypothesis given the supporting evidence alone. Thus the strategy is seen to provide an increased probability validity to the hypothesis. Franklin goes on, '[w]e can continue this procedure, acquiring additional pieces of information e_2, e_3, ... , e_N, which eliminate all the competing h_i with any significant prior probability, the plausible sources of error and alternative explanations.':

$$P(H|e \wedge e_1 \wedge e_2 \wedge \ldots \wedge e_N) = \frac{P(e \wedge e_1 \wedge e_2 \wedge e_N|H)P(H)}{P(e \wedge e_1 \wedge e_2 \wedge \ldots \wedge e_N|H)P(H) + \beta}$$

Franklin says that β is 'the very small remainder of the sum' so that,

$P(H|e \wedge e_1 \wedge e_2 \ldots e_N) \approx 1$, thereby giving large support to H.

There are some technical objections to this procedure that need not be examined here.[113] However, there are obvious ones that have immediate consequences for my argument. The Bayesian technique rests on the ascription of prior probabilities for hypotheses that are corrected by the evidence collected. In this case, the prior probabilities are a measure of the perceived

[113] See Glymour, C. (1981) *Theory and Evidence* Chicago: Chicago University Press. Of course, Glymour's objections to Bayesianism are more than just technical.

significance of particular 'sources of error' in the experimental set up. It incorporates the possibility that β does not become vanishingly small, that there are other errors that have not been given any significance by the experimenter, that is, that experimental work is fallible, while still allowing a dynamic assessment of the hypotheses.

> As the work of elimination proceeds, the Bayesian apparatus can record just how the fortunes of the rival hypotheses are changing. ... thus supporting the view that actual scientific reasoning is a fumbling approximation to something clearer and more sophisticated.[114]

However, considered alongside the Bayesian objections to the competitor theories derived from Fisher's notion of objective probabilities, there is always the option of denying that Bayesianism has the natural rational grounding it claims. Consider:

> Fisher envisaged an experimental design which would permit reliable inductive inferences to be drawn, whatever unknown extraneous influences happened to be operating. ... unless these possible influences could be assessed as to whether they are likely to have a significant effect, Fisher's recommendations would be ineffective, for they would require infinitely many randomizations.[115]

Consequently, there is a continuing difficulty with assessing exactly what is to be a 'significant.,' problem, a difficulty that only seems solvable by sacrificing whatever immediate intuitive appeal Bayesianism has of a description of the practice of experimenters to further technicalities. In either case, the Bayesian project looks difficult to complete. Kitcher also notes difficulties in the Bayesian programme. The first of which matches this objection.

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[114] *Advancement* 292.

[115] Howson and Urbach *op cit.* 152-153.

> Given the absence of constraint on prior probabilities, it is quite possible that the Bayesian arrives at some extremely unintuitive initial assignment, with the result that, when his colleague has come to accept the last remaining candidate, the Bayesian still assigns that hypothesis a very low probability. To be sure, there are convergence theorems about the long run—but as writers from Keynes on have pointedly remarked, we want to achieve correct beliefs in the span of human lifetimes. ... The root difficulty is that one *ought* not to partition the space of candidates in vast numbers of the ways for which the Bayesian allows. ... having recognised this, one can also see that *any* Bayesian partitioning of that space imposes an arbitrary and unmotivated structure. Only in special cases can responsible assignment of probabilities be made.[116]

He also points out that Bayesianism is only genuinely useful in the assessment of 'epistemically perfect situations,' which once again points us towards the very limited number of contexts where we have such situations and the application of *ceteris paribus* criteria. Conflict in the statements generated by different researchers and genuinely open questions cannot be taken account of. This can be made into a finally damning criticism of the application of Bayesianism to experiments, as follows.

3 *Epistemology and practice*

In Gooding's analysis of actions and agency in experiment he speaks of the ambiguity of the entities and events in experimental science. He says:

> ... the ontological ambiguity of manipulated objects is essential to the construction of new phenomenal possibilities. The ambiguity of what is manipulated is crucial to the creative stages in the development of thought—and real—experiments because it enables free movement between possible (mental) and actual (material) entities.[117]

The 'ontological ambiguity' Gooding is referring to is between models, hermeneutic tools, approximations, descriptions, objects and so on, between the mentally real and the materially real. This adds to the account of experiment I offered earlier by making the relationship be-

[116] *Advancement* 292-293.

[117] Gooding *op cit.* 13.

tween material and human agency dynamic. Let us suppose, for the moment, that this is correct. The amount of ambiguity may vary greatly from experiment to experiment, for example, the more the scientist thinks he already knows, the lower the ambiguity in similar experimental situations where novelty is less significant—Gooding's talk of ambiguity applies to 'the construction of new phenomenal possibilities,' and not all experiments are at this level of novelty. For example, the genetic manipulation of traits in the manipulation of commercial crop yields[118] does not of necessity involve the use of phenomena and entities that are new to the experimenters, and trying out new toothpastes seems not to create anything. However, if once again we grant Gooding the philosophical context of his investigation, the philosophical interest in experiments in science seems to rest on how they create new phenomena and novelly add to theoretical claims, even when the focus is chiefly on instruments and skills. In any case, if these novel situations are *not* understood in an examination of experiments it would make any theory that could only account for non-novel experimentation seem suspect in the extreme. From Kitcher's work, a dynamic epistemology is now seen to provide a model with superiority over one that regards science as worthy of discussion only in so far as the static status of single theories are considered.

The movement and development of science in theory and practice, while at the very least *appearing* to track something about the world, is exactly one of the reasons that make science philosophically interesting. In including the creation of new phenomena *among* the features of experiments, Gooding is more sensitive to variety of experiments than Hacking. Although, like Hacking, he also seems to suggest that without novelty nothing interesting takes place in experiments. I have already suggested that there is an available definition of the necessary conditions of experiments that does not include novelty. What I am taking from Gooding is the

[118] Described below, p. 82 ff.

idea that whilst manipulating even known parts of the world to produce an effect, perhaps in checking something one is only a little unsure about, one need not be committed to the status of the entities involved. For example, if there is an unpleasant squeak when I play a particular music CD, I may be unsure about whether the squeak is on the CD as a fault, a result of a fault in the equipment I am using to replay the music, or, more importantly part of the music that I am not hearing as such. Sorting out what is going on may involve my being undecided about what is music and what is not for some time as I experiment with different aspects of the CD and the player, altering volume and frequency play-back settings, and so on.[119]

The question is, how could Bayesianism possibly begin to say what is happening when scientists shift and change their attitude, even fail to have an attitude, towards the things with which they are experimenting when they are in this state of ambiguity? To put this in the terms I have already used, how can a Bayesian probability measure be given to the revelation of material agency when it is unclear what the relation between material and human agency is until after the fact? This is not a matter of there being different hypotheses that explain the phenomena, that each is eliminated by careful consideration of the plausibility of each of these and the evidence collected to carry out this elimination. That requires the epistemic purity that Kitcher mentions. In such experiments the Bayesian can be neither normative nor descriptive without doing damage to the history of what was actually done. Gooding states:

> ... the self-evidence of the mental (conceptual) or material (real-world) status accorded to entities to which language refers is conferred through reconstructive processes. Judgements about the reality or necessity of an entity and about the directness of an observation are therefore retrospective.[120]

[119] On a related point, in the days of vinyl LPs, in the 1970s, there was much discussion amongst sound purists over the status of the 'hum' that resulted from the electronic recording equipment used, and which was subsequently transferred to the record. Some took it to be part of the recording itself, others as an additional interference to be filtered out.

[120] Gooding *op cit.* 13.

So for Gooding what is done, the procedures employed, the techniques and their rational placing in an account of the experiment, can only be comprehended after a reconstruction of the experiment through notebook entries, demonstrations to other scientists, the publishing of journal papers, conference reports, internet notices[121] and so on. Thus, if Bayesianism has an application in such experiments it cannot be to the raw experimental situation as Franklin claims it does—it is not about what experimenters *do*, nor could it be.

The question is now whether Gooding is right to talk about this ambiguity of entities in experiments that generate new phenomena. This also must wait until we have further evidence from an examination of the biological sciences and their application, where it will emerge there is in some cases *more* ambiguity than in the physical sciences. However, it connects with difficulties in understanding what science and experimentation tells us about the metaphysics of the world and its relation to us as human subjects. There is more than one way of doubting the status of objects. Gooding speaks of the combining of material and mental attitudes to entities that embeds them in a community's multi-layered accounts of experimenting. There is a further ambiguity to consider, that of the status of the categories to which even common sense entities are ascribed. But at what level of our metaphysics do we have ambiguity? We need to be *quite sure* what we are *unsure* about, that is, about what features of the world we are taking as providing the weak, minimal background to our being able to perform any experiment without complete blindness. I need to say much more on what kind of metaphysical order science works with before this can be resolved.

[121] Gooding does not note the growing importance of the Internet to the dissemination of information amongst the scientific academic community—this is another site for the reconstruction of experiments. See, for example http://otis.msfc.nasa.gov/fiml/iml1lse.html; http://www1.cern.ch/CERN/experiments.html; http://www.jmstec.go.jp/index-e.html.

v Unification: introducing the debate

The Bayesians construct epistemologies that allow parts of Legend (rational justification) to re-enter into discussions of science and a naturalised account of science practice. Generally they are looking for (at least) a minimal unity in that method so that science can be taken as a whole when considered as an epistemically significant (or superior) part of our culture. I would now like to introduce the second theme of this thesis, that of disunity.

The twentieth century has seen various attempts to provide a unified methodological account of science in much greater detail. Popper's long term programme was such an attempt. I shall not re-analyse the problems that falsificationism still faces. However, unity of methodology across the fields of science (across the sciences) is only one form of unity. Metaphysical unity, that is, a unity of the kinds of things science investigates and supposedly reveals is a presupposition of much contemporary philosophy of science. It is still a common feature despite the fragmentation of the field into problems arising from specific matters in each of the special sciences. The most obvious growth in these areas is in philosophy of physics and philosophy of biology.[122]

In *The Disorder of Things* Dupré has argued against such unity. He uses a combination of metaphysical analysis and theoretical evidence from biology and psychology to dismantle the notion of a cosmic order. In terms of understanding the rational practice of science his aim is not entirely different from Kitcher's; he believes that:

[122] For interesting, recent examples see the following books, both published in the same 'Dimensions of Philosophy Series' (general eds. Norman Daniels and Keith Lehrer). Sklar, L. (1992) *Philosophy of Physics*; Sober, E. (1993) *Philosophy of Biology*; both Oxford: Oxford University Press.

> ... "good" science involves inseparable elements of the epistemically good and the socio-politically good. My overall thesis is that there is a lot of good science and a lot of bad. Neither the uniform reduction of science to the nonepistemic, nor the concession of all epistemic matters to those who choose to call themselves scientists, can adequately pursue this insight.[123]

It should be noted that Kitcher firmly believes that there is a way in which science can be unified, that is through explanation.[124] Indeed Kitcher has often argued for the view that progress can be characterised by an increase in the scope of scientific explanations.[125] As Dupré points out, whatever attraction this theory has at a local level, there is not of necessity an extension to the global unification of all the sciences:

> Progress ... in ... biology could go on indefinitely instantiated by a sequence of theories able to explain ever more *biological* facts, but would do so by achieving ever-more-powerful understandings of the same domain of phenomena. Kitcher's argument implies that *if* a theory were introduced that explained in approximately the same way ... a large range of both chemical and biological phenomena, it would be accepted over a theory that explained only biological phenomena. But there is no reason to believe that such theories are generally available, and much reason to doubt it.[126]

Telling the good from the bad is going to be aided by precisely the kind of framework for assessment of practice I have been considering here. But there seems to be only a minimal notion of experimentation available. So what is the relationship between metaphysical unity and methodological (experimental) unity? To answer this question Dupré's notion of disunity will have to be unpacked carefully since it is an extended argument about metaphysical disunity and is, I believe, a persuasive one. Some preliminary thoughts may help to guide this analysis.

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[123] *Disorder* 13.

[124] For discussion of the relationship between explanation and other issues raised here see p. 89 ff., below.

[125] See, for example, Kitcher, P. (1981) 'Explanatory Unification' *Philosophy of Science* 48, 507-531.

[126] *Disorder* 228.

As noted earlier, 'intervention' does not mean the same as 'experiment,' it has a wider application. Now Hacking thinks that intervention and manipulation can be part of a theory about what the world contains, it can be part of defence of realism. But realism about what? If the world consists of different sorts of things does intervention vary, and in what ways? But in completing such an investigation we will need to bear in mind that it is possible to do things to something without being immediately required to specify what it is one is manipulating. To put this less clumsily, intervention can take place in an ambiguous context, as we have noted. Part of the commitment to the nature of the thing comes exactly from what can be done to the thing, entity, or object without 'plumping' for the right definition. There is no predominance of theory over practice, or practice over theory—the two are defined and changed together. This is the 'ambiguity' of Gooding's epistemology. What I have not yet done is say how 'deep' this ambiguity can be before we lose any sense of coherence in the analysis of experiment given.

Consider, for a moment, how we investigate things that we know very little about. Perform a thought experiment.[127] In the near future NASA sends astronauts back to the Moon. On the Moon an object is discovered that is not a naturally occurring lunar feature nor something left by the original Apollo human visitors, and which has a complexity and appearance that suggests it cannot be given a simple physio-chemical explanation.[128] How would we try to work out what it is? We observe it, measure it, weigh it, turn it round, poke it, probe it, expose it to different environments, note its colour, its texture, its composition and structure, ... *and at the same time* wonder whether it is an artefact, a life-form, a fossil, or a weapon, a joke, a toy, a sculpture, a scientific instrument ... The theories are tested, but the general investigations are

[127] Although I do not address *thought* experiments in this thesis in order to keep the range of topics covered at least minimally manageable, I have attempted to keep them in mind in all my discussion of experiments.

[128] If this example is little too rich, consider a personal item, a watch for example, on an open moor where there should be none ...

not necessarily governed by these theories, it seems we would just *do* things to the object to see what happens. I would like to suggest however, that although the theories and the interventions and engagement with the object evolve in a loose and ambiguous way, we cannot really just do *anything* without some *metaphysical* commitment to the object's status. All the suggested descriptions above are for it being a singular, individual object. It may equally well be a city or a whole civilisation, and I submit, it would not be discovered to be such without a prior metaphysical commitment to its being a composite object that is more than the sum of its parts. Indeed, would we really just do things in the same way if we thought it may be alive, as when we believed it to be inanimate?

The point is that although talk of agency and ambiguity help to make some sense of intervention in experiments, they need to be properly analysed to do any work. The whole issue is tied in with how we might explicate experimental realism, for Hacking assumes that kinds of things are not manipulable at all.[129]

It is for this reason that I am focusing on biology for my own approach. Looking at a different science than the usual problems in physics, especially a science for which arguments supporting an ontological break with physics are not too hard to find, helps to clarify our understanding of practice at the experimental level and the supposed epistemological and metaphysical consequences claimed for experiments. Evolution through natural selection reveals a biological world of flux and change, and we shall have to look at the ambiguity of the kinds of the biological world. Once disunity of order at the metaphysical level is introduced, it may become impossible to support order in the methods used to experiment, because there is no obvious block to the thought that some kinds of things might be 'quirky' enough to require different or

[129] In Chapter 4 I discuss some important reasons for doubting this thesis, and the whole of Hacking's approach to experimental realism, except in so far as it highlights the 'doing' aspect of science and the epistemic role for action in science. Let us note, for now that only the entities of theoretical physics are covered by his theory as it stands.

special treatment.[130] The elimination of psychologically influential factors in experiments using sensitive laboratory rats need be nothing like the elimination of factors influencing a sensitive mass spectrometer.[131]

Dupré also sees a direct relationship between method and metaphysics in a wider context; hence he states:

> Since science does, presumably, presuppose some kind of pre-existing order in the phenomena it attempts to describe, limits to prevalence of order may entail limits to the applicability of science. Some areas of science may fail because the subject matter is inhospitable to scientific methods.[132]

What is presupposed here is that the methods of science define what are to be its limits. This is coherent with the evidence so far examined: the emergence of possibly more than one account of experimental method (or experimental method with no single list of sufficient features) and the generation of scientific *methods* does not damage the idea that these methods, although disjointed, can be justified rationally in context to be appropriate for investigation of a part of the world, in the way that common sense,

[130] Cartwright has questioned whether there needs to be a complete picture of the physical world covered by laws: 'Covering-law theorists tend to think that nature is well-regulated; in the extreme, that there is a law to cover every case. I do not. I imagine that natural objects are much like people in societies. Their behaviour is constrained by some specific laws and by a handful of general principles, but it is not determined in detail, even statistically. What happens on most occasions is dictated by no law at all. This is not a metaphysical picture that I urge. My claim is that this picture is as plausible as the alternative. God may have written just a few laws and grown tired. We do not know whether we are in a tidy universe or an untidy one. Whichever universe we are in, the ordinary commonplace activity of giving explanations ought to make sense.' Cartwright (1983) *op cit.* 49. This idea can be extended, the untidiness may apply to the kinds of things there are as well, categories may not stack neatly into law abiding hierarchies as philosophy of science supposes. Things are still real and explanation is still possible.

[131] I can find no research involving such a comparative study.

[132] *Disorder* 11.

> ... imposes order on the buzzing, blooming confusion of phenomena not by unifying them under a relatively simple structure of fundamental concepts, but by a piecemeal extension of knowledge.[133]

But it does not follow that this is going to be the only possible conclusion. As yet, the bones of this argument require more flesh, although for the moment only a sketch will be given. Not only is Dupré's work called into question, but the exact relationship between (scientific) intervention, in the form of experiment, and realism needs to be analysed further. An outline of Dupré's ideas first.

1 *Science and (dis)unity,* The Disorder of Things

Dupré's thesis is this:

> ... the disunity of science is not merely an unfortunate consequence of our limited computational or other cognitive capacities, but rather reflects accurately the underlying ontological complexity of the world, the disorder of things.[134]

> The conception of an ordered nature and a unified science belong naturally together. If there is some ultimate and unique order underlying the apparent diversity and disorder of nature, then the point of science should be to tell the one story that expresses this order. If ... we reject the assumption of *any* such systematic and universal underlying truth, not only should we give up the specific version expressed by contemporary materialism or physicalism, but the motivation for any unified account of science becomes questionable.[135]

The thesis breaks down into three parts concerning essentialism, reductionism and determinism, respectively. In describing the status of natural kinds and essentialism in science, Dupré claims that there is no requirement that we see the things that science describes as belonging to only single kinds that define the essence of that thing. He does not deny the existence of natural kinds, only that they have an order to them that categorises the world neatly for (and by)

[133] *ibid.* 19.

[134] *ibid.* 7.

[135] *ibid.* 221.

science into one scheme. His argument draws heavily on biology and ecology to show that there may be good grounds for classifying particular organisms, processes and populations in a number of different ways. He argues that there are many ways of classifying the (biological) world that can be shown to have validity, and to be real. 'And that these may often cross-classify one another in infinitely complex ways.'[136] This picture of a 'promiscuous realism' (Dupré's label for his position) of natural kinds is supported by, and supports his second thesis that inter-theoretical reductionism in science is untenable. No strategy can be constructed to reduce the terms of one field of science to a more fundamental one because of the plurality of the entities at each supposed level. He shows how the reductionist project fails in genetics, ecology and physicalistic theories of the mental. This anti-reductionism depends on a particular view on determinism and causality in which he insists on a rejection of the notion of causal completeness and a specific understanding of probabilistic causality. Much of his argumentation draws heavily on biology, because, as he says:

> Biology is surely the science that addresses much of what is of greatest concern to us biological beings, and if it cannot serve as a paradigm for science, then science is a far less interesting undertaking than is generally supposed.[137]

Even if this is granted, his view of biology could seem a restricted one. The reliance on discrepancies in taxonomic practice and common sense that is the major source of support for the first part of his argument where taxonomy is made to appear a central task for biology, presents a picture of biology that is at odds with other contemporary philosophical accounts.[138] However, if these problems of evidence are solvable, and (at least as far as the theory of natural kinds and reductionism is concerned) I believe they are, then we are left with the begin-

[136] *ibid.* 18.

[137] *ibid.* 1.

[138] See, for example, Rosenberg, A. (1985) *The Structure of Biological Science* Cambridge: Cambridge University Press.

nings of a theory from science and philosophy that undermines the supposed ontology of much philosophy of science. I say the beginnings of a theory because there are gaps in Dupré's argument that can only be filled by exploring further some of the consequences of this disunity. This examination of biology and experiments will go some way to completing the picture and gives additional support to some of Dupré's claims.

2 Metaphysics and method, metaphysics and epistemology, scientific realism

Dupré calls himself a realist. By which he means that he:

> ... can see no possible reason why commitment to many overlapping kinds of things should threaten the reality of any of them. A certain entity might be a real whale, a real mammal, a real top predator in the food chain, and even a real fish. ... I do not see why realism should have any tendency to cramp one's ontological style.[139]

This definition only goes so far. What he says I take to be correct, but we learn nothing about what sort of realism he is articulating. This becomes clearer when we also consider thoughts about the nature of the relationship between realism and our ability to make things up with descriptions (as opposed to just discovering those things).[140] He does make a passing approving reference to Hacking's entity realism in experiments, which ties some of what he says about ontology to Hacking's discussion on experiments and realism. However, I suspect that this is a commitment only to the notion of manipulation described here. Significantly, at the point where ontology meets methodology in the sketch that has been presented here, that is, over the nature of scientific realism, there is extreme difficulty in extracting a clear understanding of realism. This is the point where the wondering about metaphysical disunity can play a role in understanding methodological disunity, because it is the kinds of things that are

[139] *Disorder* 262.

[140] Susan Haack has identified a number of attitudes to scientific realism, a number of different realisms (Haack, S. (1987) "'Realism'" *Sythnese* 73, 275-299). Hacking's distinction between realism about theories and realism about entities is not discussed.

taken to exist and present 'resistance' to our activities in informative ways that will, philosophically speaking, give us the starting point for saying what it is we do to find in that resistance and interpret it. One way of taking this point of view seriously is just to say that this kind of scientific realism is derived from the general metaphysical realist approach that assumes a priority for the world, that is ontology, over other issues of semantics or epistemology. This does not require an *aprioristic* first philosophy, or foundationalism. Michael Devitt describes the objective existence of entities so:

> To say that an object has objective existence is not to say that it is unknowable. It is to say that is not *constituted by* our knowledge, by our epistemic values, by our capacity to refer to it, by the synthesizing power of the mind, by our imposition of concepts, theories or languages.[141]

Now obviously Hacking's position is not much like this, and Gooding looks not like a realist at all by these lights. Pickering has claimed that considerations of practice resolve these old questions; he says:

> There is no mystery of empirical access, to put it another way—facts and conceptualizations (and many other elements of scientific culture) are built together along the lines Gooding and Hacking lay out—and no special scientific realism is needed to explain it. ... the examination of scientific practice promises to undermine entrenched positions—here realist and antirealist alike—in the philosophy of science-as-knowledge.[142]

But can this be correct? Certainly there is a sense in which the position of the scientific realist as simple ontological realist becomes difficult to maintain without damage from analyses containing slogans like 'if you can spray them then they are real' and 'natural phenomena are bounded by human activity,' but it is not obvious that other forms of the realist debate are dispensed with. Again, these are issues we will return to as the discussion proceeds.

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[141] Devitt, M. (1991) *Realism and Truth* Oxford: Blackwell. 15.

[142] Pickering, A. (ed.)(1992) *Science as Practice and Culture*. Chicago: University of Chicago Press, 11.

In *Realism Rescued* Aronson, Harré and Cornell Way have begun a reconstruction of scientific realism that criticises the simple ontological theories.[143] They claim that taking on board analysis of practice can actually help revive an epistemic form of scientific realism. I shall return to a couple of their less dubious insights right at the end of the next chapter.

vi Interlude

We have a lot of questions then. So far I have presented a series of linked sketches about experiments, philosophy of experiments and philosophy of science. These have been selective and of necessity somewhat brief. In summary:

- Experiments are an essential component of many sciences. To date, however, there has been a lack of clarity about the nature of experimentation in general. I argued that while the idea of manipulation to uncover some 'hidden' aspect of the material agency of the realm of enquiry is a central concept in experiments, it becomes a matter of general and deep philosophical concern to state the relationship between our being material beings able to manipulate the world in certain ways and our using that for epistemological purposes. I suggested that there are no sufficient epistemic conditions to specify what experiments are beyond this observation.

- From an initial analysis Hacking's notion of experiment was seen to be flawed since it placed too great an emphasis on the creation of new phenomena, which need not be part of a general strategy of manipulation, even in a scientific context, and appears to lack wider application beyond theoretical physics. However, Hacking's highlighting of the epistemic value of intervention is the basis of the current approach. Hacking's entity realism sup-

[143] Aronson, J. L., Harré, R. and Cornell Way, E. (1994) *Realism Rescued* London: Duckworth.

poses that it is clear how we draw a distinction between theory and practice. Only entities are proved to be real by experiments. Again, this poses great difficulties in spelling out what kinds of things can be seen to be real in this way, since some entities could be thoroughly embedded in theoretical descriptions of the world *and* be used in experiments.

- It was suggested the current concerns for experimentation are not necessarily because of the expansion of research into the metaphysics and epistemology of science, but a product of a change in philosophical perspectives. One such change is reflected in Kitcher's dynamic approach to the epistemology of science. However, combining dynamism with the idea that scientific experimenters might not decide the status of the things they are investigating until after the experiment is completed and recorded, leaves us with some difficulties with the notion that *something* must be taken to be the metaphysical basis for asking 'questions of nature' in the first place. This seems true even when the traditional idea of having a fixed theory to test is rejected. This becomes especially troublesome when taken with the suggestion that there may be no one privileged ontology, but many well-founded ones in different scientific disciplines.

- Sociological, historical and Bayesian perspectives are inadequate for understanding experiment alone. *SSK* lacks any mechanism for assessing the epistemic component of any experiment (or theory). Bayesianism, formal problems aside, is inflexible and inappropriate to many contexts of experimentation.

- The disunity of science can be understood to reflect a disunity in the world. There is a natural sense in which one would expect there to be an epistemic and methodological outcome from this, but resolution of this issue requires a better understanding of the kind of realism extractable from science practice.

- Work to date on experimentation and the philosophical lessons that can be learned from such work has concentrated, almost exclusively, on the physical sciences, and that this is a serious problem in trying to resolve the problems encountered.

vii Examples

Having looked at general features of experiments and the philosophical issues that they can reveal, it is now time to turn to look at experimentation in biology specifically. In this section I shall look at a series of contemporary case studies in a range of biological fields. They will help to draw out some similarities and differences between the physical and biological sciences.

Biology has a tradition of 'great' experiments that can compete with the history of physics' own famous practical investigations and demonstrations. Certain experiments stand out for their contribution to biological knowledge—Mendel's peas are perhaps the most discussed vegetables in modern history. Harré's *Great Scientific Experiments*[144] contains accounts of seven experiments that have direct bearing on biology and the related fields of medicine, psychology and physiology including Stephen Hales' investigation of the circulation of plant sap, William Beaumont's grizzly observations on the digestion of food substances in the stomach of an injured army porter, and Pasteur's work on vaccines. Harré uses these to support some general claims, picking out various uses of experiments in the context of discussing scientific method. There can be no doubt that if we abstract the method of any two practical procedures far enough we could pick out similarities of conduct and form: something I have already done, of course. Care must be taken in ensuring that the similarities that are extracted by such a

[144] Harré, R. (1983a) *Great Scientific Experiments: Twenty Experiments that Changed our View of the World* Oxford: Oxford University Press.

method are significant enough to carry the philosophical load that is placed on them, and it is not clear that uses of experiment that Harré discusses constitute a complete method at all. Certainly they could not give us a method that is susceptible to change, progress and development since he draws on experiments from Aristotle to Otto Stern in the 1920's. More work is required to complete the project of extracting a binding method from experiments as Harré presents them, even when other general observations about inductivism and realism are added.

What we need to ask is: to what extent do experiments in biology share the epistemological aims and consequences of the physical sciences and what does this tell us about the status of biological knowledge? To this end I would like to look at three experimental investigations in contemporary biology to avoid accusations of anachronism. The first, specific uses of X-ray microanalysis, has many similarities with the HEP experiments discussed with reference to Franklin's Bayesianism[145] and highlights how biology in a modern context certainly can involve the use of complex, difficult (and expensive) apparatus, and the manipulation of unobservable entities. The second addresses some of the issues I later discuss in connection with Foucault and realism[146]—I shall look at recent work on the genetic factors involved in human sexuality, specifically human sexual orientation. Finally, I shall turn to the work on plant breeding and selection where observable and unobservable traits have been manipulated and used, but without allowing us to come to the conclusions that Hacking draws about realism.

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[145] See p. 57 ff.

[146] Chapter 3 below.

1 Experimental apparatus and techniques: X-ray Microanalysis[147]

The basic technique for determining the dispersion of diffusible ions and the elemental composition of specimens with electron probe X-ray microanalysis was invented in 1951,[148] and has been used in the biological sciences for approximately thirty years. In 1988 the Biological X-ray Microanalysis group was founded in Britain so that techniques and innovations could be more formally compared and shared.

> There is considerable interest in the application of X-ray microanalysis to the study of biological specimens since this is one of the few techniques which allows the study of diffusible elements at a subcellular level.[149]

There have been many developments in the equipment involved in the technique, which I shall outline below, but since,

> [b]iological specimens are many and varied with their own special problems for quantification ... perhaps the second most important development in X-ray microanalysis for biologists has been the commercial availability of software for carrying out quantitative routines.[150]

A great deal of development of the technique in terms of the mathematical analysis behind such software was carried out by T. A. Hall who has also recorded the history of the uses of X-ray microanalysis,[151] which I shall not cover here.

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[147] All references in this section are to *X-ray Microanalysis in Biology: Experimental Techniques and Applications* [(1993) eds. Sigee, D., Morgan, A. J., Summer, A. T. and Warley, A. Cambridge: Cambridge University Press] unless otherwise stated. Individual papers will be referred to directly with page references to the collection with the abbreviation *XRMB*.

[148] Castaing, R. and Guinier, A. (1949) 'Application des Sondes Electroniques a l'Analyse Metallagraphique' in *Proceeding of the 1st International Conference in Electron Microscopy* Delft, 60-3.

[149] Warley, A. (1993) 'Quantitative X-ray microanalysis of thin sections in biology: appraisal and interpretation of results' *XRMB* 47.

[150] *XRMB* 1.

[151] Hall, T. A. (1986) 'The History and Current Status of biological Electron-probe X-ray Microanalysis *Micron and Microscopia Acta* 17, 91-100.

I wish to draw the attention of the reader to this variety of experimentation in biology for a number of reasons. In the introduction to Section A of *XRMB*, 'Detection and Quantification of X-rays,' the editors note:

> The living biological specimen typically consists largely of water, which needs to be removed or stabilised before examination and analysis in the electron microscope can take place. This removal of water and/or its replacement by resin can lead to some uncertainties in the interpretation of data. It is, essential that all results are viewed with a cautious eye.[152]

Here in the heart of biological research we have the same philosophical problems faced by the physicists working on the problem of parity in HEP that Franklin discusses, *viz.* the rational interpretation of data from complex procedures using unobservables, the elimination of random fluctuations and artefacts, and the degree of acceptability for the interpretation of data. Furthermore, we find the kind of uses and manipulations that Hacking discusses, although the reality of, say, potassium ions is not called into question in the same way that the entities of HEP are.

X-ray microanalysis is basically a form of electron microscopy using highly sensitive (and specifically tuned) equipment based on the use of x-ray penetration of the subject matter. A vast array of variable factors have to be manipulated and controlled. For example, organic tissues can potentially suffer damage under x-ray bombardment. Consequently the specimens used have to be fixed and prepared in a similarly tight range of controlled method. These include freeze-drying and resin embedding along with the use of a host of cryogenic techniques. So there have to be checks not only on the radiation damage from the microscope itself, but also on the preparation techniques.[153] The use of any form of x-ray analysis or electron probing includes a number of skills and practical decisions to maximise the expected outcome. In

[152] *XRMB* 1.

[153] Zglinicki, T. von (1993) 'Radiation damage and low temperature X-ray microanalysis' *XRMB* 117-133.

the following passage the author is clearly outlining *practical* adaptations based on decisions taken in an experimental context exactly as in HEP experiments:

> There are applications in which the rapidity of sample preparation by the frozen-hydrated bulk technique outweighs the limited resolution. However, there are many cases where quantification at high resolution is required. The answer is to use a transmission microscope in which the beam passes through a section whose thickness can be determined. Thin sections can, however, only be prepared from resin embedded plant material and usefully analysed provided care has been taken to preserve the mineral content of cells: this can be achieved by freeze-substitution. The freeze-substitution procedure, together with dry sectioning, has been shown to be a satisfactory method of cell preparation in that there is little relocation of ionic solutes during preparation procedure.[154]

What I think is clear is that, as already suggested, there are no grounds for *not* applying whatever analyses are needed to track the rational decision procedures involved in HEP experiments to biological experiments of this sort. The appropriateness of the same kind of analysis demonstrates that there are techniques that we employ for gaining knowledge that are not limited by the sorts of things under investigation. In cases where we are using such complex settings for our empirical enquiries we are attempting to get the experimental set-up as mechanistic and machine-like as possible.[155] At the level of method this tells us little about the metaphysical assumptions we need to make about the sorts of things investigated. More will need to be said about this later.[156]

2 Plants Under Stress

A different sort of manipulation of organisms can be found in the range of breeding experiments carried out on plant populations. Here there are a large number of unknown and uncon-

[154] Hajibagheri, M. A. (1993) 'Ion localisation in plant cells using the combined techniques of freeze-substitution and X-ray microanalysis' *XRMB* 227.

[155] Perhaps, simply, because the specimens are all dead and the technique is one for the measurement of fixed properties.

[156] See, pp. 153ff., 190ff. and 202ff.

trolled factors. For example, in a paper discussing how plants are bred for drought resistance we find that complex statistical models have traditionally been used to determine the best condition in which to test the crops and the yields expected.

> The intricate statistical and biometrical designs necessary for this approach are required for two reasons. First, the inheritance of yield, which is the major selection criterion in such programmes, is 'complex' as it is determined by a multitude of physiological, biochemical and metabolic processes, whose genetics are largely unknown. Even their exact association with plant productivity is unclear. Secondly, the various environments under which genotypes are tested and under which stability is measured are largely undefined in the biological sense.[157]

Notice the use of terms and properties that are specifically biological such as 'yield,' 'inheritance' and 'genotype.' The question of whether such properties and terms can be reduced to purely physical terms will be discussed in the next chapter. Blum goes on to suggest that in determining the resistance of plants to stress, yield is an inefficient criterion. He argues that it is better to measure certain physiological factors and use them in combination with yield measurements.

> Although drought-resistant varieties have been developed by the use of empirical breeding methods that employ yield as a selection index, these methods are too costly and require a long period of testing and evaluation. ... Therefore breeding for drought resistance must depart from the use of yield as the exclusive selection index. A direct reference to some physiological attributes in the selection for drought resistance would allow us to address the underlying factors of stability, to the same extent that selection for, say, disease resistance addresses the specific plant interactions with the pathogen rather than yield.[158]

In other words, because the yield of a crop is not a good measure of resistance to damage during drought nor an adequate indicator of recovery, physiological features that have impact

[157] Blum, A. (1989) 'Breeding methods for drought resistance' in Jones, H. G., Flowers, T. J. and Jones, M. B. *Plants Under Stress* Society for Experimental Biology, Seminar Series 39, Cambridge: Cambridge University Press, 198

[158] *ibid.* 199.

on water systems in the plants must also be considered and selected for by the breeder. We see that this means that the breeder must pick out plants and select on the grounds of root growth rates and root dispersion in the ground, osmoregulation processes in cells, canopy temperature and leaf rolling. Magnesium chlorate is used in a 4% solution as a desiccant to simulate drought condition for crops during selection experiments. The details are not important to the point I wish to make.

In cases like these, what is being manipulated in the experimental context is not individual plants, but particular properties of generations of plants—even though selection is of individuals. Now, if Hacking's argument for experimental realism has any bite we need to see whether it can include cases where we manipulate and use *properties* as well as entities.[159] While selecting for good root growth rates, for example, the breeder could use the property 'has new root growth rate greater than *n* mm/day.' This is used in the experiment to understand the property of drought resistance. Indeed, the argument can be pushed further. Drought resistance is used by breeders to develop new varieties of crops, which are themselves manipulated for other purposes to produce further new varieties. Does this then give us an *experimental* proof of the reality of these crop varieties as kinds?[160]

This last question might seem easy to answer in the affirmative. But it presents us with a problem in drawing lines between those kinds which are real, those we construct and are real, and those we construct and are *not* real. If we take the above example as a demonstration of the construction of kinds because we can construct a model of the properties of varieties of crops under different conditions and test it, what are we to make of the search for a biological basis for homosexuality in humans? Could a testable model demonstrate the existence of gay

[159] If it cannot, then we need some good criteria for the scope of the realism he advocates.

[160] Again, I ask for the reader's patience. In Chapter 4 I shall address how best to deal with Hacking's thinking here.

people per se? I shall now outline recent research in the attempts to do just that, in this case the search for hereditary factors and the hunt for a 'gay gene.'

3 Gay genes

On 16th July 1993 *Science* published an article titled 'A Linkage Between DNA Markers on the X Chromosome and Male Sexual Orientation,'[161] which presented evidence intended to show that a genetic marker labelled Xq28 was statistically similar in gay men compared with other straight male relations. Xq28 is a region of the X chromosome. The study that was conducted was fairly simple. A survey was used to determine the prevalence of homosexuality in families where there was a living gay male. As Hamer and Copeland put it:

> We simply traced back the lineages of gay men, looking for signs of homosexuality in all the twigs and branches of their family trees. We drew orchards of these tress, going back as far as anyone could remember and stretching as wide as possible to include second cousins and great uncles.[162]

Discovering 'far more gays on the mother's side of the families'[163] suggested to the experimenters that there is a sex linked genetic factor tied to the X chromosome. So they conducted a study involving gay brothers only and discovered that indeed there was a significant correlation between the similarity of their Xq28 regions. I do not want to trace the whole political and social issues that are involved in even needing to look for such a genotypic trait. And, again, I do not want to explore all the details of the experiment. I call this an experiment because there were a number of factors in the model that was used to find the 'gene' that had to be changed and manipulated. That is, although people themselves were not manipulated, a whole series of

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161 Hamer, D. H., Hu, S., Magnuson, V. L., Hu, N. and Patteratucci, A. M. L. (1993) 'A linkage between DNA Markers on the X Chromosome and Male Sexual Orientation' *Science* vol. 261, 321-327.

162 Hamer, D. and Copeland, P. (1994) *The Science of Desire: The Search for the Gay Gene and the Biology of Behaviour* New York: Simon and Schuster, 20.

163 *ibid.*

self-reported properties had to be modelled and the model adjusted to construct the category of 'gay male' with a profile of expected properties.[164] To put this crudely, this kind of thing was then used to demonstrate the correct reading of a further measurement—the correlation of Xq28 in brothers falling under the same category.

To restate the issue I am trying to get, the range of experimental contexts in the biological sciences is vast. In many cases we can find obvious similarities between experiments in the physical sciences (x-ray microanalysis) showing the underlying epistemic virtue of experimentation in science. But we can also find experiments that create entities and kinds of entities that undermine a simple reading of realism from experiments. Indeed as we 'approach' the human sciences and human concerns all sorts of other issues come into play, and dealing with these has had a past tendency to threaten the very rational basis of science.

In Chapter 4 we shall see how Hacking has attempted to deal with these kinds of problems. For now they remain unresolved.

[164] The survey questions included: 'Have you ever been sexually abused?' and 'Have you ever had a problem with drug use?' the relevance of which I find incomprehensible.

Haiku

June rain drips from leaves—

Shake the branches and create

Flying water worlds.

3 The Metaphysics of a Pluralist Biology

> [T]he realist can quite coherently accept the pluralist conception of scientific categories even within a single scientific discipline ... the realist could acknowledge that for every particular scientific program there is an infinite plurality of appropriate conceptual schemes that fit the causal structure of the world equally well and between which the choice is arbitrary.[165]

i Introduction

The supposition that *everything* is real, or unreal in some way is superficially attractive as a fundamental belief in this regard: one does not have to say how it is one can tell what is real, or unreal, or state simply which bits are real and which bits are not. The history of philosophy is littered with abandoned forms of this doctrine. There is something unsatisfactory in any absolute theory that ascribes the same general metaphysical status to Prospero, *The Tempest*, the copy of the complete works of Shakespeare on my desk, the theatre in which I saw the play, the light that lit the stage, the genetic history of the actors, and the storm that raged outside. The nature of the world, its status, structure and internal coherence is the oldest of philosophical problems. Questions about how particular parts of our contemporary knowledge and practice are to be understood with regard to claims about the real world are manifestations of one of the oldest mysteries: what is there?[166] But as James notes, 'impressionistic philosophizing, like impressionistic watch-making or land-surveying, is intolerable to experts.'[167]

[165] Boyd, R. (1990) 'Realism, conventionality and "realism about"' in Boolos, G. (ed.)(1990) *Feshschrift for Hilary Putnam* Cambridge: Cambridge University Press, 189-90.

[166] It seems to me in moments of reflection that there is really only *one* mystery: why there is anything at all. Worries about consciousness, theoretical scientific entities, moral values and

Pluralism about the kinds of things there are, how they might be related and how they interact is less attractive because it is untidy and can also requires the rejection of foundationalism and certainty. One can never be sure that everything has been included, or that this is best way to describe the nature of things—this is how it should be. We are not gods. In this chapter I shall set out an account of only one way that we understand ourselves and our relationship to the rest of the natural world: the beginning of how we understand ourselves as *biological* beings.

The study of life and its environment has reached a turning point, a transmutation that is the product of no single theory or individual scientist's work, but the splicing together of many fields of enquiry. New Age musings about a technologically informed, ecologically sensitive future where the human spirit finally converges with the Earth, (with Nature, with the goddess?) after millennia adrift are dim metaphorical reflections of emerging biological and environmental theories; theories that have wide ranging conceptual and philosophical, consequences.[168] Big changes in grand schemes almost always have unforeseen fall-out.

1 Explanation

Let us for the moment accept that the world has some kind of thought-and-word independent structure, and that, in theory at least, we can conceive of ways of generating descriptions of what that world is like—even while we also accept the potential for failure in all

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the like (choose your favourite troublesome category) are only forms of *this* question. Philosophy can clarify the question and also aid in the unending pursuit to solve the puzzle; sometimes it seems closer to the heart of the solution than at other times, but the discipline of philosophy always pushes me on by presenting fresh ways of thinking about this mystery.

[167] James, W. (1909) *A Pluralistic Universe* New York: Longmans, Green and Co., 52.

[168] For one such New Age claim about the ability of the biosphere to reproduce through technology see Sagan, D. (1990) *Biospheres: The Metamorphosis of Planet Earth* Harmondsworth: Penguin, Arkana. I am intrigued by Sagan's argument and do not want to dismiss all ideas that employ an explicitly metaphorical mode of expression—analytic philosophy's metaphors are just better hidden.

our current best theories.[169] Should we envisage that the best explanation of any event or phenomenon in our limited experience of the universe at any particular time is one that 'fits' that event into an expanding, *single* and *unified* description, model or theory? Supposing I want to understand why my cat suckles her live-born young whereas this fly lays eggs and abandons them. Of little value is a *total* theory with no discrimination between its parts—God made things that way. But the entirely specific accounts of the individual life histories of my cat and the fly tell me nothing if they make no attempt to fit the individuals into some larger picture of the kinds of things they each are. An account of the evolutionary history of mammals and insects as a whole would seem better. Better still would be a theory that could show how these kinds evolved from common ancestors that themselves shared a history, taken together with a rigorous presentation of the mechanism(s) behind natural selection and the accidental environmental changes in the history of Earth, and so on. Unfortunately at this level of explanation there are of course all sorts of debates about how much a role adaptation has to play in determining the cat's and the fly's current forms and behaviour. A clear understanding of what life is, and what the relationship between biology and chemistry might be, then seems an added bonus. And yet how exactly would *this* level of general abstraction contribute to the resolution of my original concern and curiosity?

It would seem that there is an intimate relationship between explanation, realism, pluralism/unification and reductionism. A thorough-going scientific realism about the possibility of many correct and diverse ontological schemes describing the contents of the world, supported by the very sciences they are used to underpin seems to run against the idea that explanation of events and phenomena should aim to unify our understanding of the world

[169] See p. 210 ff. for my account of what this means.

in an increasingly comprehensive picture. Kitcher[170] has promoted an approach to explanation that stresses unification, seeing it as the underlying theme of earlier, more formal, accounts of explanation. This fits with a background and history of thinking on explanation well entrenched in philosophy and philosophy of science in particular, as Herbert Feigl noted,

> The aim of scientific explanation throughout the ages has been *unification*, that is, the comprehending of a maximum of facts and regularities in terms of a minimum of theoretical concepts and assumptions.[171]

With the increasing complication of accounts of the relationship between theories and explanation, the ontological components and commitments involved in this general aim for explanation have become more significant.[172] In many cases we can see that it is in grappling with explanations that metaphysics and epistemology are combined, providing a good testing ground for the coherence of any philosophical theory about the special sciences. Making sense of (or revealing as unnecessary) the apparently intuitive pull towards unification would seem to be a key feature of any attempt to construct a philosophy of science that supports the independence of the special sciences in a non-trivial way.[173] Obviously

[170] See, for example, Kitcher, P. (1981) 'Explanatory Unification' *Philosophy of Science* 48, 507-531.

[171] Feigl, H (1970) 'The "Orthodox" View of Theories: Remarks in Defense as Well as Critique' in Radner, M. and Winokur, S. *Minnesota Studies in the Philosophy of Science* vol. IV Minneapolis: University of Minnesota Press, 12.

[172] A glance through recent collections of papers on explanation confirm this. See, for example, Pitt, J. C. (ed.) (1988) *Theories of Explanation*; and Ruben, D-H. (ed.) (1993) *Explanation*, both Oxford: Oxford University Press. The problem of what ontological commitments in theoretical accounts of explanation one is prepared to make is highlighted by the old debate over the role of causation in science, see Salmon, W. C. 'Scientific Realism and the Causal Structure of the World' in Ruben (1993). See also Salmon, W. C. (1989) *Four Decades of Scientific Explanation* Minneapolis: University of Minnesota Press.

[173] Defending the notion that explanation is a central aspect of any field of science would be too much of a digression from my main argument to be considered in any depth here. However, the support I give to a realist philosophy of science and to supporting the operation of rationality, at least in abstractions about the spheres of human interest in which science operates, gives confirmation to the idea that science has many roles in the articulation of an order in parts of the Universe that matter to us. One of these roles can be assumed to be explanation of the par-

there are some very complicated issues here. What *kind* of unity is being argued for by the supporters of explanatory unity? And if one is a pluralist about ontology does it follow that one must be a pluralist about method and reject explanatory unification? Does explanatory unity even make sense? The relationship with reductionism is curious and subtle. If total explanations are vacuous why would anyone want to have a single explanatory story and ontology?

One way that the unificatory pull of explanation can be understood is as the expression of a mechanistic picture of the Universe as a whole. Small parts of the Universe have to be understood to explain the large parts—and not *vice versa*. The mechanistic model has a long and fairly distinguished history. But it is no longer a useful metaphor. The following examination of biology and ontology should help to show why.

I mention all this by way of an introduction to this section for two reasons. Firstly, to make it clear that in looking at biology and juggling the philosophical problems it generates, its status and relationship to science as a whole and the other special sciences, I am trying to keep many balls in the air. I do this deliberately. I do not suppose that any single philosophical starting point can be given priority over the others. Secondly, I have raised all these issues in recognition of the fact that the consequences of taking up particular positions on epistemology and metaphysics are always larger than perhaps anticipated.[174] At the end of the last chapter there were a number of issues left unresolved. These included how we might understand the role of experimentation in biology, given that there clearly is

ticular events and phenomena that make up this experience. Having said this, I shall assume that explanation is reasonably clear for the discussion below.

[174] As already noted, this is not a thesis about explanation, and I shall not discuss explanation *per se* in much detail, but I am sensitive to the challenge that there is a real need to work out some sort of stance on explanation that is coherent and consistent with the rest of my philosophy of science.

an epistemic value in performing experiments. I want to change tack a little on these questions to examine biology itself.

In this chapter I want to explore some important issues in philosophy about biology—rather than philosophy of biology—particularly how we might see biology as an exemplar of the ontological and epistemological unity or disunity of science. My aim is to examine and articulate a pluralist account of biology working from the discussion of such an approach given by Dupré in *Disorder*, whilst retaining the methodological insights of the previous chapter. In order to do this I shall look at reductionism first, and examine its relationship to the unity of science. To facilitate this I shall sketch out a 'minimal' approach to unity through explanation and reduction in order to test the challenges of Dupré's thesis against something that is not so strong as to be absurd. Then I shall turn to essentialism and the nature of natural kind terms in biology. Finally, I shall explore some of the criticisms raised by Rosenberg to the claim that biology, once appreciated in all its complexity, can still be treated as realistic science. This leads me to an examination of a realistic theory of natural kinds that is detailed enough to support realism for biology, which in turn helps answer some of the problems at the end of the last chapter.

2 *What is biology? Philosophy, biology and physics*

The last chapter concluded with three examples of different biological investigations. They covered a range of biological research. So far I have spoken as though there is an obvious way of picking out what are biological issues from other concerns. Is this in fact an easy matter, one of convention, or natural kind analysis, or explanations of a particular sort? This question is not as simple as it may at first appear, and I shall touch on it again later when discussing the emergence of biology from natural history in Foucault's characterisation of the science's development.[175] To begin with I want briefly to look at what are taken to be the central *philosophical* issues for biology to see if they might illuminate the issues at stake here. Amongst these issues are some important epistemological and metaphysical problems that are of direct relevance for advancing the discussion about experiments. Rosenberg says that,

> [b]iology is not a physical science. Is this statement merely a truism, reflecting a bit of nomenclature? Does it mark merely an administrative boundary between scientific disciplines? Or are the life sciences different from physics and chemistry in respects important enough to turn the truism into an important conclusion about the nature of the different subjects of these disciplines and the different means appropriate for studying them? This is the central question of the philosophy of biology.[176]

Rosenberg regards the uniqueness, or otherwise, of biological enquiry as the key issue in philosophy of biology. Interestingly, no-one ever suggests that the central question in the philosophy of physics is whether and how physics might be distinguished from other sciences. There are several possible reasons why one might take biology's difference to be the starting place for an examination of its fundamental conceptual content. One is a simple insecurity, reflecting a worry about the possibility of the elimination of biology as a special science through a crude wholesale reduction of its laws, theories and observations via chemistry to 'fundamental' physical laws.[177] However, since naive reductionism is not as healthy a programme as it once was,[178] other motivations for such an understanding of philosophy of biology should be sought.

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175 See p. 172 ff.

176 Rosenberg, A. (1985) *The Structure of Biological Science* Cambridge University Press, Cambridge, 13.

177 Is the key question in the philosophy of chemistry 'How do we distinguish chemistry from physics?' I suspect not.

178 See p. 99 ff.

A second reason is that the parent discipline is philosophy of science. The fact '... philosophy of science has been too driven by physics ...'[179] does not need to be argued for anymore. In a symposium on 'The History of Evolutionary Biology' at the 1995 Annual Conference of the *British Society for the Philosophy of Science*[180] Jonathan Hodge argued that there was no longer any need for philosophers with an interest in biology to fear that they were committing intellectual suicide; that the subject was in a healthy and rapidly maturing state; that epistemic and metaphysical problems raised by the theory and practice of biology presented genuine and deep philosophical issues which are in need of greater appreciation and careful consideration. This must surely be correct. And from a contemporary, politically aware perspective recognition of the current sophistication and potential of biology and biotechnology is now a necessity. Philosophy cannot exist in a vacuum. The picture of biology as a local, terrestrial study lacking the universal appeal of the physical sciences in our total world comprehension is a prejudice for platonic tidiness. The impinging of biology on our lives, requires more of us than this quiet desire for order. That is, biology is interesting and relevant in itself. But this does not answer the question of why we should be concerned to distinguish biology from other sciences—if biology generates such interesting issues all of its own, why be worried about it not being physics? One answer should be obvious from what I have already discussed. We now have a burgeoning philosophical literature about the role of experimentation carrying forward debates about scientific realism in physics and we want to know whether the same arguments work elsewhere.

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[179] Sterelny, K (1995) 'Understanding Life: Recent Work in Philosophy of Biology'. *British Journal for Philosophy of Science* June 1995, 155.

[180] University of Leeds, 1-3 September 1995.

A third reason for looking at biology's relationship to other disciplines can be extracted from the second. By looking at biology it is just possible that we may learn things that can be applied elsewhere—even if the philosophical lessons are just that we should avoid applying unsuitable and irrelevant models from one science to another. That is, we may learn some things of more general *positive* philosophical value from the philosophy of biology. To this end Ernst Mayr can state that,

> ... the activity in the philosophy of biology has led to a new look at the core meaning of philosophy. For many reasons, the philosophy of biology is more than an exercise in logic. It has shown ... that there is a broad area of overlap between science and philosophy. Nothing characterizes this area of overlap better than the realm of concepts. Concepts, like *cause, selection, species, evolution, development, hierarchy, reduction, emergence*, to mention only a few, are of equal interest to the philosopher and the scientist. ... [P]hilosophers coming from logic and physics must realize that the physical sciences are ... just as provincial as is biology, and that some of the standard principles of the physical sciences are only partly—or not at all—applicable to other branches of science, including biology.[181, 182]

This helps us see that we cannot straightforwardly answer our original question about what biology is. Earlier I used a definition from *Collins English Dictionary*[183] which defined biology as 'the study of living organisms, including their structure, functioning, evolution, distribution, and interrelationships.' For philosophical purposes this will not do since it

[181] Mayr, E. 'Foreword' to Wolters, G., Lennox, J. G. (eds.) with McLaughlin, P. (1995) *Concepts, Theories, and Rationality in the Biological Sciences: The Second Pittsburgh-Konstanz Colloquium in the Philosophy of Science, University of Pittsburgh, October 1-4, 1993* Konstanz/Pittsburgh: UVK - Universitätsverlag Konstanz/University of Pittsburgh Press, ix.

[182] I would add that just what the 'overlap' might consist in is open to debate. Clearly the interests that philosophers and scientists have in looking at conceptual issues like those Mayr lists may well be very differently motivated, leading to a complex and perhaps never well-defined form of overlap. Of course this does not diminish the point that philosophy of biology has been tackling matters that are also of interest to philosophy of science as a whole, and in philosophy generally.

[183] See p. 20 ff.

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See p. 20 ff.

only pushes us back to an investigation of what life and organisms are.[184] But of course saying what the proper study of biology should consist in, and what the output of biology should be—what forms its knowledge production should take—is to presuppose answers to some of the difficult philosophical questions with which I am attempting to deal here.

At present, however, a great deal of philosophy of biology is only concerned with expanding, interpreting and refining concepts in evolutionary theory and the details of natural selection. For example, Sober's *Philosophy of Biology* begins with the sentence: 'This book concentrates on philosophical problems raised by the theory of evolution.'[185] And when Kim Sterelny claims to be surveying contemporary philosophy of biology he mentions the work of Dupré and Rosenberg on biology, realism and scientific unity only to say:

> Admirable though this work is, I shall focus on *philosophy of biology proper*, most especially evolutionary theory, which continues to dominate the philosophy of biology agenda.[186] [My stress.]

However, as emerges from Sterelny's survey article, getting to grips with the kind of metaphysical framework that taxes Dupré and Rosenberg could help resolve some of the difficulties that confront writers struggling with evolutionary theory, genes, species and groups. So let me briefly mention some of conceptual problems currently encountered in evolutionary theory in order to move on.

3 Philosophy of biology—some contemporary problems in evolutionary theory

Debates about evolution fall into two broad divisions:

[184] As already indicated (footnote 14) I believe that AL research can help in this respect by attempting to model some of the necessary conditions for life by creating other theoretical life systems independent of the contingencies of the history of life on Earth.

[185] Sober, E. (1993) *Philosophy of Biology* Oxford: Oxford University Press, xvii.

[186] Sterelny (1995) *op cit.* 155.

- external questions about the theory's relationship to religious beliefs about the origins of life on Earth and its status as a scientific theory, and:

- internal questions about interpretation, what should count as the component parts of the theory and about the consequences of its correct application to understanding a broad range of biological and other phenomena.

The external issues are well known. My views on the dangers of creation 'science' would certainly obscure any attempt to present a neutral account of the debate so I shall simply direct the reader to Kitcher's *Abusing Science.*[187]

The internal issues are what Sterelny refers to as 'philosophy of biology proper.'[188] Here we find much discussion over how fitness should be characterised,[189] adaptationism,[190] classification and systematics,[191] what the units of selection should be,[192] and how 'species'

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[187] Kitcher, P. (1983) *Abusing Science: The Case Against Creationism* Milton Keynes: Open University Press.

[188] Of necessity, the references I give here are limited. For an extensive bibliography of work in the philosophy of biology on both external and internal problems see Ruse, M. (1988) *Philosophy of Biology Today* New York: State University of New York. For a range of papers on philosophical issues in evolution and related topics see Brandon, R. N. (1996) *Concepts and Methods in Evolutionary Biology* Cambridge: Cambridge University Press; Hull, D. L. (1989) *The Metaphysics of Evolution* New York: State University of New York Press; for a more wide ranging, less deep selection from classic texts which also covers some of the 'external' issues raised above see Barlow, C. (ed.)(1995) *Evolution Extended: Biological Debates on the Meaning of Life* Cambridge, MA: MIT Press.

[189] See, for example, Rosenberg, A. (1983) 'Fitness' *Journal of Philosophy* 80: 457-473 and (1985) Chapter 6; Sober, E. (1984) 'Fact, Fiction , and Fitness' *Journal of Philosophy* 81: 372-384 and (1987) 'Does "Fitness" Fit the Facts?' *Journal of Philosophy* 84: 220-223; Mills, S. and Beatty, J. (1979) 'The Propensity Interpretation of Fitness' *Philosophy of Science* 46: 263-288.

[190] See, for example, Dupré, J. (1987) *The Latest and the Best: Essays on Evolution and Optimality* Cambridge MA: Bradford Books/MIT Press.

[191] For a discussion of philosophical issues by a zoologist see Panchen, A. L. (1992) *Classification, Evolution and the Nature of Biology* Cambridge: Cambridge University Press.

[192] Famously in the 'popular science' market Richard Dawkins has promoted the idea of genes as the fundamental unit of selection: see, for example, Dawkins, R. (1986) *The Blind Watchmaker* London: Longman. See also Brandon, R. and Burian, R. (1984) *Genes, Organisms, and Populations* Harvard MA: MIT Press; Wimsatt, W. (1980) 'Reductionistic Research Strategies and their Biases in the Units of Selection Controversy' in Nickles, T. (ed.) *Scientific Discovery* vol.

should be characterised as a general term. The last of these issues will be part of my discussion below. None of these problems can stand in isolation from the general metaphysical and epistemological questions I have raised. It is a pity that these general points are seen as peripheral since, as Mayr points out, biology can play a potentially informative role in all areas of philosophy of science.

The most obvious, and well discussed problem that confronts any examination of the biological sciences' relationship to physical science is over the philosophical and practical consequences of theoretical and explanatory reductions of various sorts. This is where I shall begin.

ii Reductions, explanations and (dis)unity

1 *The middle way*

> To be sure, reductionism is a conviction that long antedates the discovery of the chemical mechanism underlying genetic phenomena. Its appeal is based on an assessment of the history of science as one that reflects progress in our understanding of nature. Since Galileo the natural sciences have encompassed more and more phenomena; their theories have become deeper and more accurate in description and prediction; these theories have been bound together more and more closely; and their technological applications have enabled us to control more and more of our environment.[193]

Reducing the poorly understood to the better understood is a good way of explaining things. Unfortunately, sometimes reduction can be inappropriate. It often seems that there is no procedure for determining when the reduction of one description of a phenomenon,

2 Dordrecht: Reidel; Brandon, R. (1990) *Organism and Environment* Princeton: Princeton University Press; Hull, D. (1980) 'Individuality and Selection' *Annual Review of Ecology and Systematics* 11, 311-322.

[193] Rosenberg, A. (1985) *The Structure of the Biological Science* Cambridge University Press, p. 69.

entity or event to another is correctly applicable that does not beg the question over the nature of the subject matter to be explained. *This* I take to be the problem that is faced in the debates that erupt from time to time in philosophy and science about reductionism, whether this is about genes, minds or society. To say that reductions are never useful or appropriate is to abandon the general nature of explaining anything, that is, say why the newly encountered parts of our experience fit the pattern of the old and already described world. Any move from the particular to the general just does involve a reduction of sorts; information is lost; irrelevant facts disposed of; possible levels of description ignored. Sometimes this is helpful, sometimes it is not.

I have no intention of arguing against all reductions. I only wish to show why there are now good empirical and philosophical grounds to see it as inappropriate and detrimental to scientific progress to pursue it as a central tool for grasping biological phenomena—that is, the hope, the belief that the life sciences will one day be eliminated in favour of a comprehensive understanding of the physical (and mathematical processes) that govern such complex systems is vain, out of date and a block to understanding biological phenomena. This thesis I take to be fairly well supported in any case. The days of extreme reduction are long past. And we are all physicalists or materialists in the widest sense:

> Adopting a physicalistic view of the domain of biology simply means that one accepts the idea that living things are physical objects. It is important to realize that this thesis does not say what the relationship is between *biological explanations* and *explanations in physics*. Even if living things are made of matter and nothing else, the fact remains that the vocabulary of biology radically differs from that of physics. Physicists talk about elementary particles, space-time, and quantum mechanical states; evolutionary biologists talk about phylogenies, ecosystems, and inbreeding coefficients. Even though the domain of biology falls within the domain of physics, the vocabulary of biology and the vocabulary of physics have little overlap. Explanations in biology are produced in the distinctive vocabulary of biology; explanations in physics use the distinctive vocabulary of physics. The question is how these two kinds of explanation fit together.[194]

Fitting the explanations together is indeed a difficult problem. I shall begin with one such attempt that results in a picture that purportedly supports a minimal conception of scientific unity as 'explanatory interfacing.' This will serve as a test case for arguments against unity, in favour of disunity. I then follow this with a sketch of specific arguments by David Hull which have formed the backbone to anti-reductionist accounts of genes and molecular biology[195]—Hull has shown that there can be no possible match between the parallel readings of 'genes' as molecular entities, parts of an organism's chromosomal DNA, and as units of heredity and selectable traits. Rosenberg has shown some important consequences of Hull's position and Dupré has drawn on it too, and I examine their different arguments here. Dupré has further strengthened anti-reductionism in biology through an analysis of ecology and its relation to population dynamics. I sketch out and consider this argument too. This leads on to my stating a clear position about reductionism and biology that incorporates some of the arguments employed in Dupré's characterising of the discussion. Fi-

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[194] Sober, E. (1993) *Philosophy of Biology* Oxford. Oxford University Press, 24-25.

[195] My understanding of how contemporary researchers in molecular biology view this debate has been greatly enhanced through e-mail conversations with Dylan Sweetman, a doctoral research student in this field at Warwick University. It interested me to note that, as one might expect, our interests in what genes are and how they can explain things differed. What surprised me was that a researcher in the field of genetics did not see the potential political and economic difficulties that his work could generate.

nally, I shall look at how this supports and is supported by the rest of my discussions on the metaphysics and epistemology of biology.

It should be clear by this stage that an anti-reductionist stance underlies many of the points I have raised about the biological sciences, but it does not negate the possibility of methodological unity. From what follows it will emerge that my defence of this anti-reductionism does not stand entirely alone, separate from the other elements of my argument. Scientific unity need not collapse with a pluralistic approach to the metaphysics biology.

2 'Explanatory interfacing'

Reductionism has been characterised in a number of different ways. In one sense it is rather a tame thesis—Peter Smith describes it like this:

> Reductions in this[196] very general sense are evidently always good things to have, and are often mandatory. If we can explain why theory T_2 holds (or at any rate, holds as well as it does) by appeal to theory T_1, then that is by any standards a worthwhile theoretical achievement. And if theory T_2 makes claims about some domain for which some more inclusive theory T_1 already purports to give reasonably comprehensive causal account, then maintaining both theories together will indeed require that the applicability of T_2 can be explained in terms of T_1.[197]

In a similar opening definition, Dupré says, '[r]eductionism, in its broadest sense, is the commitment to any unificatory explanation of a range of phenomena.'[198] He goes on to state that reductionism is a commitment to giving structural explanations of phenomena

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[196] Smith is referring to Ernst Nagel's account in *The Structure of Science* 'Reduction ... is the explanation of a theory or a set of experimental laws established in one area of enquiry, by a theory usually though not invariably formulated for some other domain.' Nagel. E (1962) London: Routledge & Kegan Paul. 338.

[197] Smith, P. (1992) 'Modest Reductions and the Unity of Science' in *Reductions, Explanation and Realism* Charles, D. and Lennon, K. (eds.)(1992) Oxford. Oxford University Press, 20.

[198] *Disorder* 87.

and that 'it is debatable and debated whether such structural insights imply anything like derivation of the behaviour of those objects whose structure is elucidated.'[199] Here he is addressing something stronger than the kind of explanation that could be called 'unificatory' in a broad sense, claiming that structural explanation lies at the heart of the forms of reduction that are most questionable: 'Many of the greatest achievements of science depend essentially on insight into the structure of objects. But the significance of this fact is very unclear.'[200]

There are different forms of reduction then. Smith characterises a deeper reductionism in this way, saying that from the general form,

> [t]here is no necessary implication ... that the mode of explanation in question has to involve type-type correlations or identifications between kinds recognized by the two theories. Nor is there any necessary implication that explaining the applicability of T_2 in terms of an underlying theory T_1 must mean that T_1 absorbs, supersedes, or eliminates the reduced theory T_2.[201]

These stronger forms aside, the general pattern of explanation that Smith describes he calls 'explanatory interfacing.' That is, 'an explanation in terms of theory T_1 of why theory T_2 works as well as it does' is 'an *explanatory interfacing* of T_1 to T_2.'[202] Let us explore this idea a little further, since Smith argues that explanatory interfacing, taken in a very open sense is enough to secure a general unity for science.

How strong is explanatory interfacing? Smith points out that in most cases arguments against reductionism are aimed at the kind of reduction he finds in Nagel. That is, the tar-

[199] *ibid.* 89.

[200] *ibid.*

[201] Smith *op cit.* 22.

[202] *ibid.*

get for anti-reductionist critiques is mostly one that assumes that reductions are carried out via a DN model of explanatory connection between theories.

> ... 'reduction' has come to be reserved for strong reductions, i.e. explanatory interfacing where one theory simply subsumes the ontology and explanatory resources of another (so that T_2 is revealed as just a special application of T_1).[203]

What Smith has in mind for the general notion of explanatory interfacing is not nearly so strong. He takes explanation to be more generally structured than the DN model allows:

> Explanation is contrastive: the fundamental form of an explanation is that p (rather than $q_1, q_2, \ldots$) explains why r rather than $(s_1, s_2, \ldots)$, and explanatory frameworks are in part constituted by the taxonomies of relevant contrasts that they acknowledge.[204, 205]

Consequently, there may well be cases where the same things are addressed by the two theories. That is, the same patterns are explained and the same ontologies used so that, as we would want,

> ... the pressure to provide an explanatory interfacing between a theory T_2 and some more sweeping theory T_1 does not arise, in the general case, because T_1 already explains what is explained by T_2 (if that means that T_1 already explains the same contrastive patterns as T_2).[206]

However, this is not the problem. What Smith wants us to accept is that science connects theories that contain differing ontologies and which explain differing contrastive patterns. Smith's strategy rests on the possibility that explanatory interfacing is intuitively appeal-

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[203] *ibid.* 27.

[204] *ibid.* 22-23.

[205] For further explication of contrastive explanation see also van Fraassen, B. (1980) *The Scientific Image* Oxford: Clarendon Press. Chapter 5; and Lipton, P. (1991) *Inference to the Best Explanation* London: Routledge.

[206] Smith *op cit.* 23.

ing. He does not think that the reduction it involves is based on anything more than the kind of basic physicalism outlined above.

> If there is a driving prejudice at work here, it is not radical physicalism but a principle, *P*, to the effect that the behaviour of wholes is in general causally produced by the behaviour of the parts, so that our explanatory stories about wholes must be consonant with our stories about the causal mechanisms constituted by their parts.[207]

P is also assumed to be intuitive. We shall need to return to this later. *If P* is acceptable, then Smith believes that explanatory interfacing best captures all our intuitions about reduction and explanation. Apparently strong reductions are very rarely Nagelian in form. And even when we seem to have strong, strict cases of Nagelian reduction only explanatory interfacing can in fact capture the core of the explanation involved.[208]

> Only the most extravagantly formalistic conception of scientific theorizing could sustain the thought that basic principles are everything, and all else is mere mathematics; but if this absurd formalism is abandoned, there is no route from the claim that the basic assumptions of some area of science are (near enough) explanatorily reducible by Nagel's formal criterion to the conclusion that we have thoroughgoing theoretical supersession or absorption. So, we do need to divorce the ideas of such absorption from the core sense of reduction, i.e. the explanatory interfacing of theories.[209]

Now the point of all this is that Smith believes he has done enough to secure a general unity for science. Modest reductions are enough to maintain that unity:

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[207] *ibid.* 25.

[208] Smith uses a number of examples to make these points. On the whole these are taken from mechanics and thermodynamics—I shall not repeat them here.

[209] *ibid.* 32.

> The demand to provide at least such modest reductions may often be compelling, for in many cases it is arguable that we need to be able to see, given the T_1 facts, how some other theory T_2 can hold good of the domain in question. Moreover ... *modest reductions can still subsume the old programmatic aim of demonstrating unity*, i.e. of showing how science hangs systematically together, with higher-level theories being shown to have application in virtue of lower-level facts ...[210]

We need to ask what sort of unity this is. Can it really be the 'old programme'? Well, in the sense that it leaves a form of reduction in place, it is. Any *systemic* demonstration of high-level theories' applicability based on more basic principles would fit the bill. But how systematic can Smith's explanatory interfacing be? Since he has loosened the criteria for being the kind of explanation and reduction he wants, we are left with intuitive appeals to *P* as a substitute for a formal system. *P* is defended, and it is particularly sensitive in its calling upon causality to secure the connection between parts and wholes, since this in turn rests on an assumed asymmetry of causal relations. It is assumed that parts can bring about changes in the wholes they make up, but not *vice versa*, at least not in a way that is epistemically valuable for science. That is, explanations of the world by its smallest parts that only contain partial descriptions of larger scale behaviour are, generally, to be preferred over explanations that include partial accounts of the smallest parts, which adequately describe large scale systems.[211] For example, as I type this sentence the index finger of my left hand is moving over the keyboard. It is made up of molecules that are moving. The prejudice is to prefer an explanation that gives an account of the movement of the molecules in terms of their behaviour and the forces acting on them *alone* as complete. But this obviously is a prejudice. The possibility that *my typing* could be a cause of the movement of the parts of my finger has to be ignored to make this assumption work. Once re-

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[210] *ibid.* 36.

[211] Where 'adequate' is taken to mean being accurately descriptive and predictive within reasonable limits.

ductionism is rejected the grounds for ignoring causes that are not based in micro-theories seems shaky and greatly in need of further argument.

Whatever difficulties there are with Smith's position—and we shall return to them shortly—I am going to assume that it does represent a fairly coherent defence of scientific unity with reductionism in place. It would be easy to set up a straw-man on these points, but that would be of little value for the points I am trying to make. It can look as though Dupré is attacking such a straw-man. He outlines the strongest form of reduction in the following way:

> Assume ... a hierarchical classification of objects in which the objects at each level are complex structures of objects comprising the next-lower level. ... The investigation of each level is the task of a particular domain of science, which aims to discern the laws governing the objects at each higher (reduced) level from the laws governing the objects at the next-lower (reducing) level. Such reduction, in addition to knowledge of the laws at both the reducing and the reduced levels, will also require so-called bridge principles (or bridge laws) identifying the kinds of objects at the reduced level with particular structures of the objects at the reducing level. Given the transitivity of such deductive derivation, the end point of this program will reveal the whole of science to have been derived from nothing but the laws of the lowest level and the bridge principles.[212]

In Dupré's strong account there is no talk about theories superseding one another nor about the elimination of lower level theories, but simply a direct deductive *derivation* of higher, more complex levels from the lowest and simplest (together with bridge principles that relate the various parts and objects of the different levels through identity statements), that is, a Nagelian, DN explanatory connection. (Elimination is a further complication that does need to be kept separate.[213]) However, the strength of this position—Dupré goes on to re-

[212] *Disorder* 88.

[213] Having said this, there are a number of writers who do run these issues together. Paul Churchland, for example, in following his own brand of reductionism says: ' ... a reduction *locates* the newer theory within the conceptual space currently occupied by the older theory. It provides the basic instructions, as it were, for the orderly displacement of the latter by the for-

fine it and examine other versions in any case—is not what is important for Dupré. His challenge, ultimately is to the very assumptions Smith needs to make to get his account of reduction and unity up and running. That is, that 'ontological priority must be accorded to the allegedly homogenous stuff out of which bigger things are made.'[214] Dupré points out that constructing *any* coherent and sensible account of monistic materialism runs up against the problem of making either that definition too flimsy to support what it is supposed to, or so strong that it requires a robust defence of reductionism to support it. Compositionally defining materialism or physicalism[215] as was done above is inadequate. Saying that everything is made up of the same kind of stuff does not lead one to suppose that there is only one kind of things.

> Provided the alternative is seen to be a pluralism based not on different kinds of stuff but on irreducibly different kinds of things, and provided we reject the false dilemma of materialism versus Cartesian dualism, this kind of materialism is irrelevant to the issue between monism and pluralism.[216]

Now when we take on board the desire to have some sort of explanation of the kind of world we find ourselves in, the 'explanatory interfacing' of theories comes to the fore. As Dupré puts it in the context of discussing different concepts of ontology:

mer.'—for 'displacement of the latter by the former' we can read 'elimination of the latter in favour of the former.' (Churchland, Paul M. (1979) *Scientific Realism and the Plasticity of the Mind* Cambridge, Cambridge University Press, 81). Yet it is unclear why location of one theoretical claim in the conceptual space of another should imply displacement at all—why could the two theories not co-exist in such a space?

[214] *Disorder* 89.

[215] See *Disorder* p. 90-91 for why Dupré prefers 'materialism' over 'physicalism.'

[216] *ibid.* 92.

> The idea ... is that we are "ontologically committed" only to whatever entities we need to appeal to in order to explain anything. Prima facie, this conception will commit us to acknowledge all manner of diverse entities. However, if *reductive* materialism were true—that is, if we could explain everything by referring only to physical entities—this conception of ontology would provide a clear sense in which only physical entities need be admitted to exist.[217]

Smith's reductions require that there is a consistency with the various levels of scientific description that disciplines provide. That consistency is explanatory. And as I have already pointed out,

> [c]ertain views about the nature of causality suggest that only some kind of reductive relation between higher and lower levels can achieve such consistency.[218]

This is precisely what Smith is arguing, but with a thesis about causality smuggled in. Dupré picks this out as a thesis about causal completeness.[219] It amounts the same issue I have highlighted, namely, that causal descriptions of the parts (especially at the micro-physical level) of a thing or phenomenon will be preferred over descriptions of wholes, because they are taken to be entirely *sufficient* as an account of why any thing or phenomenon is as it is. Principle *P* is a given for Smith's position but it is not unassailable. I do not intend to detail the particular view of determinism that Dupré puts in place as a critique of causal completeness, largely because I consider it enough for my purposes here that *P is* questionable. It is at this point that Cartwright's observations on the limited nature and range of physical laws are apposite. Constitutive materialism is weak—it does not contain an explanatory commitment to universal, complete laws. The scientific laws that do all the explanatory work are phenomenological in nature, hedged about by *ceteris paribus* criteria drastically delimiting their application.

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[217] *ibid.* 94.

[218] *ibid.* 99.

[219] *ibid.* 99-102.

> Reducing Mendelian genetics to molecular genetics was widely thought tantamount to reducing theories about living things to theories about inanimate phenomena. Once accomplished in genetics, the cat was out of the bag, the in-principle possibility of a fully physical account of all biological process was as good as proved.[224]

The payoffs were thought to be important too:

> Not only was the unity of science vindicated at the critical juncture between physical science and life science, but the metaphysical thesis of materialism was given powerful scientific vindication. Here at last was factual proof, not merely philosophical argument, with which to confront the dualist and the antimaterialist in metaphysics.[225]

Unfortunately, under analysis from Hull it turned out that the actual mechanisms involved in translating an organism's material genes into real expression as traits of the organism, that is its phenotype, undermines that possibility.[226] There is no one-to-one unique correlation of genes to phenotypic properties in any significant way.

> Even if gross phenotypic traits are translated into molecularly characterized traits, the relation between Mendelian and molecular-characterized predicate terms expresses prohibitively complex, many-many relations. Phenomena characterized by a single Mendelian predicate term can be produced by several different types of molecular mechanisms. Hence, any possible reduction will be complex. Conversely, the same type of molecular mechanism can produce phenomena that must be characterized by different Mendelian predicate terms. Hence reduction is impossible.[227]

Rosenberg uses predicate logic to illustrate Hull's point that Nagelian bridge principles demonstrating the biconditional relationship between Mendelian predicates and their ap-

[224] *Instrument* 19.

[225] *ibid.*

[226] As all good school children know, *phenotype = genotype + environment*. In this case the environmental factors affecting the development and maintenance of an organism can be discounted or taken to be 'ideal' and the argument still has force. Shortly it will become clear that once the role of environment in determining *when* and *how* particular genes are active is taken into account a wreath is thrown on the coffin of reductions to the physical. I suspect that the assumption that there are ideal conditions for the functioning of genes is where the problem lies. In real biological contexts this notion seems completely empty.

[227] Hull, D. (1974) *Philosophy of Biological Science* Englewood Cliffs, NJ: Prentice Hall, 39.

parent expression would be effectively unlimited disjunctions on each side of the biconditional.[228] Rosenberg holds that,

> This sort of reduction is not methodologically useless, it is probably unattainable by agents of our cognitive and computational powers. Reductionism thus seems fated to cast little light on intertheoretical relations in biology.[229]

Note that Rosenberg ties this failure of reduction to our cognitive and epistemological powers.[230] His reasons for doing so will be discussed shortly. Rosenberg's solution of how to get Mendelian properties to behave regularly in relation to their material components is a familiar one, and one that is amenable to the kind of explanatory interfacing Smith has in mind. Rosenberg believes that supervenience will do as the relation between the two levels of description.

> Mendelian phenomena are *supervenient* on molecular ones: given any two biological systems that are identical in all their molecular properties, they will *have* to be identical in all their Mendelian properties. Two biological systems with different Mendelian properties will have to differ in some molecular property or other, although two biological systems may be identical in Mendelian properties while differing in molecular ones.[231]

That is, Rosenberg takes supervenience to be a way of retaining the general form of reduction and unity that Smith discusses. So we need to know how adequate the idea of supervenience is, and what commitments it involves. Something like principle *P* is assumed here too, along with causal closure. Question these and the *prima facie* attractiveness of super-

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[228] *Instrument* 21-22.

[229] *ibid.* 22.

[230] The response of philosophers to the 'many-many' problem has been questioned and examined in Waters, C. K. (1990) 'Why Anti-Reductionist Consensus won't Survive: The Case of Classical Mendelian Genetics' *PSA* 1990, Philosophy of Science Association. The observations he makes about refinements to the reductionist argument to accommodate Hull's general thesis are irrelevant if one takes a general 'externalist' view of the information required to bring about a particular phenotype.

[231] *ibid.* 23.

venience collapses. And as Dupré points out, if there are no general grounds for accepting supervenience we are thrown back on looking for evidence[232] of it and,

> [*e*]*vidence* for supervenience, it seems, would have to be the kind of evidence necessary for reductionism. It would be evidence that higher-level phenomena are indeed determined by lower-level phenomena, or that identical (or sufficiently similar) lower-level phenomena do indeed produce the same higher-level phenomena.[233]

Viz., evidence for *P*. The problem for anyone trying to make the supervenience case for Mendelian genes' dependence on molecular genetics is that the evidence is in fact *against* the case. Simply put, in the vast majority of cases, particular phenotypic traits are not determined by genes alone—DNA is not stored as a simple list of genes that are unique and genes do not function independently of the environment. If one assumes that there are real causal powers in the large scale of things, then building the kind of loose asymmetric bridge laws needed for supervenience is also impossible. The facts involved include the *shape* of amino acids (and hence proteins—a four levelled structuring) produced; the factors affecting the actual activation of genes, that is, the conditions of their expression; the actual form of the traits' phenotypic expression; and the complexity, redundancy and interaction of genes themselves.[234] Unpacking the 'many-many' problem does require acceptance of actual natural biology, but it is straightforwardly clear that the 'information' con-

[232] For a discussion of a number of failed reductions and the nature of genes, see, Hubbard, R. and Wald, E. (1993) *Exploding the Gene Myth: How Genetic Information is Produced and manipulated by Scientists, Physicians, Employers, Insurance Companies, Educators, and Law Enforcers* Boston, MA: Beacon Press.

[233] *Disorder* 97.

[234] Famously, problems in how to explain many aspects of biological development within a mechanistic, unified framework are the starting point of Rupert Sheldrake's 'hypothesis of formative causation.' Whatever the peculiarities of his metaphysics might be, I always find his insights into problems in contemporary biology helpful in breaking expectations of 'scientific explanation' from the perspective of the mainstream science writing. See, for example, Sheldrake, R. (1987) *A New Science of Life: The Hypothesis of Formative Causation* (2nd Edition) London: Paladin, Grafton Books; and (1989) *The Presence of the Past: Morphic Resonance and the Habits of Nature* London: Fontana, Harper-Collins.

tent of an organism's phenotype has to be determined in an externalist way by an environment that affects the phenotype directly, and also affects the expression of the genotype. The sensitivity of the genotype to changes in the environment is indefinite and complex. Genes are phenotypically *meaningless* recipes for protein production without any built-in outcome when considered on their own.[235] The reduction of the biological traits of organisms to the information content of genes stops with this fact.

4 Ecology

A further example of the failure of reduction in the biological sciences can be found by looking at ecology. Dupré uses examples from ecology to good effect and I shall follow his argument closely.[236] The examples also have bearing on the discussion of natural kind terms to follow.

Although gene-trait reductions are the most obvious kind for the philosopher's attention, biology covers a wide range of descriptive levels of organisation:

> These include, at least, genes, cells, organs, multicellular organisms, and groups of organisms ranging from family groups and demes, through populations and species, to ecosystems and higher classificatory taxa. Since genes are generally assumed to belong simultaneously to the domain of chemistry, and cells to that of biology, it is worth mentioning also such things as viruses and viroids, which hover uneasily on that boundary. Given this array of structural levels, there are a correspondingly large number of possible reductionist projects.[237]

And, given this range of projects, it is no surprise that we find attempts to reduce ecology to lower structural levels. But given the discussion so far, it is going to be hard to find a

[235] If any analogy is appropriate for describing genes, recipes are probably the best. Good food depends only in part on the recipes. A recipe is inert. So much depends on the quality of the ingredients, the skill of the cook, the equipment available and even the weather!

[236] *Disorder* Chapter 5.

[237] *ibid.* 107.

way of showing that the content of ecological theories are open to reduction to anything lower without the supposition that all the causal mechanisms are explicable in terms of the lower level's causal architecture. But this is not all that can be seen. If one is sensitive to what most ecology is there is a further issue about relating mathematical models for populations to actual populations.

Ecology can be characterised in the following way:

> Apart from a lot of purely descriptive work on the way organisms interact with one another and their environment, the theoretical core of ecology is the construction of mathematical models, based on empirical observation and plausible biological assumption, intended to reflect the development over time of interacting populations.[238]

Dupré uses the example of parasitic wasps that lay their eggs in live insects of other species. It is possible to construct mathematical models relating the numbers of parasites to hosts where assumptions are made about the number of eggs laid in each generation, the survival of each lava and irrelevance of other factors affecting the life and health of both parasite and host. It is possible to construct any number of similar models for populations interacting in a similar way. For example, one could model the dynamics of thrush and snail populations in a woodland area. Here the assumptions would be about the predation of snails (whether it was due *only* to thrushes), the diet of the thrush population, the general survival rates of thrushes, climate changes, and so on. In this second example the assumptions that are made are more restrictive on the model than in the first case in the sense that there we are dealing with single generations of host and parasites—the parasites kill their hosts before the host can reproduce. The point is that the models tend to be idealised mathematical predictive tools. This brings us up against the issue of how such abstract

[238] *ibid.* 108.

models can be appropriate to complex real situations.[239] With reference to Cartwright[240] Dupré notes that,

> [w]e need to ask ... to what extent it is legitimate to apply such a model [wasp parasitism] to real ecological systems. Of course, the model was not derived as a random stringing together of symbols: it was constructed to reflect the actual interactions and behavior observed in certain kinds of organisms. On the other hand the model clearly embodies an idealization of such observations.[241]

Such models are of value. They allow us to predict in general terms future populations and to adapt our models to include other factors—how would a drainage program for woodland close to farmland, known to affect the snail population, affect the thrush population? Dupré argues that the models could never reach a point of convergence with the real ecosystems under investigation.

> A crucial explanation of its [the model's] limitations is in terms of the conflict between the taxonomic principles implied on the one hand by the abstract structure involved, and on the other by the practical demands of the concrete application. In the latter we have actual, countable populations of particular organisms, and in the former, theoretically defined terms such as *parasite*, *prey*, and *competitor*. It is hardly likely that these could ever be wholly extensionally equivalent.[242]

This outcome is what we want to build up a picture of disunity in biology. Models of ecosystems, be they simple or complex, will be built around the kinds that are most significant in the situations under investigation, rather than those which match kinds generated for other purpose, such as phylogeny. Even within ecological models we may not always want

[239] The nature of realism and biology, as a science of the complex, is of interest here too. See p. 133 ff.

[240] Cartwright, N. (1983) *op. cit.*

[241] *Disorder* pp. 110-11.

[242] *ibid.* 111.

information simply about abundance and distribution alone, but may wish to examine more complex relations between organisms.[243] Consequently,

> ... our interest in a kind of organism need not be restricted to those features that predominantly determine its abundance; and kinds of organisms that are highly similar with respect to features and interactions that determine their abundance may nevertheless differ in respects that are of crucial interest to us. Thus, any attempt to develop models of ecological systems in terms of ecological natural kinds runs the risk of delivering information quite unrelated to the prevalence of organisms of the kinds in which we are interested.[244]

This position, Dupré argues, is likely to be met with resistance. Technical objections might arise about the possibility of 'maximizing the information content of a classification.'[245] That is, there will be an indefinite range of possible properties that could be maximised, and hence there can be no robust account of how they might be worked into a variety of interest based taxonomies of different sorts. Whatever technical issues arise about classification schemes—and as I shall suggest shortly, we *can* give an account of schemes and systems of classification that have a variety of interest bases—Dupré points out that to argue that diverse human interest will not be a limiting factor in taxonomies would be simply to run against the tide of actual practice:

> But though variable, the condition of human interest or theoretical significance surely provides a very substantial limitation, or at least pragmatic ranking, on the range of properties of special relevance to taxonomy. The adjustment of classificatory practices in response to such more or less significant distinguishing properties, the significance of which derives from a diverse and pluralistic set of interests, cannot but be a central aspect of the evolution of taxonomy.[246]

[243] Dupré uses a simple sketch of a model using lynxes and their prey (rabbits and hares) where phylogenetically insignificant features about the living habits of rabbits and hares have direct impact on the model. *Disorder* 111-113.

[244] *ibid.* 112-113.

[245] *ibid.* 113.

[246] *ibid.* 113.

Dupré also suggests that the prevalence of phylogenetic explanations in biology, although giving a large weighting to taxonomies of kinds based on phylogenetic interests, does not negate the tension that these taxonomies create in comparison with the ecological models we have been discussing.[247]

The point of these observations is to demonstrate that the laws used to describe the behaviour of populations will not be explicable in terms of laws governing the properties of the individuals comprising those same populations. That is, that there are properties of populations that are not derived from properties of the organism concerned. Now Dupré generalises the point to claim that this difference arises because at whatever level of description we apply a taxonomy governed by laws we will be using some ideal notion of the kinds involved—models are always abstractions of some sort. From this he also extracts an explanation of why sometimes we do look for micro-explanations:

> It is typically the observation that a certain property is quite *consistently* displayed in a certain group of organisms, at least under specifiable circumstances, that leads us to investigate the structural or other basis of this property in the individuals. On the other hand, when our interest in a property is extrinsically determined by its relevance to a macrotheory, we have no reason to assume that this property will be a stable feature of the members of the kind in question, and no reason, more generally, to expect that property to be amenable to systematic investigation at the individual level.[248]

Dupré further secures his position, but I think we have seen enough here to support the general argument that given a range of inquiry goals a whole set of independent taxonomies can be generated based on laws that are not derivable from any other taxonomic system that describes the same organisms. Taken together with the observations about the failure of genetic reductionism, we see that not only is there no reduction of biology to parts of

[247] *ibid.* 114.

[248] *ibid.* 117.

chemistry, but there is no possibility of reducing biology to one simple account of the taxonomy into which life could be fitted.

5 *Anti-reduction and disunity*

The above discussions shows that principle *P*, understood in terms of explanation and the relationships between disparate theories, cannot be assumed as a given, and that there is evidence from examination of biology to support a pluralistic picture of explanations. There are good grounds for rejecting *P* and therefore dismantling even the modest form of reduction that Smith puts forward as a model for unity for scientific explanations. He takes that model to show that there can be a way of connecting inter- and intra-disciplinary descriptions of the world at different levels. Stronger forms of reduction and explanatory interfacing are therefore blocked too.

The consequence of this is that we have no route from the explanatory success of particular fields and disciplines to a complete or even potentially complete picture of the world described by science. We explain parts of the world with theories that are appropriate to them. What has yet to be shown is that this implies that explanations above the supposed final grounding in physics should be read in a realistic spirit. That is, it is entirely possible that, similarities in method aside, biology does not reveal a real world in the robust way physics is taken to (by scientific realists at least). The success of biological theories need only be pragmatic or instrumental. Such a position could incorporate all the points made above about the failure of reductionism whilst simply qualifying the failure with the rider that the kinds that are used in biological theories are not real. Such a position, however, could not properly account for the causal properties of biological kinds.

Let us now turn to natural kinds terms. This is a further aspect of the assumption of causal closure of events at the micro-level. The assumption about natural kind terms that supports principle P is that natural kind terms can be fixed by definitions that make essential reference to the constitutional parts of the individuals of that kind. This assumption too can be challenged.

iii Natural kind terms and essentialism

How are we to understand natural kind terms? Do natural kinds have essential properties? The prevalent set of ideas about natural kind terms is rooted in Locke's distinction between nominal and real essence.[249] Dupré observes that:

> The traditional view ... is that terms of ordinary language refer to kinds whose extension is determined by a nominal essence, and hence not ... to natural kinds. Optimists, at least, believe also that science, by contrast, attempts to discover those kinds that are demarcated by real essences. It is compatible with these two views that on occasion real and nominal essences might coincide.[250]

To begin with a sketch, Locke was not an optimist. Although we may aim at an articulation of real essences when trying to say what makes the natural kinds of the world what they are—for Locke the microstructural composition of each kind (famously gold)—this information is unattainable.[251] However, in the twentieth century, probing and testing the microstructure of stuff like gold (atomic no. 79, atomic weight 196.97 ...) *is* possible.

[249] Real essence is, according to Dupré (quoting Locke) ' ... whatever accounts for the characteristic nature of things of a certain kind ("the being of anything whereby it is what it is") ... ,' and nominal essence, ' ... merely the feature or set of features that we use to distinguish objects as belonging to the kind ("the abstract idea which the general, or sortal ... name stands for.")' *Disorder* 20.

[250] *ibid.* 22.

[251] There are many interpretations of Locke on his understanding of substance and essence. Since depth scholarship is not necessary here, I have followed the 'guidebook' remarks of Lowe, E. J. (1995) *Locke: on Human Understanding*, London: Routledge, Chapter 4.

Thus, Putnam[252] argues that through the division of labour we can have at our disposal, in our ordinary use of language as a community of language users, a close mapping of the real and nominal essences. That is, experts can tell me whether the heavy, bright yellow, malleable, ductile metal I have discovered in nuggets in the stream at the bottom of the garden really is gold; my ordinary competent use of the word 'gold' can be checked against the complete definition that includes the properties of gold's real essence, in referring to the stuff accurately, and corrected if necessary.

The detail of Putnam's theory is discussed by Dupré in the following way. The meaning of any natural kind term is composed of four parts which he calls a syntactic marker, a semantic marker, a stereotype, and an extension. For *mouse* the syntactic marker is 'noun,' the semantic marker 'animal,' the stereotype could be 'small, furry creature with a long tail, hand-like paws, that squeaks, likes cheese and is eaten by cats, *etc.*,' the extension is then whatever theoretical information (microstructural and otherwise) is available about mice. Thus, the nominal essence of 'mouse' roughly corresponds to the stereotype and the real essence to the extension. Dupré then points out that this picture does not require that everyone in a community of language users should have knowledge of the extension of a term to be competent in talking about particular natural kinds. Experts can tell us what terms 'really' mean if we have difficulty with the stereotype or need a finer judgement. However, this does seem to generate a problem for the ordinary use of language:

> Devotees of the so-called pessimistic induction on the history of science might find it particularly distressing to discover that the provisionality of science here threatens to undermine the intelligibility of our everyday speech.[253]

[252] Putnam, H. (1975) 'The Meaning of "Meaning"', in *Mind, Language and Reality: Philosophical Papers*, vol. 2, Cambridge: Cambridge University Press. It is only an earlier Putnam that I am critiquing here. See p. 210 ff. for discussion of my support for the most recent Putnam.

[253] *Disorder* 24.

Although this comment is bracketed in Dupré's text it is an important observation. Putnam's theory attempts to show how the mapping of stereotype onto extension can be achieved through a 'sameness relation' for any instance of a kind to the key 'real essence' exemplar of that kind. The exemplar having been defined for the actual world (against other possible worlds, such as Putnam's 'Twin Earth'). Ordinary language becomes dependent on science talk:

> It should be clear that Putnam's theory offers precisely a way of tying our ordinary language classifications to those provided or eventually provided, by science. Given Putnam's picture it is natural to suppose that as science advances, ordinary language will be adjusted ... to conform to these more accurate categories. Categories that turn out to be, from a scientific point of view, wholly wrongheaded may be abandoned. Or if they are not, we can perhaps recognize that certain backwaters of ordinary language carry on outside the pale of orderliness described by science (perhaps as a resource for poets and the like).[254]

Dupré goes to some lengths to show that ordinary language, as it embodies common sense knowledge of the world, is disorganised and disunified. 'The ontology of common sense is highly pluralistic.'[255] He uses many examples from biological taxonomy that show there is a mismatch between the categories that science picks out as significant and those that we use everyday to describe the natural world. For example, rabbits and hares are different species of the same genus, *Lepus*, and in terms of physiology they are very similar. Yet there has been a tradition of regarding them as distinct in fairly clear ways. Dupré quotes Bewick on this point:

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[254] *ibid.*

[255] *ibid.* 19.

> Notwithstanding the great similarity between the Hare and the Rabbit, Nature has placed an inseparable bar between them, in not allowing them to intermix, to which they mutually discover the most extreme aversion. Besides this, there is a wide difference in their habits and propensities: the Rabbit lives in holes in the earth, where it brings forth its young, and retires from the approach of danger; whilst the Hare prefers the open fields, and trusts to its speed for safety.[256]

More interestingly, there is no mapping of the 'obvious' distinction between butterflies and skippers on the one hand and moths on the other.

> The order Lepidoptera includes the suborders Jugatae and Frenatae. It appears that all the Jugatae are moths. The Frenatae ... are further subdivided into the Macrolepidoptera and the Microlepidoptera. The latter seem again to be all moths. But the former include not only some moths but (all) skippers and butterflies.[257]

It would seem on the Putnam scheme that either biological categories are not to be regarded as having the real essential properties of natural kinds in the same sense as those natural kinds discovered by the physical sciences, or many of the natural kind terms that we do use in common sense talk about the world are literally meaningless, since their extension and stereotype seem too far removed from each other to be reconciled—butterflies would have to be taken to be a kind of moth. Once one moves away from stock examples about simple substances like water and gold the pictures becomes complicated in the extreme. Sometimes our common sense natural kind terms pick out individual species, neatly mapping onto the extension of scientific classification, for example bottle-nose dolphin and field mouse, but more often the taxon to which to a common label refers varies widely:

[256] *ibid.* 29.

[257] *ibid.* 28.

> Ducks, wrens, and woodpeckers form families. Gulls and terns form subfamilies. Kingbirds and cuckoos correspond to genera, while owls and pigeons make up whole orders. The American robin ... is a true species, although ... in Britain *robin* refers to quite a different species,[258] and in Australia ... it refers to a genus of flycatchers.[259]

Other classificatory differences between common sense and science are discussed by Dupré, and as he points out, common sense classifications are, 'unsurprisingly, overwhelmingly anthropocentric.'[260] The point is that we are presented with the following possible outcomes to the recognition of the failure of extension and stereotype to even approximate to each other in important natural kind terms concerning the natural world: we reject any form of taxonomic realism, taking biological terms to be instrumentally valuable only—they are no longer taken to be the extension of natural kind terms, they do not represent the real essence of these terms; we eliminate, by a Churchland-type reduction to the language of science, our common sense terms; or we abandon the idea that the definition of natural kinds need be via essences (of any variety), and that the world that science describes is any more ordered than common sense accounts of it. Now clearly the second option is ruled out by the failure of reductions of the type needed. This leaves two options which are substantially different but which are consistent with the discussion up to this point.

To begin addressing the question of natural kinds and realism we should note that Dupré's argument is towards an acceptance of a disordered scientific picture alongside the pluralism of common sense. Rosenberg has claimed that it is 'remarkable'[261] that Dupré does not

[258] Interestingly *Collins English Dictionary* (first edition) defines 'robin' as 'a small Old World songbird, *Erithicus rubecula*, related to the thrushes: family *Muscicapidae* ... [and] a North American thrush, *Turdus migratorius* ... [and] any various similar birds having a reddish breast.'

[259] *Disorder*, 33.

[260] *ibid.* 34.

[261] *Instrument*, 12.

take up the instrumentalism that he himself advocates to avoid the consequences of *epistemological* pluralism that seem to follow from any combination of realism and anti-reductionism about the complexity of biological phenomena. However, it seems to me that Rosenberg is giving an over hasty reaction to Dupré's position.[262] But before looking at this point more fully we need to be clear how Dupré makes his case against the definition of natural kind terms through essences in science.

I shall argue that Dupré is correct in his general assessment of the disorder present in the natural kinds the biological sciences discover (indeed, present in any system more complex than molecules[263]). His evidence need not entail a pluralism that reaches beyond ontology and epistemology in science. By this I mean, although we may come to a pluralism about different kinds of things that there are—and consequently to a pluralism of different scientific realisms—we do not need to lose sight of science as having a substantially unified grounding in rational practice based on the nature of the kind of beings we are.

Having introduced the realism problem, which will inform the rest of the discussion, let us return to the argument against essentialism. We have seen how Dupré demonstrates a schism between common sense and biological terms for natural kinds. Biology's taxonomies do not match the kind terms of our intuitive division of the living world into useful categories of things—and they do not map onto each other. Consequently picking out essential properties to define kinds is either question begging in terms of picking out the 'real' world, or the wrong strategy for natural kinds.

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[262] Admittedly, Rosenberg points out that in writing *Instrumental Biology or the Disunity of Science* he had no access to Dupré's *The Disorder of Things* until the manuscript was all but complete.

[263] Rosenberg neatly demonstrates the diversification of systems above the molecular level in *Instrument*, chapter two, roughly through the distinction of structure and function which is increasingly important in higher level systems. More on this below.

This general point can also be seen in the more abstract debate about the definition of an apparently indispensable biological concept, species. This is one of the issues I mentioned earlier currently included in the cannon of philosophy of biology proper. A problem arises in determining how the term 'species' should be identified so that all the groups of individual organisms we want to be included are covered, while the concept still usefully excludes higher-level groups or sets of organisms based on 'arbitrary' properties such as colour, mass or location. Hulls' work on this problem has been influential for decades:

> If 'characters' is taken to refer to evolutionary homologies, then periodically a biological species might be characterized by one or more characters which are both universally distributed among and limited to the organisms belonging to that species, but such states of affairs are temporary, contingent, and relatively rare. In most cases, any character universally distributed among the organisms belonging to other species, and conversely any character that happens to be limited to the organisms belonging to a particular species is unlikely to be possessed by all of them.[264]

So for any single species there is no defining characteristic. The problem is further compounded:

> The natural move at this junction is to argue that the properties that characterize biological species at least "cluster." Organisms belong to a particular biological species because they possess enough of the relevant properties or enough of the more important relevant properties. Such unimodal clusters do exist, and might well count as 'statistical nature,' but in most cases the distributions that characterize biological species are multimodal, depending on the properties studied. ... To complicate matters further, these clusters of properties, whether uni- or multi-modal, change through time. A character, state (or allele) which is rare may become common, and one that is nearly universal may become entirely eliminated. In short, species evolve, and to the extent that they evolve through natural selection, both genetic and phenotypic variation are essential.[265]

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[264] Hull, D. (1989) *The Metaphysics of Evolution* New York: SUNY Press, 11.

[265] *ibid.*

This presents the logical problem of defining a set of things without an intension and without a fixed extension. Hull's own approach has been to treat species as individuals for metaphysical purposes, but this does not solve the host of difficulties then arising about how to fix which parts (organisms) belong to which species when, as observed, no characteristics determine species in any useful way. And this does not help us resolve the counter-intuitive nature of species as individuals. Dupré suggests the solution to the status issue should be a pragmatic one:

> ... to the extent that we take theoretical embedding as the correct way to consider the question of the ontological status of species, we are driven to a pluralistic answer: in some contexts species are treated as individuals, in others as kinds.[266]

Starting with this pragmatic position Dupré goes on to discuss the genuine difficulties of specifying the criteria for species membership in any consistent way. He rejects Mayr's 'biological species concept.'[267] 'This takes a species to consist of a group of organisms connected to one another by actual or possible reproductive links, and reproductively isolated from other organisms.'[268] This concept has been shown to have only limited applicability.

> [I]t has no apparent application to asexual organisms ... Perhaps a more serious difficulty is that in a great many actual cases, especially, but by no means only, among plants, reproductive isolation is fairly weak.[269]

The idea has been generalised by discussion of the flow of genetic material as the guide to the isolation of a species. This too has faced many problems, as Kitcher has shown.[270] The

[266] *Disorder* 44.

[267] Mayr, E. (1963) *Animal Species and Evolution* Cambridge, Mass: Harvard University Press.

[268] *Disorder*, 46.

[269] *ibid.*

[270] Kitcher, P. (1989) 'Some Puzzles about Species' *What the Philosophy of Biology Is*, Ruse, M. (ed.) Dordrecht: Kluwer Academic Press.

transfer of genetic material within a population can only be used as a coherent definition of species membership via appeal

> ... to epigenetic and homeostatic mechanisms that maintain the genetic unity of the species in the face of the insufficiency of gene flow to serve this end. But such an appeal immediately raises the question whether these epigenetic mechanisms should not be taken as the decisive criteria of species membership. As Kitcher points out, if it is these mechanisms that are in fact the explanation of the unity of the species ... even an additional requirement that the members of the species be connected by historical links would be no more than an ad hoc attempt to insist on the importance of reproductive connection.[271]

Dupré then addresses phylogenetic taxonomy where the definition of species is shifted to reflect evolution. 'Thus a necessary condition for a group of organisms to constitute a species is that they should share descent from some common set of ancestors.'[272] Discussion of the further conditions that are required to spell out a sufficient criterion for species membership has generated a further set of difficulties. A strict cladistic approach calls for a convergence of genealogical and taxonomic mapping of biological taxa. This presents its own problems leading to weakened versions, such 'that classification not be inconsistent with the genealogical tree.'[273]

> The general motivation for such divergence from strict cladistics is the thought that judgements of similarity and difference should have some relevance to taxonomy independent of the desirability of recording phylogeny. Such positions will thus require some appeal to criteria of speciation distinct from phylogenetic separation.[274]

Finally, Dupré moves on to a pluralistic conception of species. It is a radical pluralism that seems to deal best with all the difficulties encountered, whilst remaining true to the initial

[271] *Disorder*, 47.

[272] *ibid.* 48.

[273] *ibid.*

[274] *ibid.* 49.

insights in biology on which the various species definitions are based. Dupré takes the idea from Kitcher[275] who,

> ... argues that both historical (evolutionary) and structural (or functional) inquiries should be accorded equal weight in biology, and that they may require different classificatory schemes, the latter in some cases demanding a morphological classification. ... Nothing in evolutionary theory guarantees that genealogy will always provide us with the distinctions we need in order to understand the current *products* of evolution as opposed to the process by which they came to be.[276]

That is, Dupré believes that the different results provided by examination of evolutionary and functional studies of species relations and membership, demonstrate the kind of ontological diversity he is promoting, and that it is disingenuous to subordinate one or other approach in describing the (ontological) structure of biology:

> ... I am inclined to suspect that the persistence and intractability of the species problem has much to do with a tension between the assumption that science is concerned with discovering the real and unique structure of nature and the only slowly dawning realization that Darwin has bequeathed us a nature with no such structure.[277]

However, embracing such a position does not in itself show that essentialism is a false doctrine for science, only that the concept of a biological species cannot be simple or open to obvious essentialist theorising. However, in biology there is no shortage of non-essential kind terms at all levels of discussion. Again, it is by looking at real kinds that play important roles in biology that we move away from the original appeal of a Putnamesque account of natural kind terms in science. To this end Dupré turns a chapter over to sex.

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[275] Kitcher, P. (1984) 'Species' *Philosophy of Science* 51, 308-333.

[276] *Disorder*, 50-51.

[277] *ibid.* 51.

> By arguing that sex is indeed an indispensable concept for biology, but also that it admits no possibility of an essentialist interpretation, I hope to support the idea that natural kinds themselves should be reconceived in a nonessentialist way.[278]

It is not necessary to follow through the details of Dupré's argument. The main thrust of his argument is that biological sexes cannot be expressed in essential terms given their wide variety of expression in the natural world. He is not concerned to deny that, in a general sense, we can pick out males and females in sexually reproducing species, only that there is no defining property which makes such a distinction.

> Crucial for my argument against essentialism, is the observation that even if a kind *is* determined by a real essence, the *discovery* of such an essence presupposes the discovery of the kind. Only the most extreme reductionist could suppose that examining a particular individual would allow one to determine to what kind it belonged apart from the prior recognition and at least partial characterization of that kind. This simple observation should be sufficient to raise serious doubts about the empirical credentials of real essences ... [279]

Dupré urges us to take natural kinds as real but without instantiating them through essential definition:

> There is certainly no harm in calling a set of objects that are found to have substantial number of shared properties a natural kind. The discovery of such a kind, however, provides no basis for the supposition that some particular properties can nonarbitrarily be single out as essential. But there is no reason why the term *natural kind* should be wedded to essentialism—or, anyway, no more reason than an accident of linguistic history that could readily be rectified.[280]

The upshot of all this is that Dupré uses the variation amongst the individuals of any set of biological categories at any level of abstraction to present a pluralistic picture of natural

[278] *ibid.* 61.

[279] *ibid.* 63.

[280] *ibid.* 83.

kind terms. There is no unique account of how natural kinds are to be related to one another in a fixed hierarchy of terms—an indefinite number of hierarchy relationships can be constructed depending on the interests motivating the questioner—an individual organism may indeed really be, a blue whale, a mammal, a top predator or a fish.[281] We end up with the possibility that natural kinds overlap and are disordered.

This is not to suggest, as Rosenberg does in his reading of Dupré, that an obvious consequence is a pluralism of methodology or epistemology. In fact, Dupré's insistence on the empirical content of his theory—that natural kind classifications cannot be discovered a priori—is consistent with a more traditional reading of scientific methodology as a whole. He goes so far as to wonder whether the physical sciences are open to the types of criticism he has constructed for biology:

> The only way that we could provide grounds for dispensing with this empirical stance would be if we were somehow to know what the members of certain kinds were completely homogeneous in all respects (or in some set of respects somehow distinguishable a priori). Many people seem to believe this to be true of the kinds distinguished by physics and chemistry, although I find this doubtful. If these doubts are unwarranted, physics and chemistry are, in an important respect, very different from biology. But even if this is the case, it is surely an empirical fact, not something that could be known a priori.[282]

That is, the search for the epistemologically valuable, however that is characterised, need not be abandoned in the face of ontological pluralism emergent from the practice of science itself. Later in *The Disorder of Things* he does argue for a more pluralistic approach to epistemology of science too, citing Feyerabend as a role model,[283] but this is only to dem-

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[281] And presumably butterflies are sometimes moths, and are sometimes not.

[282] *Disorder* 83-84.

[283] *Disorder* 262-264.

onstrate that there is still a place for determining the good parts of science from the bad; that, '[i]t is precisely the importance of recognising the disunity of science that it encourages us to try to sort the scientific sheep from the goats.' It is to Feyerabend's commitment to analysing the social and political aspects of science that Dupré is appealing. Recognising these factors in scientific enquiry does not lead to science studies, however, only to an increased sensitivity to the number of ways we can get the world right in the biological and social sciences, and also to how we make mistakes with disastrous consequences for our fellow human beings.[284]

The pluralism Dupré urges for epistemology is more like a spectrum of applicable epistemic values or virtues for a variety of problems. We have already seen how experimentation can be regarded as such a value when regarded as an agent perspective practice, but that its value is hard to specify without a picture of the general structure of science and (dis)unity. As things stand we can see an emerging general structure that supports the notion that there are a number of rational scientific practices that we use to investigate a disunified world—experimentation being a major one. In other words, positive liberation from the restrictions of reductionism and essentialism does not imply a negative liberation from rational, empirical practices.[285] Although in the end I hold to an almost neo-Kantian account of rationality that I read in Putnam,[286] what is to count as a rational practice at any moment in history can only be discovered by looking at what we are doing (and thinking) to find out about things.[287] In this case I would not want to put forward Feyerabend as a

[284] See the section on Foucault and his use of biology below, p. 175 ff.

[285] This does raise the question of whether picking out current methodological features of science as a whole is consonant with metaphysical disunity. See p. 200 ff. for discussion of this question.

[286] See p. 210 ff.

[287] Having said this, the point I have been urging is that I cannot see how we could ever escape from the idea that manipulation is epistemologically valuable because of our being embodied beings in a physically changeable world with epistemological interests of our own.

champion of the kind of position I am advocating, because I want to support a way of separating good science from bad that does not throw aside the possibility of demonstrating the value of one practice over another.

iv Natural kinds and realism

As already noted, what Dupré does not address in any particular detail is how this picture of ontological pluralism[288] is to be reconciled with a specific form of scientific realism. He says that:

> Certainly I can see no possible reason why commitment to many overlapping kinds of things should threaten the reality of any of them ... I do not see why realism should have any tendency to cramp one's ontological style.[289]

But what this could mean for realism is not explored in any detail in *Disorder*. This issue must now be addressed, because I have been urging that a realist and non-reductive account of the ontology of biology *is* tenable. More specifically, I want to present a way that natural kind terms can be understood as real.

What is scientific realism? In Chapter Two I discussed some issues raised in connection with experiments and realism, and while some of Hacking's claims have an intuitive appeal through the treatment of some philosophically neglected aspects of science, I have already

[288] For further discussion of the nature of promiscuous realism see Wilson, R. A. (1996) 'Promiscuous Realism' *British Journal for the Philosophy of Science* 47, 303-316; Dupré, J. (1996)'Promiscuous Realism: Reply to Wilson' *British Journal for the Philosophy of Science* 47, 441-444; Wilkerson, T. E. (1993) 'Species, Essences, and the Names of Natural Kinds' *The Philosophical Quarterly* 43, 1-19; Daly, C. (1996) 'Defending Promiscuous Realism about Natural Kinds' *The Philosophical Quarterly* 43, 496-500. Dupré's and Daly's responses to the technical problems raised against promiscuous realism about natural kind terms seem to me to be convincing.

[289] *Disorder* 262.

said enough to call into question the application of Hacking's experimental realism to anything other than a falsely idealised notion of the physical sciences. What the discussion of experimentation showed is a need to move beyond the current discussion of realism in terms of truth or verisimilitude of simple propositions, sentences or statements.

The traditional picture of scientific realism[290] gives us a three part account of a theory's ability to refer, its truth value status, and the conditions for its acceptance. That is, the terms of the best current theories in any (mature) science refer to the external world; those theories are true in a correspondence sense (or approximately true, within a specific reading of 'approximate truth'); and acceptance of those theories involves spelling out the grounds for a belief in this truth[291]—the ontology, semantics and epistemology for theories as linguistic entities with a propositional structure (or at least one that is open to propositional analysis).

Whole forests have been pulped to carry debates over scientific realism with regard to the physical sciences.[292] There is a further problem when we try to make the debates fit the biological sciences. It is that biological phenomena and entities seem far more complex than those picked out by physics or chemistry. The whole discussion of natural kinds and

[290] I deliberately simplify the variety of realist theories here in order to demonstrate a common error in dealing with science practice. In this section by 'practice' I do not mean to draw a sharp distinction between theories and experiments, but to include thinking and acting for specific epistemological goals (however these are characterised) in a scientific context.

[291] See, for example, Bas van Fraassen's characterisation of realism in his (1980) *The Scientific Image*, Oxford: Oxford University Press. Chapter 2, where he says 'the correct statement of scientific realism' is (for his purposes) that: 'Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.' 8. Also see, Putnam, H. 'What is Realism?'; Boyd, R. 'The Current Status of Scientific Realism'; Laudan, L. 'A Confutation of Scientific Realism' and Leplin, J. 'Introduction', all in Leplin, J. (ed.)(1984) *Scientific Realism* Berkeley: University of California Press.

[292] As the use of electronic communication increases, it is likely that more of the debate about living entities will rely on the use of silicon and fibre optic technology, than trees and paper. This does not diminish philosopher responsibility to make sure they are using resources for debates that really have a purpose!

essentialism is the product of this complexity. When we turn to biology the possibility of grasping a realist understanding of a pluralistic overlapping, naturally evolving arrangement of categories and things, can seem extremely remote. This leads Rosenberg to claim that the criteria for accepting theories in a realist way are not fulfillable by the biological sciences. Biology, he says, is limited in a far stronger way by our current values and interests than the physical sciences—the same range of values, interests and theoretical starting points that motivate the pluralism Dupré describes. Rosenberg puts it like this:

> [b]iology is more of an instrumental science than physics and chemistry in this sense: if our cognitive and computational powers were vastly greater than in fact they are, biological theory would be much different from what it is, while physical and chemical theories would not be so different from what they are. ... There are interesting generalizations embodied in biological theory that we would miss if we eschewed the descriptive vocabulary of biology—its "natural kinds"—but these are generalizations in part about us and our epistemic resources, as well as generalizations about the world.[293]

The complexity of the biological is, according to Rosenberg, so great that an extension to biology of the kind of realist interpretation we give to the physical sciences is beyond mere humans. The consequence is that only the physical (the micro-physical) objects of the world are real. And as already seen, this has consequences for how we might conceive unity in science:

[293] *Instrument* 5.

> ... we understand why the smooth reduction of biological theory to physical theory is not on the cards. Our understanding is compatible with, indeed rests on, a materialist and mereological determinist approach to biological systems: they are, as we thought, "nothing but" physical ones, even though we cannot systematically derive the biological from the physical. We also understand ... why the doctrine of the unity of science must be qualified. Its epistemological requirements need no qualification. Biology fully honours the requirement of empirical evidence. But its demand that we systematize theory needs to be qualified, and the goals set for the unity of science need to be restricted above the level of physical theory.[294]

This notion of complexity in biology emerges from Rosenberg's accounts of natural selection for function. It is the result of a sharp distinction between the one case where a single molecule has one function (DNA carrying heredity information) and all other biological functions, which are multiply instantiated in many cases. This is an extension of the 'many-many' problem:

> ... at apparently every level above the polynucleotide, *physically distinct* structures are frequently found with some *identical* or nearly identical functional properties, different combinations of different types of atoms and molecules, that are close enough to being equally stable and equally likely for purely physical causes, to foster the appearance of more instances of the kind they instantiate. So far as adaptation is concerned, there are frequently *ties* for first place in the race to be selected. As with many contests, in the case of ties, duplicate prizes are awarded. The prizes are increased representation of the selected types in the next "reproductive generation."[295]

This is one of the reasons why we, with our limited abilities, will never be able to describe the complex biological world without importing our own interests and schemes to structure the description—the multiple realisation of biological functions in physical systems is just *too* dense a network of relations that we could hope to properly say what is going on.

[294] *ibid.* 55.

[295] *ibid.* 27.

> In principle the dispute between realism and instrumentalism should be neutral on the question of whether nature is simple enough for us to discover regularities about it. If we can discover such regularities, the instrumentalist and the realist will fall to disputing whether such regularities provide knowledge beyond the sequence of sensory data to which we are subjected. My thesis, however, is not neutral on the simplicity of nature: it claims that nature is sufficiently complicated that *we* cannot hope to discover regularities that operate at the level of biology. Thus in biology we must content ourselves with heuristic devices, useful instruments.[296]

So the question about how to properly explicate the realism that could support ontological pluralism is brought to the fore, otherwise there is no means to choose between Rosenberg's instrumentalism and Dupré's realism—at least about the ontology of the world biology appears to reveal. This returns us to natural kinds and taxonomy. If it were possible to show just how natural kinds can be related to each other in a complex, pluralistic way *and* how we would want to find theories about that world acceptable, then there would be grounds to favour a realist picture of biology. Such an account is available and is sensitive to the dynamic epistemological models already mentioned in the previous chapter.[297]

Rosenberg's fears about the consequences of pluralism are very like Smith's. Their motivations for trying to secure unity for science are based on the fear that without unity in methodology and epistemology gaps wide enough to allow in such offensive 'sciences' as astrology and creationism are opened in the framework of the rest of our best scientific theories. As I have stated already, and shall continue to demonstrate, these fears are ill-founded.

[296] *ibid.* 7.

[297] p. 53 ff.

1 Kinds and laws

Recent work by Aronson, Harré and Way[299] provides an interesting combination of insights into work in artificial intelligence, epistemology and metaphysics. Their aim is to articulate a form of scientific realism that by-passes some of the problems that have beset old attempts to relate the ontological, semantic and epistemological components of realism—basically by trying to avoid talk of 'degrees of propositional truth.'

> The unique feature of our treatment is the way in which we have substituted a model world serving as a knowledge-representation system for the traditional idea that all knowledge/world relationships must be discussed in terms of a wholly propositional way of representing knowledge.[300]

Although their theory is developed for the physical sciences, their solution to how theories relate to the world as models of type-hierarchies of natural kinds has application to the biological sciences, thus demonstrating the possibility of an articulated account of realism for biological kind terms.[301] Their argument is as follows.

The key is to see scientific knowledge as a model that relates to the world, not principally through truth, but through similarity. They refer to sets of things in the world as 'kinds' and in models as 'types.' Types are ordered in type-hierarchies, to be discussed below.

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[299] Aronson, J. L., Harré, R., Way, E.C. (1994) *Realism Rescued: How Scientific Progress is Possible* London: Duckworth.

[300] *ibid.* 15.

[301] Interestingly, in discussing their account of type-hierarchies Aronson *et al.* refer constantly to biological classification problems and intuitions for examples and counter-arguments to alternative positions.

It is worth addressing the kind of causal mechanisms that are being imputed by Rosenberg. In an unpublished response to Rosenberg, Dupré characterises Rosenberg's position in the following way:

> On Alex's picture it is by virtue of being a certain aggregation of physical particles that the object [a bear] has the causal consequences it does. Perhaps most of the aggregations of physical particles that would succeed in constituting a bear would have the relevant causal consequences, and perhaps there is even a selective explanation for this happy coincidence—an explanation that might even explain why an ontology of bears is one that serves my reproductive interests well enough. But for all this, the ontology, or perhaps the mythology of bears, lions, snakes, and so on is one forced on us only by our cognitive limitations. If we were smart enough we would identify things as just the precise aggregation of physical particles that they were and predict their behaviour from the laws of physics; we would not shackle our thinking to the physically heterogeneous categories of biology.[298]

Dupré goes on to remark that Rosenberg's characterisation of theorising about biological kinds as 'empiricist' is odd, given his acceptance of the reality of only unobservable physical particles. Rosenberg is trapped by the belief that there will be complete causal descriptions of the objects in all our other ontological schemes obtainable from a finished physics. This gives priority to physics that is justified only by arbitrary choice and a seventeenth century obsession with atoms. However, it remains to be seen whether we can be clearer about the reality of ontological schemes in biology. Having exposed the assumptions of Rosenberg's argument for instrumentalism we are certainly in a position to say that there is no reason *not* to take biological kinds as real—the grounds for each case will be as good (or as bad) as you like—but I think it is possible to be more positive in saying how we can understand natural kind terms to fit with disunified pluralism of real kinds.

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[298] Private communication of a verbal response to Rosenberg, 2. Marked ‡ hereafter.

> The basic idea behind our point of view is this: in the real world there is just one set of objects, each of which has its own cluster of properties. Instead of attempting to represent our knowledge of this world primarily in propositions or in terms of sets which are defined only by their members, we propose to represent that knowledge in a model world. ... In our treatment the relation between the multiplicity of properties of one set of objects that constitutes the real world is represented in the model world by splitting the ontology of the model world into a hierarchy of sets of entities, each set distinguished by the properties common to the members, where the multiplicity of properties of an entity in the real world is represented by the multiplicity of sets of ordered pairs of entities each with just one property in the model world.[302]

Ultimately they believe that scientific theories are an expression of the metaphysical structure of the world, which is, I take it, a reasonable form of scientific realism. They survey a number of different ways that kinds have been understood in philosophy and elsewhere—that is, the ways in which kind terms have been seen to get their meaning and have been related to one another—dismissing simple accounts of the use of similarity to group individuals together. The main objection to these theories is that any two objects can be seen to have an indefinite number of similar properties.[303] This leads them to acceptance of an 'open texture' account for scientific discourse, based on Waismann's[304] modification of Wittgensteinian 'family resemblance':

[302] *ibid.* 16.

[303] It is this that Dupré is trading on, in part, in his criticisms of traditional accounts of natural kinds. Aronson *et al* also note some problems with robins:

> "Keil (1979, 1981) has pointed out that many commonplace categories such as 'robin' and 'squirrel' collect up diverse entities that share many important properties that almost never show up in people's listings of attributes for a category. For example, has a heart, breathes, sleeps, is an organism, is an object with boundaries, is a physical object, is a thing, can be thought about, and so on." *Disorder* 21.

[304] Waismann, (1968) *How I See Philosophy* London: Macmillan.

> According to Waismann the structure of a scientific terminology is such that there is always an open question as to how our ways of describing a new instance are to be fitted into an existing type-hierarchy. No hierarchy is so complete that it is predetermined how we should use it to deal with marginal cases. ... Type-hierarchies are sensitive to the state of our empirical knowledge and the articulations of our theoretical concepts.[305]

This is consistent with the empiricism at the heart of Dupré's theory of natural kinds. Similarly, Aronson, Harré and Way dismiss accounts of kind meaning that refer to 'prototypes' and 'cores' for reasons that appear to accord with Dupré's. 'The prototype seems to function like the nominal essence, while the core sounds suspiciously like real essences.'[306] There is a difference here, however. Dupré, following Cartwright, limits the application of laws to restricted cases as *ceteris paribus* laws, whereas Aronson *et al* embrace the notion that natural kind terms operate as they do because of fixed laws:

> ... the idea that the extension of ... natural kind words ... are not fixed by a set of 'criteria' laid down in advance, but are, in part, *fixed by the world*. There are *objective laws* obeyed by multiple sclerosis, by gold, by horses, by electricity; and what is rational to include in the classes of entities constitutive of these kinds will depend on what those laws turn out to be.[307]

Aronson *et al* are more committed to there being a discoverable, objective and regular ordering to the world. The question is, does this entail a commitment to an essentialist definition of natural kinds, one that would put this theory in conflict with Dupré? I do not think so. What these considerations reveal is a distinction in Dupré's account of realism for natural kinds that needs to be appreciated with care. We need to return to the distinction Dupré makes between a strong and weak reading of natural kinds. His thesis is that natu-

[305] Aronson *et al op cit.* 24.

[306] *ibid.* 26.

[307] *ibid.* 40.

> ... realism *vis à vis* natural kinds does not commit one to the belief that there is a 'unique best taxonomy in terms of natural kinds' The belief that there are many orders of natural kinds is perfectly compatible with the claim that natural kinds are objective and carve nature at its joints. What type-hierarchy we choose to work with may depend on the type of problem we are trying to solve, the nature of the phenomenon under investigation, and so on.[311]

This can be made consistent with Dupré's pluralism. Disagreement over the nature of laws might well be problematic if one were to accept the whole of Aronson's *et al* theory of scientific realism, since they insist that dispensing with the semantic and epistemological parts of realism about science has been a failing of past theorising under the rubric of scientific realism.[312] However, I see no reason for one not to support the idea that scientific laws are far more restricted in scope *and* are also the structure behind the natural kinds we find it useful to describe: Dupré notes,

> ... the fact that physicists are doing something very well doesn't imply that they have the most sophisticated grasp of what they are doing so well. (If it did there would surely be no use for philosophers of science ...)[313]

In any case I am not proposing that all of the theoretical position of Aronson *et al* is taken up, only that their analysis of natural kind terms can be used to support the disunified pluralism discussed.[314]

Where we need care is in reading what follows from this interpretation. Aronson *et al* want to argue that natural kind terms understood in this way reveal (and are revealed by) the

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[311] *ibid.* 42. See also Boyd, R. (1990) *op cit.*

[312] *ibid.* Chapters One, Six and Nine.

[313] Dupré ‡ 4.

[314] I am aware that I leave myself open to criticism by using the material from *Realism Rescued* in this way. It is possible that Aronson *et al* have made some fundamental errors that I uncritically incorporated into my account here. However, I feel that as an attempt to articulate certain difficult problems with natural kinds that does not fall back on positivist formalism, their position, whilst itself using older theories and discussions, does advance the debate about how we *might* take natural kinds to be real to a considerable extent.

nomological structure of the world. The only alternative to their form of realism about natural kind terms is conventionalism—which they take to be the view that categories and kinds do not exist independently of their description by human beings. They allow for interests and conventions to enter into their theory of kinds, but only in a limited way and they leave unanswered questions that we might raise about the ontological status of kinds found in the social sciences:

> The principle that any classification scheme must pay homage to the actual causal powers in nature does allow for a measure of convention to enter into the way we classify things but not in the way the conventionalist intended. Choice between competing schemes may be a matter of convention to the extent that each system is compatible with the causal structure of the world. If the choice between two conceptions is arbitrary, ... then they reflect the causal structure of the world equally well (or badly).[315]

They point out that many of our everyday categories and kinds are the result of a mixture of conventional and natural (causally independent) choices. And they also note that there are a wide variety of cases that are a combination of natural and artificial classifications.[316] However, what their notion of conventionality implies is that there are limits on what is to be classed as natural. '[T]he naturalness of kinds,' Dupré also observes, 'will turn out to be a matter of degree: some kinds will turn out to be a good deal more natural than others.'[317] Obviously there are categories of things that are made by human beings having particular interests. 'Surfaces that are good for use as an artificial football pitch' is one such category. We may ask: what *makes* them good as artificial footballing surfaces? what property(ies) do they have that allows us to identify them and thus leads us in pursuit of other such surfaces? And the answers may point to an 'open textured' (family resem-

[315] *ibid.* 43-44.

[316] *ibid.* 44.

[317] *Disorder* 63.

blance) response, there being no single property that all such surfaces share. But once again, it does not follow from the idea that there are no essential properties picking out kinds that there is no real kind of surfaces good for football matches independent of human beings classifying them as such.[318] But neither does this position imply that *all* kinds are real. Before I say any more on this, let me fill in the Aronson *et al* account of natural kind terms.

2 *Hierarchies*

> It is important to bear in mind that we are not primarily concerned with the question of how we decide whether an individual belongs to a kind, artificial, conventional or natural. Rather our project is to give an account of what it means to say that it does.[319]

Types are ordered into a hierarchy—types, you will recall, being representations of kinds. These are related via a semantic network, an idea developed in AI, for the representation of how types are ordered in a hierarchy of increasing complexity (a taxonomy). In this ordering (taxonomy) types are nodes in a network. Higher types that include one or more lower types are sometimes called supertypes and the lower types are called subtypes. For example, 'bird' is a supertype that includes the subtypes 'duck' and 'goose.' This results in a shift away from dealing with individuals represented in propositional form:

[318] The issue of whether and how we might draw a line between the entirely made up human constructions and real kinds will have to wait for the next chapter.

[319] Aronson *et al op cit.* 15.

> Instead of speaking of isolated substances and their properties, we speak of systems where the internal relations among properties determine the system as being of a particular type, captured by giving it a specific location in the type-hierarchy. ... It is the very same location in an ordering of natural kinds that enables systems, in the above sense, to serve as models. In other words, ontological atomism is replaced by global-ontological relationalism. And here we ... learn one way in which this particular metaphysical doctrine has serious ramifications for the epistemology of science. We cannot make categorical or counterfactual predictions ... until we first determine the specific nature of the system with which we are dealing.[320]

We are returned to one of the questions from the end of the last chapter—how deep will our ambivalence about the kind of things that we manipulate in experiments reach? Once we understand a little more fully the system being described here, we shall be closer to an answer to this question. In fact Aronson *et al* are aware of the need to include material agency as part of their account (although they do not use this term).

> It has been customary for philosophers to take 'science' as something made concrete as discourses: that is, as journal articles, text books, monographs and lectures. ... 'Science' also takes concrete form in models and experimental procedures. Models are real or imagined representations and analogues of naturally occurring entities, structures and processes. Experimental procedures not only lead to observable results but involve the manipulation of substances and entities which human beings are unable to observe.[321]

It is modelling that is crucial to the type-hierarchy representation of natural kind terms. Models are indispensable parts of our descriptions of the world. Indeed for Aronson *et al* type-hierarchies just are models of parts of the world. As I pointed out with explanation, universal answers to specific questions that include everything, are of no value—a model of the Universe that includes everything in it (including all models) would be valueless. So we need a procedure for determining which models are better than others in picking out important analogies between the model and the world, analogies that will do work for us in

[320] *ibid.* 6.

[321] *ibid.* 3.

explaining other phenomena and entity behaviour. More basically we also need a way of characterising the connection between the nodes of our models— that is, capturing the relationship between types that actually does model the kind of relationship between kinds in the world.

Looking at the basic issues first, Aronson *at al* point that defining hierarchy relationships in terms of class inclusion (and modifications thereof) fail because of problems in distinguishing between classes that have co-extensive membership. To use a well worked example, all organisms with a heart also have lungs, so simply defined extensionally 'organism has a heart' will be just the same type (node) in the type-hierarchy (network) as 'organism has a lung.' This is unacceptable since, obviously, we want to make a distinction between having a heart and having lungs for modelling and explanatory purposes in real science contexts. Adding an account of meaning, intension to the class inclusion relation does not solve the problem.

> The fact is that if we use class inclusion to order the nodes of the hierarchy, then the nodes still have to denote classes. For every intension that is a corresponding extension. Thus, although we have a sense or meaning for the concept 'dog', it also will have an extension, which is all the actual (and possible) dogs. By introducing the notion of a class intension, we are able to distinguish two classes in our intensional network by virtue of their members having different defining attributes ... .[322]

But where two categories are again co-extensive, any subtype will have the supertype of both classes. That is,

> ... if the relation between these intensions is class inclusion, then what we really have are two classes whose members are identical and, as a result, whose subtypes have multiple parents.[323]

[322] *ibid.* 30.

[323] *ibid.* 31.

For example, 'has a four chambered heart' is a subtype of 'has a lung' as well as 'has a heart.' From these considerations Aronson *et al* conclude that, despite the apparent power and formally well established benefits of class membership as the defining relationships between types,

> ... it is inadequate to the task of representing the relation between the nodes of a hierarchy in a way which naturally motivates the actual arrangement of subtypes and supertypes that we find in hierarchical systems in use in science and elsewhere.[324]

So they use the notions of determinate and determinable to define how one type falls under another. This gives them a way of pointing out the specificity of the relation—'the determin*ate* ... is more *specific* than the determin*able*.'[325] They begin with Searle's discussion of this relationship to spell out the conditions for types falling under other types.[326] The criteria are:

1. **Specificity**. Types at the lower levels imply the types above them to which they belong, but not *vice versa*, ' ... a determinate entails its determinable but the determinable does not entail its determinate.'[327] For example, if there is a simple type-hierarchy BIRD—DUCK—MALLARD— then if *Donald* is a duck, then he is a bird, but it does not follow that he is a mallard.

2. **No differentia**. That is, there is no "genus-species" relation where a third, logically independent property determines the more specific type. A mallard is a duck, but there is no extra, essentially defining property that makes the grouping of creatures 'mallard.' '

[324] *ibid.*

[325] *ibid.*

[326] Searle, J. (1959) 'On determinables and resemblances' *The Aristotelian Society for the Systematic Study of Philosophy* Part II. Supplementary volume 33, 141-158.

[327] Aronson *et al op cit.* 33.

... for a term A to be a determinate of a term B, A must be an *undifferentiated specifier* of its determinable B.'[328]

3. **No conjunction of determinates**. This excludes determinables that have parts where only one part is functioning as the determinable. For example, in the type-hierarchy given we want to exclude the possibility of 'fat duck' being a determinate of 'bird' 'Being non-conjunctive entails being *undifferentiated*, so we now have as a necessary condition of A's being a determinate of B that A is a *non-conjunctive specifier* of B.'[329]

4. **Determinable and determinate are logically related**. This requirement states that the more specific determinates should be logically exclusive of one another in any particular type-hierarchy. A duck cannot at the same time be a goose. Of course, we may at different times use different schemes to call an individual a duck and later a goose. This excludes determinables such as 'duck and happy' where 'duck' and 'happy' can be determinates. Aronson *et al* quote Searle's own description of this criterion:

    Genuine determinates under a determinable compete with each other for position within the same area, they are, as it were, in the same line of business, and for this reason they will stand in certain logical relations to each other.[330]

5. **Same level determinates**. The determinates of a given determinable are at the same level in hierarchy. For example, 'duck and 'goose' are at the same level. 'Two terms A and B are *same-level determinates* of C if and only if they are both determinates of C and neither is a specifier of the other.'[331]

328 *ibid.*

329 *ibid.* 33-34.

330 Searle *op cit.* 148.

331 Aronson *et al op cit.* 34.

In Searle's account of the determinable-determinate relation there is an unanalysed notion of entailment. Determinates entail determinables. So we still need to find the precise nature of the relationship between the parts of the type-hierarchy so that it becomes clear what 'logical relation' involves. Aronson *et al* make this point further:

> There is another more serious problem with using entailment to explicate the relation between determinables and determinates. It is the same problem that confronted the ... rival analyses: if two concepts are co-extensive, they can be uniformly substituted in any entailment relation. Thus by using entailment as a primitive ... Searle has reintroduced all the problems of co-extensive classes. ... Thus, like classes and class inclusion, intensions and class inclusion, entailment is too weak a relation to capture the structure of our concept.[332]

In order to resolve this problem, we need to modify the relation rather than junking all of Searle's criteria. At this point Aronson *et al* make explicit an important feature of how they are using the notion of a semantic network.

> Semantic networks which have types organised according to levels of generality also support a very important property, that of *inheritance*. In a hierarchically structured semantic net the properties and relations of any given type can be *inherited* by all of its subtypes. ... This is called *an inheritance hierarchy*, or sometimes an *isa*-hierarchy.[333]

It is the idea that subtypes inherit the properties of the supertype to which they belong that structures the hierarchies for Aronson *et al*. A mallard *isa* duck *isa* bird and in being ascribed to the type mallard inherits the properties of being a duck and being a bird.

[332] *ibid.* 35-36.

[333] *ibid.* 36-37.

> Thus what collects the determinates under a determinable is the fact that they are all capable of inheriting meta-properties from that determinable. ... Inheritance gives us a way, then, to non-arbitrarily structure hierarchies: according to whether or not the subtypes of determinables can take on the meta-properties of the supertype or determinate parents. The notion of inheritance is also able to explicate Searle's notion of the logical relation between determinates under the same determinable. They are all 'in the same business' because they all inherit a set of meta-properties from their determinables.[334]

This means that we can generate a sixth criterion for the structuring of type-hierarchies:

6. **Inheritance of properties.** ' ... for any two terms A and B, If A is a determinate of B then for any property P, if P is a second-order property of B then P is a second-order property of A.' For example, if we specify 'having a beak' as a property of 'bird' and define it in terms of 'hardened skin around the mouth' and 'evolutionarily adaptable for variable feeding and display purposes' then we would understand 'having a beak' through the same properties for 'duck.'

What we now need to do is define what is in fact meant by inheritance. Aronson *et al* do so through identity relations by arguing that we need to find the laws describing the properties for each type in the hierarchy and then show that properties at lower levels are just the same properties (and therefore manifestations of laws) of a higher supertype.

> To say ... of a given object that it is a kind of thing means that the object in question is the same as one of the combinations or arrangements of objects represented by the relevant supertype, a combination or arrangement that is a way in which the common 'supertype' property is realised. It ... means that the common subtype property is *identified* with the common supertype property. The identity, here, is between the macroscopic property common to the things presented by the subtype and a macroscopic property common to all the combinations of entities represented by the supertype.[335]

[334] *ibid.* 38.

[335] *ibid.* 45.

I shall not pursue these details any further. As suggested, it serves as an example of how we might understand what it means to say that we can grasp a diversity of ways of classifying an object without supposing that this classification of things is any less robust than a simple unifiable system. It does not follow that all systems will work in this way, nor that this model will work for all fields of science—discovering that would involve a survey each area separately. But we do have a way of beginning in this enterprise, and hence a way of facing critics who would doubt the rigour of a philosophy of science that strays a long way from the formal, positivist, unificatory epistemological and metaphysical ideal.

Unfortunately what this account of natural kinds, as I have sketched it, has *not* shown is how we can understand scientific realism about kinds. Although we have good grounds for rejecting monism and instrumentalism, and we can now be fairly specific about how to understand natural kind terms and their expression in scientific theories, my argument has not directly given grounds for a realist reading of natural kinds. Aronson *et al* do so by providing an account of verisimilitude that attempts to avoid the problems of accumulating truth as a collection of true propositions. In so far as this leads us to embrace practice and the non-propositional components of science it can only be for the good. But as already noted, Aronson *et al* rely heavily on a straightforward notion of scientific laws as universal, fundamental and expressible. If this is rejected, the defence of realism does not of necessity fail, but it does look more disunified. It should be obvious that I do not consider this a bad conclusion to draw. The rest of the thesis will show how deep this methodological disunity might be and why a rejection of scientific realism and the idea that realism about science is a *reasonable* assumption, cannot be countenanced.

v Manipulating real kinds—*some* questions answered

I have argued that experimentation is an epistemological virtue based on our ability to manipulate things beyond ourselves. This virtue is found in the context of science as well as elsewhere. We can now see more clearly how we might characterise the context for science. Scientific theories are instantiated in models of parts of the world. The type-hierarchies that these models form, as Aronson *et al* describe them, are analogues of the ordering of real natural kinds. Natural kinds reflect the law-like generalisations that we draw about the behaviour of the world, and the world participates in the forming of these laws through material agency, which in turn is used by us as the core of manipulation and, hence, experimentation.

However, our understanding of the laws of nature is limited by the *ceteris paribus* character of their application. Consequently, since we have thereby limited the nature of the laws we can form, we only ever construct type-hierarchies that are orders in part—every context of interest reveals a different ordering of types. Types are related to natural kinds through identity and analogy in a way that, again, can be spelled out by use of the Aronson *et al* theory of natural kinds as described above. Taken together with the analysis of the failure of reductionism, essentialism and any minimal account of unity the result is a realistic, pluralistic and disunified reading of the natural kinds in biology. We do not need to lapse into instrumentalism to comprehend the complexity of biological kinds.

Aronson *et al* argue that their ontosemantics determines that we cannot just *do anything* in an attempt to discover the nature of things in the world; we do need a context of kinds in which to carry out our manipulation. I do not believe that this is in conflict with the kind of ambiguity that Gooding discusses. Gooding is concerned with cases where we have some uncertainty about whether the things we observe in experimental situations are in fact really entities of specific kinds or *outside* the possibilities for the experiment altogether. Further investigation would help to resolve this ambiguity, but the sort of experiments that are carried out do rest on assumptions about the kind of things that are assumed to be real. This naturally brings us back to Hacking's experimental realism, but the final demonstration of why it fails on its own will have to wait for the next chapter.

What we are left with is a problem about how to decide the status of the vast range of type-hierarchies we might construct. Aronson *et al* are happy to admit that there are systems that are conventional and based on artifice, and human interests beyond the epistemic. For our present concerns, the question is this: how are we to understand the possibility that some of the types we find in parts of biology that deal with human beings have no analogy in real natural kinds? Does not the possibility of the social construction of types, and hence kinds, undermine the possibility of grasping anything real. This will be the subject of the next chapter, and will provide a solution to the issues about experiments and realism.

Haiku

How many questions

Are there on a forest floor?

What's a forest floor?

4 Foucault and the Construction of Kinds

When I heard the learn'd astronomer,
When the proofs, the figures, were ranged in columns before me,
When I was shown the charts and diagrams, to add, divide, and measure them,
When I sitting heard the astronomer where he lectured with much applause
in the lecture-room,
How soon unaccountable I became tired and sick,
Till rising and gliding out I wander'd off by myself,
In the mystical moist night-air, and from time to time,
Look'd up in perfect silence at the stars.[336]

i Introduction

Having put in place the metaphysics for a pluralistic, non-reductive biology we now need to return again to the issue of realism. Having an account of a possible metaphysics for biology is not enough to properly articulate the science and its impact on our lives, since it is more than 'mere theory,' but also a practice sensitive to the agent perspective discussed in Chapter Two.[337] The question that we have been concerned with throughout can be put like this: by what criteria are we to judge the reality claims for objects of study of a field of enquiry? This is a question that is only answered once we can also make sense of that field of enquiry as practice.

We saw in Chapter Two how Hacking articulates an experimental realism for the physical sciences and why there is some doubt about the extension of this form of realism to other

[336] Whitman, W. (1975) 'When I Heard the Learn'd Astronomer' *Walt Whitman: the Complete Poems* Harmondsworth: Penguin Books, 298, originally published in *By the Roadside*.

[337] See p. 26 ff.

disciplines. Hacking is not particularly helpful here. Whilst he acknowledges that pluralism is an entirely acceptable stance,[338] he wants to draw a clear line between science that deals with entities in an objective mind-independent world and sciences that construct kinds of people for us to discuss and into which we can transform ourselves. So there is an immediate question about whether classes of things that are *not* people can be real. Of course, given the discussion of the previous chapter this question is most pressing for us in any analysis of biology that supposes that a plurality of kinds is real, and that biology is an experimental science, which it clearly is.[339] We have seen that the manipulation of classes and kinds of things is a part of biological experimental practice in experiments with crop yields and plant behaviour under environmental pressure. And in studies of human sexuality, types are manipulated in models to find further correlations of types to supposed kinds. However, Hacking applies his analysis only to theoretical entities. So one solution to the tension here would be simply to say that kinds are not real in the way the entities of particle physics are—species in this context just are individuals, as the pluralist approach to species suggests. But even if it could be argued that we need to treat species and strains as individuals whose properties are being manipulated in these cases, we are certainly not dealing with unobservable entities that somehow remain free of our schemes of classification—whether this classification is based on common sense, phylogenetic, or ecological taxonomies. That is, treating a species as an individual rather than a class does not provide us with neutral entities free from the taxonomy in which we find these entities. This problem remains: how might we understand Hacking's experimental realism in any case that moves away from its initial application to theoretical unobservables?[340]

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[338] Hacking, I. (1992a) ' "Style" for Historians and Philosophers' *Studies in History and Philosophy of Science* 23.

[339] See the examples, p. 78 ff.

[340] There remains the problem of how one should pick out these unobservable entities without wondering about their belonging to a classification of any sort. However, I do not think that a

In what follows we shall see more clearly the motivation behind Hacking's program and how we can reinterpret his experimental realism in a pluralistic metaphysics of science that uses experiments as a central methodological tool. I also address the issue of how biology should be seen in relation to the social sciences—an important relationship, given the impact of biology on our self understanding and the discussion above.[341] My aim is to complete my analysis of Hacking's philosophy of science and show what can be salvaged from his experimental realism that is of value for biology. I do this by taking a somewhat unusual route through the work of Michel Foucault. It will emerge that correcting Hacking's reading of Foucault gives us a better, more nuanced description of biology's relationship with the physical and social sciences, and points the way to an overall assessment of biology and scientific realism.

To begin with a sketch, Hacking's basic claim with regard to the relationship between the natural and the human is that,

> In natural science our invention of categories does not "really" change the way the world works. Even though we create new phenomena which did not exist before our scientific endeavours, we do so only with a licence from the world (or so we think). But in social phenomena we may generate kinds of people and kinds of action as we devise new classifications and categories. My claim is that we "make up people" in a stronger sense than we "make up" the world.[342]

He takes two influential, but divergent philosophers as central to this distinction, suggesting that Kuhn and Foucault have done roughly the same thing for the natural and social sciences respectively. According to Hacking, they have each introduced an aspect of a

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criticism generated from a position that placed the metaphysical horse before the pragmatic cart is entirely appropriate here. This is not to deny the importance of the point: there are no bare particulars.

[341] See p. v ff.

[342] Hacking, I. (1995) 'Three Parables' in *Pragmatism: a Contemporary Reader* Goodman, Russell B. (ed.)(1995) London: Routledge. 241.

nominalism into our understanding of the categories that structure each of these areas of enquiry. What this actually means will become apparent as we proceed. But in short, for Hacking's nominalism the transparent rationality of following this or that line of enquiry is not a feature of the world: it is the product of the taxonomies we have *chosen*. Hacking has argued that Kuhn and Foucault address different issues—natural sciences and human sciences—in different ways because there is a fundamental resistance from the world to our actions on it that is absent in the construction of accounts of human beings. Consequently Hacking's Foucault does not talk about the physical sciences, and his Kuhn does not discuss the social sciences. This leaves the biological sciences undiscussed, as we have already noted. We need to look in more detail at what Hacking says about Foucault to see why this is the case.

Foucault often seems to be referring to biology, giving various roles to the development of the understanding of life and the medical application of biological knowledge in his writings across all stages of his work—from the archæological, to the genealogical, to the ethical. Yet, in most cases this reference is indirect. Most obviously in *The Order of Things*[343] he discusses the development of biology from natural history in the eighteenth and early nineteenth century. The key transformation is from a procedure of listing and classifying to a recognition of function as a defining property of life. But even here, where Foucault sounds most like a structuralist, the notion that biology's history—beginning in an *episteme* of classical ordering and listing—should lead us to believe that modern biology is to be understood as no more than a particular way of talking, or writing, is not followed through by Foucault. So Foucault's attitude to biology is not immediately clear.

[343] Foucault, M. (1970) *The Order of Things An Archæology of the Human Sciences* London: Routledge [trans. of (1966) *Les mots et les chose* Paris: Gallimard].

An outright rejection of Foucault's insights into our institutionalised 'normalising techniques'[344] is unwarranted,[345] yet his overall strategy often seems ill at ease with *any* form of scientific realism, *if* we suppose that Foucault was ultimately concerned to undermine the possibility of any philosophically sound account of how we would justify knowledge of an objective world at all. If we are reading Foucault in this way, there is no reason to suppose that any aspect of science should give us objective knowledge that is unstructured by our linguistic practices founded in socio-political interests. For Foucault, biology plays a crucial role in the variety of categories that crop up in his studies of the construction of human kinds—sometimes it seems that this reading is the correct one. This is an important issue. Having partially dispensed with Rosenberg's instrumentalist challenge from a shared rational stance, are we then faced with the charge that this initial assumption of rationality should be abandoned? If we take biology seriously *can* we maintain that there is a clear distinction between the natural and the human, when biology has such an immediate impact on questions of human nature at the end of the twentieth century, and the human sciences seem in a state of continuing, spasming self-reflection? Hacking wants to secure natural science by making this distinction clear without providing any indications of what happens to biology.

Let us first uncover some shared background. I shall firstly explore a series of connections between Hacking, Kuhn, Bachelard, Canguilhem and Foucault. Then I shall present a reading of Foucault that makes more sense of his expressed opinions of his own philosophical project, than the selective interpretation given by Hacking. I shall then support

[344] Foucault's term for the means by which we come to accept the kinds and classifications of ourselves that are the norms of our culture. Famously Foucault discusses the mad, the sick, the criminal, the sexually deviant, and so on.

[345] For this claim I offer no defence here, except in so far as Foucault's whole program can be interpreted as consistent with an objective reading of rationality in the way I shall suggest below.

this with examples of where we would expect to find Foucault using biology in his histories. Finally, I shall return to Hacking to pin down the faults in his epistemology that the discussion reveals. In the last section of this chapter I shall briefly look at how we might correct recent historiography of discussion of sex hormones, given my analysis of Hacking and Foucault.

ii Hacking on Kuhn and Foucault—nominalism in philosophy, science and society

Hacking has great respect for Kuhn. 'No one from his generation has had a more dramatic impact on the philosophy of science than T. S. Kuhn ... the totality of the work of this historian places him among the major philosophers of this century.'[346] Although the paper that contains this praise was written over ten years ago, it is still generally true that, 'any discussion of the relation between history and philosophy of science will begin with *The Structure of Scientific Revolutions*.' This, he notes, is peculiar since Kuhn wrote only on the natural sciences (indeed only on the physical sciences) and, 'there is a time-honoured opinion that history matters to the very content of the human sciences, while it does not matter much to the natural sciences.' It is not through the application of history to the comprehension of ideas that marks Kuhn out for Hacking. In fact, Hacking suggests that on the whole Kuhn has been ignored by the majority of scientists working in the natural sciences in this respect: ' ... he did not succeed, and could not have succeeded, in historicizing natural science.'[347] Where Kuhn's power lies is in the analysis of the construction of the order on the world:

[346] *ibid.*

[347] *ibid.* 240.

> I hold that Kuhn has importantly advanced the nominalist cause by giving some account of how at least an important group of "our" categories come into being in the course of scientific revolutions. There is a construction of new systems of classification going hand in hand with certain interests in describing the world, interests closely connected with the "anomalies" on which a community focuses in times of "crisis." At the same time this cannot lead us to a *very* strict nominalism, for the anomalies "really" do have to appear to be resolved in order for a revolutionary achievement to be recognized. Removal of anomaly is never enough, Kuhn has taught, because all sorts of social conditions are needed for a revolution to "take." But reality has to go some part of the way—more than a wider, stricter, nominalism would allow.[348]

Hacking has further clarified his reading of Kuhn in 'Working in a New World: The Taxonomic Solution.'[349] The nominalism that Kuhn advances in *Structure*[350] has a fairly simple form, despite the ontological relativity that seems to be implied by talk of new worlds after scientific revolutions—what Hacking calls 'the new-world problem.' Hacking proposes that while the Kuhnian nominalist accepts that the world itself does not change during a change of paradigm, that world is composed only of individuals; the world that we describe from within a particular paradigm is one composed of 'kinds of things.'[351] Thus it is possible for us to speak of living in a different world after a revolution: it is the taxonomy of the world, the ordering of the kinds of things that it contains as understood, described and manipulated by the scientific community, that has changed.

Hacking is the first to admit that his 'is an unusual approach to Kuhn's past and even present writing.'[352] It is, however, most instructive in understanding *Hacking's* philosophy of

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[348] *ibid.* 240-241.

[349] Hacking, I (1993) 'Working in a New World: The Taxonomic Solution', in *World Changes, Thomas Kuhn and the Nature of Science* ed. Horwich, P., Boston: Bradford Books, MIT, 275-310.

[350] The nominalism that leads Kuhn to say that, 'though the world does not change with a change of paradigm, the scientist afterwards works in a different world ... ' – Kuhn, T. S. (1962) *The Structure of Scientific Revolutions* Chicago: Chicago University Press, 121.

[351] Hacking (1993) *op cit.*, 277.

[352] *ibid.* 280.

science. Firstly, it gives further insight into how methodological unity in science, understood through a philosophical examination of experimentation may contain confusions with ontological unity. Secondly, if correct as an account of what scientific realism is (that is, experimental realism), its usefulness can be tested by observing its applicability to all of the natural sciences, that is, to the biological as well as the physical sciences. If we suppose that biologists also experimentally create phenomena, does Hacking's position have any power, especially when taken together with the recognition that biology can be used in the 'making up' of kinds of people, and even sometimes looks like the human sciences as Foucault addresses them?[353] Without a stronger commitment to giving a place for the metaphysics of science to provide an appropriate context, experimental realism looks empty with regard to the proposal that the world itself does not change in the application of revolutionary nominalism, as he labels Kuhn's position.

Hacking's reading is confirmed when we look at the expressed relationship between Kuhnian nominalism and Hacking's own position:

> ... Kuhn leads us into a "revolutionary nominalism" which makes nominalism less mysterious by describing the historical processes whereby new categories and distributions of objects come into being. But I assert that a seemingly more radical step, literal belief in the creation of phenomena, shows why the objects of the sciences, although brought into being at moments of time, are not historically constituted. They are phenomena thereafter, regardless of what happens. I call this "experimental realism."[354]

This, Hacking claims, places him in similar territory to that of Gaston Bachelard who 'believed in scientific accumulation and *connaissance approchée*,' that 'what we accumulate are *experimental techniques* and *styles of reasoning*,'[355] not knowledge. It is worth

[353] See p. 175 ff.

[354] Hacking (1985) *op cit.* 244.

[355] *ibid.*

looking at this claimed similarity because Bachelard is formative for Foucault, the source of human science nominalism. Hacking's Foucault was concerned with uncovering the hidden processes of category creation, the coming into being of kinds of people. As shall be made plain shortly, Hacking wants to use the *differences* between Kuhn and Foucault, despite their shared rejection of essential ordering categories, to lend support to his own general attitude to understanding the natural sciences. Locating himself alongside Bachelard, Hacking is linking himself to a constellation of ideas in French philosophy—not the usual French considerations of the subject and her experiences, but the examination of knowledge, reason and rationality. Gary Gutting calls this the 'Bachelard-Canguilhem network.'[356] Both Bachelard and Canguilhem can be seen to have been significantly formative for Foucault, and they are both important in deepening our understanding of Foucault's position on a number of questions about the natural sciences.

Their influence can also be seen in other writers concerned with the questions Hacking finds interesting. Certainly the idea that there are breaks in the historical development of science is a key feature of Bachelard's work, decades before Kuhn,[357] and it is clear that he was sensitive to the practices and techniques that make up the day-to-day activities of scientists; Mary Tiles states,

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[356] Gutting. G. (1989) *Michel Foucault's Archaeology of Scientific Reason* Cambridge: Cambridge University Press. Chapter 1.

[357] Gutting [*op cit.* Chapter 1] picks out two different uses of "break" in Bachelard's work—between science and common-sense and between 'two *scientific* conceptualizations' (p. 16). As Gutting points out, all the notions of "*rupture*," "*coupure*," "*mutation*," and the like that Foucault, Althusser and others popularised in the 1960s are directly taken from Bachelard (p. 52).

> Bachelard is concerned with ... the dynamic processes of correction, revision, rejection and creation of theories, with the dynamics of the experimental and the theoretical practices of science ... his concern is not with scientific knowledge as expressed by theories, but with the knowledge, the understanding of scientists which enables them to make scientific advances. The knowing subject is never absent from Bachelard's epistemology, and, perhaps most importantly, this subject is historically located.[358]

Hacking writes on these issues too, as my earlier examination of *Representing and Intervening* showed. However, Bachelard was alert to the notion that science just does not display a unified *application* of rationality. Examination of the history of science will uncover only a regional application of rationalities, '*les régions rationelles*.'[359] This regionalism does not imply the all-embracing *epistemes* of Foucault's *The Order of Things*—it is a recognition of the potential impossibility of unifying science across the history of particular fields, or between these fields. It is hard to find such sensitivity in Hacking—he moves back and forth from statements about the physical sciences to sweeping claims for scientific realism for science as such.[360] Hacking may even be read as implying a simple metaphysical reduction of biological categories to physical ones through his approach to experimentation, although it is unclear that he could consistently state this explicitly. Furthermore, Bachelard did not support the kind of phenomenological realism about everyday objects that Hacking quite obviously upholds with his talk about the extension of the notion of the real *from* everyday objects to the world that physics describes. By contrast Bachelard 'vigorously defends the reality of the entities postulated by explanatory scientific theories and even maintains that it is these entities rather than the objects of ordinary expe-

[358] Tiles, M. (1984) *Bachelard: Science and Objectivity* Cambridge: Cambridge University Press, 9.

[359] Bachelard, G. (1949) *Le rationalisme appliqué* Paris: PUF, Chapter 7.

[360] See, for example, his thoughts about the manipulation of biological entities and light microscopy in *Representing and Intervening*, a book otherwise concerned with physics and spraying electrons. His supposed pluralism is ambiguous in these contexts.

rience that are the concrete realities of the physical world.'[361] Again this touches on what kinds of pluralism and realism Hacking is defending. He often seems to be saying that we suppose everyday objects to be real because he can do things with them, hence manipulations and interventions are our best criteria for judging theoretical things to be real. Again, this seems not to exclude kinds of things as he claims it does.

However one chooses to look at Bachelard and Hacking these surface similarities and differences are apparent when we look at what Hacking says about Foucault:

> Foucault's books are mostly about practices and how they affect and are affected by the talk in which we embed them. The upshot is less a fascination with words, than with people and institutions, with what we do for people and to people. He does have a noble obsession with what he takes to be oppression: the asylum, the prison, the hospital, public hygiene and forensic medicine. His view of these practices may be entirely wrong ... But one thing is clear. Foucault ... has not been locked in a cell of words. Moreover, it is precisely his intellectual work, his philosophical work, that directs our attention away from our talk and on to our practices.[362]

Hacking too has 'a noble obsession' with practices that create. But, Hacking argues, the nominalism that Foucault introduces into our understanding of the sciences concerned with describing human beings has a much greater scope than the 'revolutionary nominalism' of Kuhn, which he endorses. In effect Hacking does not see that there may be more to practices in contexts that are not physics, practices that also provide support for realism in contexts.

In 1982, Foucault wrote:

[361] Gutting (1989) *op cit.* 29.

[362] Hacking (1985) *op cit.* 246.

> I would like to say, first of all, what has been the goal of my work during the last twenty years. It has not been to analyze the phenomena of power, nor to elaborate the foundations of such an analysis.
>
> My objective, instead, has been to create a history of the different modes by which, in our culture, human beings have made subjects ... Thus, it is not power but the subject which is the general theme of my research.[363]

The contrast between Kuhn and Foucault rests for Hacking on the fact that he thinks, 'a strict and universal nominalism is a preposterous mystery.'[364] Nominalism about the ordinary natural kinds of experience, 'about grass, trees and stars' is a real problem. People, on the whole, present no such problem.[365] He suggests that this gives us a second kind of nominalism, 'dynamic nominalism.' 'Categories of people come into existence at the same time as kinds of people come into being to fit those categories, and there is a two-way interaction between these processes.'[366] He gives examples from his own studies of early nineteenth century statistical measurement where,

> [c]onstantly new ways of counting people were devised. New slots were created into which people could fall and be counted. Even the decennial censuses in the different states amazingly show that the categories into which people fall change every ten years. This is partly because social change generates new categories of people, but I think the countings were not mere reportings. They were part of an elaborate, well-meaning, indeed innocent creating of new kinds of ways for people to be, and people innocently "chose" to fall into these new categories.[367]

[363] Foucault, M. 'The Subject and Power', *Critical Inquiry*, 8, no.1 (Summer 1982), 777, 778.

[364] Hacking (1993) *op cit.* 247.

[365] Hacking does say that, '[p]eople are alive or dead, tall or short, strong or weak, creative or plodding, foolish or intelligent. These categories arise from the nature of people themselves ... ,' *ibid.* 247, but he offers no account of why *these* categories (particularly intelligence, for example) should not be inventions of kinds themselves.

[366] *ibid.*

[367] *ibid.* 248.

It is with these constructions that Hacking's Foucault seems solely concerned, around which the human sciences are clustered. 'Like Kuhn's revolutionary nominalism, Foucault's dynamic nominalism is an historicized nominalism.'[368] But the difference really does *make* a difference. Thus, Hacking gives the warning:

> I think we shall lose ourselves in confusion and obscurity for some time yet, in the so-called social and human sciences, because in those domains the distinction between word and thing is constantly blurred. It is precisely experimental methods that I take to be essential to the physical sciences and which, I claim, make Kuhn's historicized revolutionary nominalism fall short of a strict nominalism. The experimental methods of the human sciences are something else.[369]

He does not say what the experimental methods of the human sciences are. Note, once again the lacuna of biology.

To digress slightly, once more, in 'Michel Foucault's Immature Science'[370] Hacking does comment that, '[w]hen we turn from a belief in revolutions to an attempt to analyze their structure there is little agreement between Kuhn and Foucault, but possibly this is because Kuhn is less concerned with immature science.'[371] However, the distinction between mature and immature science does not re-occur in Hacking's writing about Kuhn and Foucault, which is a pity because it might have better served Hacking as a means of untangling his own position on kinds and the construction of kinds.

To repeat the point, there is a potential gap in all this. It concerns what we are to make of the biological sciences. In order to show why this hole is damaging for Hacking's attempt to drive a wedge between the social and the natural we must firstly look at whether the de-

368 *ibid.* 248.

369 *ibid.* 249.

370 Hacking, I. (1979) 'Michel Foucault's Immature Science' *Noûs* Volume XIII Number 1 39-52.

371 *ibid.* 45.

scription of Foucault's work given by Hacking is a sustainable interpretation. Then we need to look at how biology is employed by Foucault, and examine whether, and to what extent the categories and taxonomies used are mere creations. In doing this I could be accused of confusing medicine and biology, for it is surely medicine which has the most relevant impact on the human sciences. I hope that the following will suggest why the social aspects of the biological sciences, that is, accounts of physiology, pathological anatomy, genetic diversity and determinism, are not simple medical issues. The final section on a recent study of the history of sex hormones should make this clear enough.

iii Foucault, history and philosophy

1 Foucault, Bachelard and Canguilhem

There is a tendency to downplay Foucault's work as a philosopher and historian of science—his other more socio-political theses tend to dominate current interpretations of his writings. Whether 'Man' is dead or not[372] is no longer a live issue understanding Foucault's work.[373] As Gutting's admirably clear (and, in places, controversial) survey of his earlier published material shows,[374] Foucault did have an evolving general picture of science. In this aspect of his work the debt to both Bachelard and Georges Canguilhem is clear.[375] From Bachelard Foucault took the notion that the disclosure of practices in any

[372] One of the central theses in *The Order of Things* being that it is only through the possibility of separating the notions of the object and subject of knowledge that 'Man' has become something that can be studied at all, allowing Foucault to talk about the 'death of Man' with the dissolution of this divide with the end of the modern *episteme*.

[373] Foucault explicitly rejected this thesis later in his expansion of his archaeological method of the limits of language into examination of the practices, techniques, institutions that are characteristic of his genealogy, as we shall see.

[374] Gutting (1989) *op. cit.*

[375] Georges Canguilhem succeeded Bachelard at the Institute d'Histoire des Science et des Techniques, University of Paris in 1955 where Bachelard had been professor from 1940. Foucault places them both within a tradition in recent French thinking of a 'philosophy of knowledge, of

particular area of human enquiry can only be 'regional'; he also made great use of the breaks that occur in each of these regions of rationality. However. Bachelard's focus is on physics—he is concerned to show how the revolutions in physics at the beginning of the century should be incorporated into our philosophical perspectives of science; Tiles describes his position in the following way:

> ... Bachelard sees his task. as a philosopher of science. as being to give a philosophical characterisation of contemporary. twentieth-century scientific thought and of the difference between the philosophy appropriate to the science which is developing in the wake of relativity theory and quantum mechanics.[376]

Georges Canguilhem. concerned primarily with the articulation of the history of biology and medicine. carried forward many of Bachelard's key concepts including those of epistemological breaks and obstacles.[377] but these become less a case of all-or-nothing. Epistemological obstacles are not of necessity negative in Canguilhem's epistemology of science. Gutting suggests that the difference lies in Canguilhem's starting point with history rather than philosophy. and his interest in the biological sciences and with their applications to people. We do not need to trace the details of the precise differences between Bachelard and Canguilhem here. but for our current purposes it is also worth noting that it is in Canguilhem that we find another key idea for Foucault. the discussion of the opposition of the normal to the deviant. the healthy to the pathological. He argues that although there is a sense in which the environmental options for a diseased organism are fewer than for a healthy one. this can be expressed as a normative understanding of these terms. The

rationality. and of the concept' – Foucault. M (1985) 'La vie: l'expérience et la science.' *Revue de métaphysique et de morale* 70. 4. trans. Gutting. G.

[376] Tiles *op. cit.* 10.

[377] An epistemological obstacle is any anachronistic concept that is used in a field of science. That is. it is an idea that prevents further development of that science. or the full adoption of a new theory. by an 'unconscious' reference back to an older theoretical framework.

healthy are not only best fitted to the environments relative to which they are found to be healthy, but are also in a position to decide what should constitute health in other situations too. That is,

> [b]eing healthy means being not only normal in a given situation but also normative in this and other eventual situations. What characterizes health is the possibility of transcending the norm, which defines the momentary normal, the possibility of tolerating infractions of the habitual norm and instituting new norms in new situations.[378]

The way in which Foucault extends and develops this idea is easy to see. For a long time it seemed to be the question of how objective analysis of what we know about ourselves is possible that occupied Foucault. In his early and middle period works it looks as though the possibility of 'transcending the norm' is lost. That is, he talks as though the processes through which we come to know about people in the regions (the discourses) he examines are interminable power games, where people are subjected to norms and classifications for control, without the possibility of ever adopting a rational perspective from which these norms can be justified. This is what Hacking understands as 'dynamic nominalism.' Of course, if this were correct, one would have serious doubts about how *this* insight about the structuring of the human world (and hence the world for all good structuralists) through the interplay of language, power and institutions could ever itself be justified, or given a status above these games. But in Foucault's later work we see that this is to assume too much about the structuralist components of Foucault's ideas.

[378] Canguilhem, G. (1978) *On the Normal and Pathological* Dordrecht: Reidel, 115, quoted in Gutting (1989), 47.

2 *Aufklärung*[379]

'What is Enlightenment,'[380] an essay never published in French, contains many revelations about what philosophy was for Foucault and how he had come to see his whole life's work by 1984, the year of his death. In discussing Kant's own account of the Enlightenment, he presents a picture of philosophy as a means of liberation through an uncovering and critique of illegitimate uses of reason, although this may not always be in the systematic theoretical way that is recognisable in contemporary analytic philosophy. When confronted with the uses of reason to dominate and control:

> It is precisely at this moment that the critique is necessary, since its role is that of defining the conditions under which the use of reason is legitimate in order to determine what can be known, what must be done, and what may be hoped. Illegitimate uses of reason are what give rise to dogmatism and heteronomy, along with illusion; ... it is when the legitimate use of reason has been clearly defined in its principles that its autonomy can be assured.[381]

This is consistent with the Kantian project of finding the limits of reason through the application of reason itself. But Foucault abandons the hope of securing any a priori or necessary limits.

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[379] 'Enlightenment' The interpretation I present of Foucault's philosophy owes much to Gutting (esp. (1989) Introduction and Chapter 7, 'Reason and Philosophy') and Davidson, A. I. (1994) 'Ethics as Aesthetics: Foucault, the history of ethics, and ancient thought' in Gutting, G. (ed.) *The Cambridge Companion to Foucault* Cambridge: Cambridge University Press.

[380] Foucault, M. 'What is Enlightenment?' in Rabinow, P. (ed.)(1986)*The Foucault Reader* Harmondsworth: Penguin Books.

[381] *ibid.* 38.

> Criticism indeed consists of analyzing and reflecting upon limits. But if the Kantian question was that of knowing what limits knowledge has to renounce transgressing, it seems to me that the crucial question today has been turned back into a positive one: in what is given to us as universal, necessary, obligatory, what place is occupied by whatever is singular, contingent, and the product of arbitrary constraints? The point, in brief, is to transform the critique conducted in the form of necessary limitation into a practical critique that takes the form of a possible transgression.[382]

So that ultimately,

> The critical ontology of ourselves has to be conceived as an attitude, an ethos, a philosophical life in which critique of what we are is at one and the same time the historical analysis of the limits that are imposed on us and an experiment with the possibility of going beyond them.[383]

Often it is assumed that Foucault's work contains within it the self-refuting relativism that threatens all forms of structuralism. It does not. He saw philosophy as 'the endeavour to know how and to what extent it might be possible to think differently.'[384] The projects he undertook were 'regional' studies—following the regionalism of Bachelard—with a view to exposing the illegitimate limitations 'imposed on us' in each of these areas, the human sciences as such, psychiatry, clinical medicine, judicial imprisonment and punishment, and so on.

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[382] *ibid.* 45.

[383] *ibid.* 50.

[384] Foucault, M. (1985) *The Uses of Pleasure—The History of Sexuality Volume 2* Harmondsworth: Penguin Books, 9 [trans. by Hurley, R. of (1984) *Histoire de la sexualité, II: l'usage des plaisirs* Paris: Gallimard].

> … Foucault's focus is always on the domains of "dubious disciplines" dealing with human beings. There is no suggestion that he thinks his archaeological method could be applied to sciences like physics or chemistry to show that their claims to truth and objectivity are questionable. There is, in fact, strong reason to think that Foucault on the whole accepted the objectivist view of these disciplines held by Bachelard and Canguilhem.[385]

Indeed it has been argued by Rudi Visker in a recent book, *Michel Foucault: Genealogy as Critique*[386], that one of Foucault's concerns is about a rather familiar problem, that of demarcation, how we assign 'scientificity' to fields of enquiry. Thus Foucault says:

> It is surely the following kinds of question that would need to be posed: What types of knowledge do you want to disqualify in the very instant of your demand: 'Is it science'? Which speaking, discoursing subjects—which subjects of experience and knowledge—do you then want to 'diminish' when you say: 'I who conduct this discourse am conducting a scientific discourse, and I am a scientist'?[387]

Clearly this has a different edge to Popper's concerns with demarcating science from non-science, but it contains within it the suggestion that there are some domains that do qualify as science, though there must be care in how we see power in the accolade 'science.'[388]

We are in a position to state a little more clearly what Hacking's 'dynamic nominalism' does and does not involve, and whether it can be taken as a good interpretation of Foucault's general program. In Hacking's reading of Foucault, dynamic nominalism is a perspective on how people are constructed by the network of relations they have to other people, bodies of knowledge, linguistic practices, institutions and society as a whole. The cate-

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[385] Gutting (1989) *op cit.* 273.

[386] Visker, R (1995) *Michel Foucault, Genealogy as Critique* (trans. Turner, C.) London: Verso.

[387] Foucault, M (1980) 'Two Lectures' *Power/Knowledge* London: Harvester Press, 85.

[388] Of course, in a more analytic setting it is Mary Midgley who has most consistently questioned an unchecked scientism in our culture, our practices and institutions. See, for example, Midgley, M. (1989) *Wisdom, Information and Wonder: What is Knowledge For?* London: Routledge.

gories that are so constructed have no 'real' basis in the world and would *seem* to be as plastic as you like. This is as far as Hacking goes. What he fails to take on board is that Foucault's analyses do not involve a denial of the possibility of having any objective knowledge. It is entirely compatible with Foucault's philosophical project that we could get the human sciences 'right' in the future, although we would always be on the look out for illegitimate use of reason in such a future. The emergence of biology from natural history as a shift in underlying epistemes does not deny biology's current status as an activity that tells us from a rational standpoint what the world is like.

3 Foucault's use of biology

So how does Foucault use biology? Roughly speaking, the chronological order of Foucault's interests, the institutions that exercised him during his life, are psychiatry, insanity and mental illness, clinical medicine, the human sciences, punishment and prisons, sexuality and the creation and discipline of the subjective self. Of course, he addresses many other issues, but these are the concerns of his major works. What I wish to show now, by a brief survey of some of these, is that it is the application of knowledge about the biological world that is so important to how biology became involved in the power/knowledge relations he examines.

I have already mentioned that for Foucault, biology has not always existed as identified field of enquiry. In both *The Order of Things* and *The Archaeology of Knowledge*[389] he gives an analysis of the construction of discursive practices that bring together all sorts of separate questions and information enabling a science such as biology to emerge. It is clear that Foucault believes that it is only when there is a concept of function incorporated into our world picture that biology can come into being in its present form. *The Order of*

[389] Foucault, M. (1972) *The Archaeology of Knowledge* London: Routledge [trans. by Sheridan Smith, A. of (1969) *L'achéologie du savoir* Paris: Gallimard].

Things contains an account of the birth of biology from the transformation of natural history. For Foucault natural history is not the study of 'life', there is no such unifying property in the Classical *episteme*. From the mid-seventeenth century until the beginning of the modern *episteme* in the late eighteenth and early nineteenth century natural history was a procedure of classification. Putting things in order and specifying their place in great tables of being does not necessarily require that the life is picked out as an object of study in itself. The development of analysis as a conceptual tool points, for Foucault, to the historical observation in the Classical age,

> ... *analysis* was quickly to acquire the value of a universal method; and the Leibnizian project of establishing a mathematics of quantitative orders is situated at the very heart of Classical thought; its gravitational centre. But, ... this relation to mathesis as a general science of order does not signify that knowledge is absorbed into mathematics, or that the latter becomes the foundation for all possible knowledge; on the contrary, in correlation with the quest for a mathesis, we perceive the appearance of a certain number of empirical fields now being formed and defined for the very first time. In none of these fields, or almost none, is it possible to find any trace of mechanism or mathematicization; and yet they all rely for their foundation upon a possible science of order.[390]

So, during the Classical *episteme*, there can be no biology as such. This requires the modern concern with function as the primary point of view in studying living things. With the introduction of the over-arching idea of function, finding the similarities of structure and mapping increasing complexity in forms of organisms onto a continuum no longer looks good enough. Gutting comments,

[390] Foucault (1970) *op cit*. 57.

> ... the property of *life* is no longer just one category of natural classification. Rather, all classifications express subdivisions of life (defined in terms of functional system); to define a thing's species is to specify the precise sort of functional system that it is. As a result, life becomes the category that defines the objects of biological enquiry as such, and modern biology becomes, in contrast to Classical natural history, the *science of life*.[391]

There have been many criticisms levelled at the historical accuracy of Foucault's factual support for his argument in *The Order of Things*. The difficulties with the details need not detract too much from the point being made, which is repeated in a slightly modified form in *The Archaeology of Knowledge*. Here Foucault lays out what it is that the archaeological method he has been employing up to this point is supposed to be. He explicitly states the purpose of archaeology to be the uncovering of the discursive formations[392] that make up our field of knowledge. On first reading this would suggest that biology is going to have to be treated simply as the constructed amalgam of many different discursive techniques. This, however, is not what Foucault argues in *Archaeology*. Uncovering discursive formations is not the same as the analysis of the philosophy of a science. Foucault draws a distinction between *connaissance* and *savoir*. *Connaissance* is an specific body of knowledge, which could be any modern science. *Savoir* is the discursive formations that make possible any particular *connaissance*:

> In Foucault's view, a particular science ... is the locus of *connaissance* whereas a discursive formation is the locus of *savoir*. As such, the *savoir* of a discursive formation provides the objects, types of cognitive authority ... , concepts, and themes (theoretical categories) that are necessary for a body of scientific *connaissance*.[393]

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[391] Gutting (1989) *op cit*. 191.

[392] '... Foucault regards a discursive formation as involving four basic elements: the *objects* its statement are about, the kinds of cognitive status and authority they have ... , the *concepts* in terms of which they are formulated, and the *themes* (theoretical viewpoints) they develop.' Gutting (1989) *op. cit.* 232.

[393] *ibid.* 251.

This can be used to strengthen the claim made earlier: Foucault does not question the objectivity of the natural sciences. The *connaissance/savoir* distinction does not lead us to conclude that, although particular aspects of the discursive formations are traceable through the history of a science such as biology, they are all that there is to such sciences—discursive formations are linguistic practices only and the natural sciences consist of more than that. It is possible that an attack on the 'legend'[394] notion of rationality could be constructed along these lines, but Foucault does not do this. Clearly then it is legitimate to question the status of the norms of practice, inference and rationality that underpin this unity—as recent work on the philosophy of experimentation does, for example. *This* sort of analysis of 'good' and 'bad' science is important whether one is a pluralist or a unificationist about methodology. However, it does not necessarily follow that rationality is excluded from *savoir*. That is, it does not follow that rationality is always excluded from the conditions that make scientific knowledge possible. Indeed, the reason why Foucault's studies are interesting is because he shows how the *savoir* of all sorts of human practices *can* be thoroughly *irrational*. A deeper relativism would imply quietism about these issues—something which Foucault quite obviously opposed in his active political life. Consequently, although (according to Foucault) biology comes into being in the nineteenth century with Cuvier's discussions of organ functions, there is little to suggest that biology should be treated as constructed knowledge, in the sense that it lacks objectivity and rationality. I concede, of course, this is only a possible interpretation of what Foucault has to say about science—indeed it could be argued that there is evidence in his writing that when taken alone would discredit it. As Robert A. Nye has noted, in reading what Foucault says about science:

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[394] See the discussion of Kitcher's *The Advancement of Science*, above, p. 53 ff.

> ... biology, it would appear, is a special case, falling somewhere between these two discursive domains [physical science and social science], and it is by no means clear that Foucault decided where it belonged. With his teacher George Canguilhem, he appears to recognize the distinction between the sciences of life and other sciences, but, also like Canguilhem, he contrasts self-regulating organic systems with human societies that think about themselves with the help of biological models. Yet elsewhere he assimilates biology to the human sciences with their notorious epistemological sensitivity to the discontinuities and upheavals of political and social history[395].

So having said all this, it is knowledge of the living world, through its many applications, that time and again Foucault reveals as essential to the hidden power play behind our social institutions, and that seems vulnerable to the same critiques that Foucault applies to the institutions that they support. So how *does* Foucault use biology?

In what follows I shall use the word 'biology' to refer both to biology in its modern sense and the natural history of the eighteenth century. The sharp split that Foucault sees in *Order* is unworkable in practice in any case: the date for the birth of biology is much harder to place. Be this as it may, time and time again in the earlier works it is the general transformation of discursive practices from the Classical tabulation of all beings and forms to the modern era's concern with discovering hidden functional and historical relations between things that is the background to the discussions.

4 Mad, bad and dangerous to know

In his earliest writings Foucault's notion of an 'archaeology' for each of his studies is still vague and ill-defined. His earliest accounts of mental illness attempted an analysis of mental disorders that broke with the traditional picture of their being similarly structured to organic disorders, the 'psychic analogue of disease'[396] and instead revealed them to be

[395] Nye, Robert A. (1994) 'Love and Reproductive Biology in *Fin-de-Siècle* France: a Foucauldian Lacuna?' in *Foucault and the Writing of History* Goldstein, J. (ed.)(1994) Oxford: Blackwell, 151-152.

[396] *ibid.* 67.

products of any bourgeois society[397], however that society might be structured. By the time he wrote *Madness and Civilisation*[398] the Marxist elements in his work had been dropped in favour of an exploration of the mechanisms that those who failed to share the values of their society were controlled and regulated at different times—thereby suggesting a constructivist, culturally relative reading of mental illness. This view of the mad involved extensive analysis of those who tried to say exactly what was wrong with these people. Foucault traces the treatment of insanity from the Classical to the beginning of our own modern age—roughly, from the sixteenth century and the dominant popular image of 'the Ship of Fools', through the incarceration of mad people ('the Great Confinement' in the seventeenth century), through attempts to normalise and make them socially acceptable in the eighteenth century, to nineteenth century conceptions of mental disease and the beginnings of psychology and psychiatry proper, particularly as instantiated in the work of Freud.[399]

The standard history suggests that this progress is a movement towards increasingly humane treatment of insanity brought about by an increased understanding of what insanity is. Foucault questions whether this is an accurate history. He wishes us to take from his study the notion that madness has become increasingly regulated during this period; that the authority of those who diagnose madness, especially doctors, has been strengthened tremendously; and that the medicalisation of madness presents a solidification and institutionalisation of the means of control for undesirable elements in society. Now clearly this

[397] See Foucault, M. (1976) *Mental Illness and Psychology* New York: Harper & Row [trans. by Sheridan A. of (1954) *Maladie mentale et personnalité* Paris: PUF.]

[398] Foucault, M. (1971) *Madness and Civilisation: a History of Insanity in the Age of Reason* London: Routledge [trans. (and abridged) by Howard, R. of (1961) *Folie et déraison: Histoire de la folie à l'âge classique* Paris: Plon.]

[399] The subtitle to *Madness and Civilisation* is *A History of Insanity in the Age of Reason* because the majority of his analysis is of madness in the eighteenth century.

picture is one that contains the simple idea that all power represses. As Hacking has said it seems that,

> [a]n exclusion is an exercise of power. It is a putting away. Despite all the fireworks, *Madness and Civilisation* follows the romantic convention that sees the exercise of power as repression, which is wicked.[400]

The wicked physicians of *Madness and Civilisation* are seen to be using developing knowledge about physiology (amongst other things) to exclude and repress. Thus rudimentary medical, physiological and moral techniques are combined via notions of the metaphysical nature of soul and body—during the Classical age they are treated together. Admittedly it was only later, in the nineteenth century, that 'the doctor' began to play a significant role in the *treatment* of madness, but there is enough evidence to show that the construction of apparently objective mental disorders and the general understanding of mad people trades on the objectivity of other kinds of knowledge, but is motivated by political and economic concerns.

The position of medicine in Foucault's account of the history of madness is not examined in itself, and I have perhaps suggested that it is to be seen to constitute objective knowledge in itself, but this need not be so. For Foucault, the development of medicine is not a simple progression, a gathering of knowledge. Indeed, it is only in looking at medicine that the application of biology to human life becomes apparent. So we turn to *The Birth of the Clinic*.[401] Here we find Foucault begins to refine his 'archaeology'. Medicine is clearly de-

[400] Hacking, I. (1981b) 'The Archaeology of Foucault' *The New York Review of Books*, in Hoy, D. C. (ed.)(1986) *Foucault, a Critical Reader* Oxford: Blackwell, 30.

[401] Foucault, M. (1976) as *The Birth of the Clinic: an Archaeology of Medical Perception* London: Routledge [trans. by Sheridan, A. of (1963) *Naissance de la clinique: une archéologie du regard médical* Paris: PUF.]

fined as a vital part of the assumptions of contemporary human science. Right at the end, in the 'Conclusion' he remarks:

> It is understandable ... that medicine should have had such importance in the constitution of the sciences of man—an importance that is not only methodological, but ontological, in that it concerns man's being as object of positive knowledge.[402]

However, the central issue is how the notion of disease has changed and developed. Disease, Foucault argues, can be seen to alter as part of a shift in a 'spatial' reading of the kinds of things there are in the world. 'Classical medicine ... conceived of diseases as abstract essences.'[403] That is, the Classical notion required that physicians make a valiant effort to subtract the actual patient from their considerations of the disease in order to see the essential nature of the particular disease present—old age, environmental conditions, education and so on would obscure the disease's true expression. Using the analogy of spatial mapping Foucault puts it this way:

> The exact superposition of the 'body' of the disease and the body of the sick man is no more than a historical, temporary datum. Their encounter is self-evident only for us, or, rather we are only just beginning to detach ourselves from it. The space of *configuration* of the disease and the space of *localization* of the illness in the body have been superimposed, in medical experience, for only a relatively short period of time—the period that coincides with nineteenth-century medicine and the privileges accorded to pathological anatomy.[404]

The details of Foucault's analysis are not vital to my case. What is important to note is Foucault's ambivalence towards the status of the knowledge that informed the change in the concept of disease. Whilst the Classical age had had some notion of the connection of diseases to environment and circumstance with debate about the nature of epidemics, it is

[402] *ibid.* 197.

[403] Gutting (1989) *op. cit.* 112.

[404] Foucault (1963) *op. cit.* 3-4.

only later with the use of pathological anatomy and physiology in medical contexts that the modern disease can emerge. The change is also expressible in terms of a shift from concerns with health to normality:

> Generally speaking, it might be said that up to the end of the eighteenth century medicine related much more to health than normality; it did not begin by analysing a 'regular' functioning organism and go on to seek where it had deviated, what it was disturbed by, and how it could be brought back into normal working order; it referred, rather, to qualities of vigour, suppleness, and fluidity, which were lost in illness and which it was the task of medicine to restore ...
>
> Nineteenth century medicine, on the other hand, was regulated more in accordance with normality than health; it formed its concepts and prescribed its interpretations in relation to a standard of functioning organic structure, and physiological knowledge—once marginal and purely theoretical knowledge for the doctors—was to become established (Claude Bernard bears witness to this) at the very centre of all medical reflexion.[405]

Physiology here provides knowledge as a background to nineteenth century medicine. And although Foucault regards the development of pathological anatomy as 'late,' being held up by a lack of medical contexts for its application,[406] once it is launched by the work of Marie-François-Xavier Bichet there is reason to suppose that Foucault took that science to be well founded and objective:

> Hence the appearance that pathological anatomy assumed at the outset: that of an objective, real, and at last unquestionable foundation for the description of diseases.[407]

Foucault thinks that the change was more than a difference in words and their use—it was not just a surface jostling of already formed concepts,

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405 *ibid.* 35.

406 *ibid.* 126.

407 *ibid.* 129.

> ... it was the result of a recasting at the level of epistemic knowledge (*savoir*) itself, and not at the level of accumulated, refined, deepened, adjusted knowledge (*connaissance*).[408]

The knowledge that underpins modern medicine is of a different sort than that of the eighteenth century. There is no denial by Foucault that that knowledge cannot be regarded as objective.

> Like Bachelard, who emphasised the controlling role of reason in the experiments of physics and chemistry without denying the objectivity of these disciplines, Foucault does not present the interpretative grid of modern medicine as undercutting its scientific status. Nor does he think that the value-ladenness and ideological content of medicine exclude its objectivity.[409]

So not only are the sciences that ground modern medicine taken to provide objective knowledge, a well monitored and carefully controlled practice in medicine could too. Foucault's 'analysis is a splendid instance of laying bare the a priori presuppositions involved in reports of allegedly uninterpreted data ...'[410] There is, by the way, no assumption that medicine is a science in itself, as we shall see.

The Archaeology of Knowledge was Foucault's next major study. In it he attempted to pull together the general method of his 'regional' studies up to that date. It is here that we find the distinction between *savoir* and *connaissance* more clearly laid out than in *The Birth of the Clinic*, alongside the general notion that there is a wide ranging break between the sciences and knowledge of the Classical age up to the end of the eighteenth century and the modern period. Here he explicitly introduces the idea that we are now moving beyond the human subject as the focus for study to the study of 'discursive practices'. Since I have

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[408] *ibid.* 137.

[409] Gutting (1989) *op cit.* 137.

[410] *ibid.* 136.

already described these aspects of Foucault's above. I shall not dwell on *The Archaeology of Knowledge* here. But we do find further confirmation of Foucault's acceptance of an objective reading of the biological foundations of modern medicine:

> Clinical medicine is certainly not a science. Not only because it does not comply with the formal criteria, or attain a level of rigour expected of physics, chemistry, or even of physiology; but also because it involves a scarcely organized mass of empirical observations, uncontrolled experiments and results, therapeutic prescriptions, and institutional regulations. And yet this non-science is not exclusive of science: in the course of the nineteenth century, it established definite relations between such perfectly constituted sciences as physiology, chemistry or microbiology; moreover, it gave rise to such discourses as that of morbid anatomy, which it would be presumptuous no doubt to call a false science.[411]

Of course Foucault does not tell us what he means by the 'formal criteria' for science, nor what it takes for one to be 'perfectly constituted'. This is how it should be for Foucault's project of illustrating the hidden difficulties that arise when we fail to familiarise ourselves with the hidden in our investigations—the hidden that is contingent and different in each discipline. If entirely *formal criteria* could be extracted for the identification of 'science', the additional parts of our enquiries—the power relations, the abuses and misuses—would be apparent. But we do not see this in actual practice. The conceptual archaeologist continually brings us up against our past 'discursive practices', showing us how contingent they are, challenging us to reform our current attitudes and epistemic commitments. Yet, as I have already stressed, this does not lead us to an abandonment of reform for a purpose—we suppose that we can speak about the world and we want to know how we might do that better. We want to improve on our current commitments. Science and ideology may have similar historical roots but they can be distinguished in order to tell the good knowledge and practices from the bad.

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[411] Foucault, M. (1972) *op. cit.* 181.

The status of clinical medicine as a 'non-science' is interesting in that it gives us some clues as to what we should take to be included in a list of accepted and acceptable sciences. Biological knowledge is definitely included, despite its relationship to questionable disciplines. At the end of his paper on nineteenth century reproductive medicine in France Nye asks:

> Did Michel Foucault miss a chance to extend his analysis of *fin-de-siècle* discourses on sexual knowledge and power further into the domain of biology? Did he eschew consideration of reproductive biology because of some disciplinary line be believed divided biological science from medical science? I hope I have provided here an answer to the first of these questions; the second I leave to the epistemologists.[412]

Nye's response to the first question is that Foucault did indeed fail to follow through a potential analysis. He shows how some aspects of the supposed objective knowledge that constituted reproductive biology were the product of a constructive, controlling process to maintain gendered social positions and promote racial purity in France.

Nye's second question can be answered too. Foucault did not see a boundary dividing biology and medicine *per se*. Biological knowledge can be made for controlling purposes and medicine has the potential to be well founded and articulated. I suggest that Foucault did not look at particular cases of biological abuse quite simply because against the background of his contemporary concerns they were not as pressing as pointing to the regulation of people's lives through conceptually murky practices in psychiatry, medicine and judicial punishment.[413] Had he lived just ten more years I suspect the situation would have been different. Having said this, we can quite clearly see that in many parts of biology

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[412] Nye (1994) *op. cit.* 164.

[413] The area where he might have most followed through such a study, in the construction of modern notions of human sexuality, was left largely unexplored as the project to map this field was transformed into a theory of the self and the articulation of an ethic for the self in Greco-Roman and early Christian philosophy. Hence, my survey does not include the second and third volumes of the *History of Sexuality*.

there are rational, well-controlled experiments, whereas medicine on the whole does not use them. There is not so much a line or boundary as a spectrum of increasing rational engagement. As Gutting also comments:

> ... clinical medicine is not an experimental science. The latter involves putting questions to nature whereas the former is merely a matter of listening to what nature has to say. This, [Foucault] says does not mean that clinical medicine is antiexperimental. Its observations will naturally lead to experiment, but the questions posed will be expressed in the language of observation—that is, in the language spoken by nature to the clinical gaze.[414]

Nowhere does Foucault exclude the possibility that the epistemological virtue of experimenting could be matched in medicine or other human investigations by other rationally appropriate methods. In *Discipline and Punish*[415], as the archaeological method is augmented to become a genealogy that includes concern for non-discursive practices too, we see a greater sensitivity to the interplay between objective and questionable realms of knowledge:

> The classical age discovered the body as object and target of power. It is easy to find signs of the attention paid to the body—to the body that is manipulated, shaped, trained, which obeys, responds, becomes skilful and increases its forces. The great book of Man-the-Machine was written simultaneously on two registers: the anatomico-metaphysical register, of which Descartes wrote the first pages and which the physicians and philosophers continued, and the technico-political register, which was constituted by a whole set of regulations and by empirical and calculated methods relating to the army, the school and the hospital, for controlling or correcting the operation of the body.[416]

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[414] Gutting (1989) *op. cit.* 124.

[415] Foucault, M. (1979) *Discipline and Punish: The Birth of the Prison* Harmondsworth: Penguin Books [trans. Sheridan, A. from (1975) *Surveiller et punir: Naissance de la prison* Paris: Gallimard].

[416] *ibid.* 136.

Discipline and Punish is the history of the control of the body in and through institutions. It focuses on judicial punishment, but also has much to say about schools, the army and hospitals. Here biological knowledge informs and is itself transformed. The genealogical method, that runs through Foucault's work from the mid-seventies through to the end of the decade, also informs the beginning of his work on the history of sexuality.[417] This project, starting out as a regional articulation of nineteenth century construction of sexual identities from behaviour, is similarly sensitive to the entanglement of varied contexts and criteria for the real. Once again it is biology (and his continued interest topic of 'health care') applied in a medical context that informs his discussion, especially in discussion of homosexuality:

> We must not forget the that psychological, psychiatric, medical category of homosexuality was constituted from the moment it was characterized ... less by a type of sexual relations than by a certain quality of sexual sensibility, a certain way of inverting the masculine and feminine in oneself. Homosexuality appeared as one of the forms of sexuality when it was transposed from the practice of sodomy onto a kind of interior androgyny, a hermaphrodism of the soul. The sodomite had been a temporary aberration; the homosexual was now a species.[418]

Given my earlier discussion of recent research in human sexuality and partner preference, this is most interesting.[419] There I suggested that manipulation of models was an obvious feature of the experimental situation, and that such research, whilst not directly manipulating people, does involve the manipulation of kinds (types) in models, and is, in that regard, experimental. What we see here is that even the rational use and manipulation of entities and classes in models does not *on its own guarantee* that the kinds involved are real (outside of the model). But it does not undermine the use of experiments to discover the

[417] Foucault, M. (1981) *The History of Sexuality: Volume I, An Introduction* Harmondsworth: Penguin Books [trans. by Hurley, R. of (1976) *La Volonté de savoir* Paris: Gallimard].

[418] *ibid.* 43.

[419] See p. 85 ff.

real. In fact, it is precisely because experiments on kinds can reveal the real that they are used in contexts where further investigation is required to ensure the appropriateness of the model used. The consequences of this will become apparent below.

In one sense, for Foucault, there *are* differences to be drawn between the human and natural sciences, between the dubious and the acceptable, but they do not, and could not consist in specific criteria that would apply across the board. Rather the differences can be seen in the attitude taken to the history of each area. Whilst parts of biology have at times been, and still are called 'objective' when such a label would be inappropriate, on the whole biology is secure as an objective science because it is not surprised by its own parents, and neither should it be:

> Why should an archaeology of psychiatry function as an 'anti-psychiatry', when an archaeology of biology does not function as anti-biology? Is it because of the partial nature of the analysis? Or is it rather that psychiatry is not on good terms with its own history, the result of a certain inability on the part of psychiatry, given what it is, to accept its own history?[420]

If psychiatry sets itself up with certificates copied from the biological laboratory walls it should not be disappointed by an unfavourable response from clients and observers when the fraud is uncovered. Biology's own credentials are, on the whole, all earned.

5 Reading Foucault

So what does all this show? I think it is fairly clear that we can interpret Foucault in the following way. His ambivalence with regard to the status of biology is not the result of failing to follow through with his analysis of powerful scientific practices, neither is it a product of his possessing irreconcilably different attitudes towards human and natural sciences. Rather, precisely because he saw that only regional criticism of knowledge is possi-

[420] Foucault, M. (1980) 'The History of Sexuality' in *Power/Knowledge, op. cit.*, 192.

ble in any fruitful enquiry uncovering the roots of contemporary thinking, he could quite coherently hold that objective knowledge is attainable from any rational enquiry into the nature of parts of the world, whilst constructing severe and often crippling critiques of what is now taken be obvious and given about ourselves and our world. Biology, having had fingers in many pies, is sometimes to be criticised, sometimes to be praised. Nothing leads us to accept Hacking's labelling of Foucault as a 'dynamic nominalist', because it does not follow from this interpretation that we *cannot* discover anything *real* about ourselves, only that, in the regions of knowledge that Foucault examines, there are serious reasons for us rejecting the objectivity of knowledge claims and re-examining how we might better describe things. How else are we to understand appeals to 'liberation'?

Rejection of Hacking's reading of Foucault has a knock on effect for the rest of Hacking's philosophy, particularly with regard to experimentation and kinds. I shall now return to his experimental realism and present a case against the position he presents and provide a better use for the epistemological virtues[421] provided by experiments.

iv Scientific realism and experimental realism

Let us recap. Hacking states that there is a nominalist element to the taxonomies we use to give meaning to the singular objects in the natural and the human domains. He calls the

[421] In a number of places I have made reference to 'epistemological virtues', or the like. Dupré speaks of epistemological virtues (*Disorder* 10-11, 243). Working out a complete virtue epistemology is beyond the present study. My comments on Putnam and Rorty below, p. 210, should (indirectly) point to how I see such a project panning out. To date, the only book-length treatment of virtue epistemology I know is Zagzebski, L. T. (1996) *Virtues of the Mind* Cambridge: Cambridge University Press (although this is by no means a satisfactory book)—here the idea is traced to Ernest Sosa. One important aspect of Zagzebski's analysis is that she shows why simple forms of reliablism will not do as virtue epistemologies, which they are often taken to be.

nominalism he ascribes to natural science 'revolutionary nominalism' and he sees it at work in Kuhn's work. He calls the nominalism he ascribes to the human domain, to social science, 'dynamic nominalism' and says that the writings of Foucault admirably demonstrate what this form of nominalism means in practice. Hacking then goes on to claim that the difference between dynamic and revolutionary nominalism is to be found by observing that the world that natural science examines is not just constructed by human beings, even though the categories that describe that world may change, whereas the human world is entirely 'made up'.

Behind all this is Hacking's belief that scientific realism in the physical sciences is secured by the creation of physical phenomena that can be used for other purposes. 'If you can spray them then they are real,' whatever way you then choose to describe the theoretical make up of electrons. If phenomena can be manipulated and used then we should believe, *on these grounds alone*[422] that the entities manipulated really do exist. As we have seen, biology has a methodology (especially in terms of experimentation) that seems entirely compatible with the physical sciences. Biological sciences too create phenomena for the purposes of manipulation of other entities. But more than this, in biology we do see also the manipulation of kinds and their use—recall the experiments on plant traits.

Now clearly this could be presented as a problem with the status of the biological sciences. If we were to take Rosenberg's instrumental reading of biology as given, then *pace* all the similarities in practice, nothing that experimental biology reveals needs challenge us—only physics really gets at the world, and physics experiments show us what entities exist. Taking Rosenberg and Hacking together we end up with the very kind of nominalism that

[422] Otherwise Hacking has to give an account of how *theory* 'connects' with the world—precisely the project criticised and rejected in the *Representing* part of *R&I*.

Hacking ascribes to Foucault being applied to biology. That is to say, a construction of instrumentally valuable kinds that should not be taken to be real since they are based on limited epistemological abilities and motivated by interests of a socio-political nature. But we have already found problems with Rosenberg's epistemology.[423] And we have seen that an alternative reading of Foucault—one that is much more in accord with his own stated aims—paints a very different picture of how we should understand biology and its relationship to our struggle to understand ourselves. This different picture being one where awareness of the ongoing invasion by other interests of our 'pure' search for rationally justified knowledge of the biological world is accepted together with the possibility of there being correct answers to biological questions about the kinds of things biology describes.

To put the situation another way, how can we accept the important insights Foucault gives us into our cultural practices and maintain a consistent stance on scientific realism if we follow Hacking's own acceptance of Foucault's work?[424] So if we assume that we want to retain some objectivity for the natural sciences three possible solutions present themselves:

1. Deny biology the same objectivity status as physics. That is, accept Hacking's experimental realism, but exclude biology from this story of 'making things real' on the grounds that the appearance of similar methodology is an illusion since the nominalism that underpins biology is just like that he sees in Foucault's human sciences—dynamic and constructive.

[423] See p. 119 ff.

[424] See pp. v ff. and 172 ff. for my own reasons for responding positively to much of what Foucault says.

2. Discount the cultural impact of the peripheral uses of biology as irrelevant to the core of biology. That is, draw a clear line between knowledge statements and uses in human practice.

3. Reject the thesis that says that scientific realism can only be understood as experimental realism and reintroduce the possibility of there being epistemologically virtuous, rationally grounded means of enquiry that 'get the world right'—a stronger form of scientific realism. Reject also the idea that a similar methodology across different sciences *must* imply a single ontological structure to account for all things. That is, reject the distinction between dynamic and revolutionary nominalism in favour of increasingly numerous taxonomies corresponding to increases in the complexity of the systems studied.[425]

The reading of Foucault that I have presented above, especially with regard to biology, shows us exactly why options 1 and 2 can be ruled out immediately. Lines of demarcation should not be drawn on the grounds Hacking recommends because we end up with a weakened notion of good and bad analyses of human nature and the world. If, however, we accept that experimentation, as manipulation and testing,[426] is one amongst other techniques that we use to rationally explore the world, then we can augment our historical analyses of our current practices, whilst retaining an edge to our lookout for dubious and spurious knowledge claims. Of course, what questions we ask is also open to historical analysis. That is, Hacking's experimental realism hampers our attempts to assess *natural* science precisely because it prevents looking at situations where experiments have been used rationally to help determine the ontological status of entities and kinds that are nevertheless

[425] The real problem with this soultion is that the notion of 'complexity' is central, yet in itself it is notoriously vague.

[426] See p. 43 ff., above.

embedded in theory. And it hampers our attempts to assess *social* sciences because it prevents the possibility of ever understanding them as anything but the 'dynamic' creation of ways for us to be, and thereby removing a context for struggle and liberation—'better' implies more than 'different.'

This brings us right to the heart of my thesis. William McKinney[427] gives an interesting and clear critique of Hacking's claims that being able to manipulate and use entities is a guarantee that such entities are real. My own critique is deeper than this. I have given my reasons for why there can be no detailed account of experiments beyond a workable and effective epistemology of experiments that *includes* intervention and manipulation. I have pointed out that there can be no interpretation of the ontology that such an epistemology is supposed to support without a commitment to a general structure (hierarchy) of kinds of entities under investigation. Biology illustrates this perfectly, and at the same time shows how there always remains the possibility of our simply inventing kinds, especially when dealing with applications of biology to human beings. Hacking's epistemology of experiments rest on a distinction between sciences that trade in real kinds and those that do not. No such simple distinction exists.

To criticise Hacking in this way is not to detract from his skill in highlighting the need to examine the role of experimentation in science and life. The resistance of the world to our actions on it *is* a vital component in finding out about that world. What we cannot do is extract a single, simple epistemological framework for the application of that fact alone. Hacking's attempt to do so results in a distortion and over-simplification of complex relations of knowledge, practices and techniques for discovery and invention, which, whilst

[427] McKinney, W. J. (1991) 'Experimenting on and Experimenting with: Polywater and Experimental Realism' *The British Journal for the Philosophy of Science* Vol. 42, No. 3 September 1991, 295-307.

sharing aims and general strategies in rationality, can only be assessed regionally in the context of that rationality. The key then is the supposition that we could describe science as rational. How and why I think this is possible will be part of the conclusion below.

v Sex hormones—kinds and experiments applied

I shall now briefly illustrate the lessons of the previous section by looking at the application of a Foucauldian archaeological analysis to the history of research on sex hormones early this century. In the study I shall consider appeal is made to Hacking's experimental realism, and I shall show why it is inappropriate for precisely the reasons I have mentioned above.

1 Hormones and sex

In *Beyond the Natural Body: an Archaeology of Sex Hormones*[428] Nelly Oudshoorn examines the debates that took place in the biological community over the role and nature of human sex hormones in the 1920s and 30s, especially with regard to whether and how they function in determining gender. Starting with the assumption that our concepts for bodies are in a permanent flux, she argues that the discovery of a range of hormones in male and female bodies had a specific and profound effect on how our hormonal notion of body was constructed, and that the notions used further directed the science thus employed. She guides us to the conclusion that regional concerns about specific hormones in particular contexts are all to easily used to inform a picture about identity and sexual kinds:

[428] Oudshoorn, N. (1994) *Beyond the Natural Body: an Archaeology of Sex Hormones* Routledge: London.

> In ... biomedical discourses, the construction of the body as something with a sex has been a central theme all through the centuries. The myriad of ways in which scientists have understood sex provide many illuminating counter-moves to the argument that sex is an unequivocal, a historical attribute of the body that, once unveiled by science, is valid everywhere and within every context.[429]

I already noted Dupré's discussion of the general sex categories above.[430] Oudshoorn supplements the observation there that no essential features exist that pick out sex across the board—Oudshoorn shows how the concept of sex in *humans* is the product of asking particular questions of the world, particularly in the modern era. She describes her own book thus:

> *Beyond the Natural Body* illustrates how scientific body concepts such as the hormonal body assume the appearance of natural phenomena by virtue of the activities of scientists.[431]

So she believes that the blame for concepts being used *as if* they were natural is a result of scientists work and, as she discusses it, their relationship with industry. She also points out how prescientific notions of the body remain unexamined in the scientists' research—in much the same way as Bachelard's 'obstacles.' In places echoing Foucault of *The Birth of the Clinic* and *Discipline and Punish* Oudshoorn pinpoints various mechanisms that are employed in this construction:

> With the rise of modern science, bodies have thus become transformed into objects that can be manipulated with an ever growing number of tools and techniques.[432]

One of the major mechanisms involved, Oudshoorn claims, is the laboratory science, experimentation.

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[429] *ibid.* 6.

[430] See p. 120 ff.

[431] Oudshoorn (1994) *op cit.* 138.

[432] *ibid.* 5.

> Laboratory experiments have played a major role in this decontextualization of knowledge claims. Scientists used experiments to transform the concept of sex hormones into standardized substances with precisely defined qualities that then become accepted as such by the international scientific community and the industrial world.[433]

Now in order to make this claim stick Oudshoorn calls on Hacking's analysis of experimentation. She praises *Representing and Intervening* for its portrayal of the creation of artefacts in the laboratory, and quotes two passages from 'Filosofen van het experiment'[434] (a paper appearing in a Dutch journal), in which Hacking mentions the laboratory history of sex hormones:

> We did not find sex hormones somewhere in a lost corner, like a desert island lost in the mist. We ourselves called sex hormones into existence.[435]

Oudshoorn then comments:

> What Hacking describes here is precisely what science makes so powerful: its capacity to create new things and new worlds. By doing this, laboratory sciences establish a material authority that is very dominant in our present culture. ... By selecting specific methods of testing, scientists defined which substances they would label as "male" or "female."[436]

Now there seems to be some confusion here. It is over the existence and nature of the substances involved. Oudshoorn suggests that the discovery of chemical messengers, hormones, in the first decade of this century[437] led to a 'drastic change in the paradigm of physiology.'[438] Be that as it may, surely she would not want to deny that something chemi-

[433] *ibid.* 142.

[434] Hacking, I. (1989) 'Filosofen van het experiment' *Kennis en Methode* 13(1), 11-27.

[435] *ibid.* 21.

[436] Oudshoorn (1994) *op. cit.* 43.

[437] In 1849 Berthold had demonstrated the effect of *something* chemical playing a role in the regulation of organisms by implanting testes into castrated cocks and thereby preventing the onset of the signs of castration. But it was not until 1905 that the term 'hormone' was coined by Ernest Starling [(1905) 'The Croonian Lectures on the Chemical Correlation of the Functions of the Body' *Lancet* ii, 339-41].

[438] *ibid.* 16.

cal was discovered that affected specific organs and the organism as a whole. By contrast, it would seem extreme to deny that insulin, which can be manufactured and manipulated in controlled and specific ways, in fact exists. So hormones must exist for us to have doubts about whether it is appropriate to label them 'male' or 'female.' However, in terms of her support for Hacking's position on the creation of phenomena, the hormones themselves must be created by the experimental techniques involved in their isolation—*whatever sort of hormone they are*. It is unclear what sort of nominalism we are being offered here, just as we would expect from Hacking's failure to address biology properly. If it is the Hacking-Kuhn revolutionary sort, then hormones must be seen as real particular things that are being described under a constructed scheme which is relative to our interests—but they are taken to be chemical substances with particular molecular compositions and biological functions to get even this far in demonstrating their existence. In any case, there is not a simple technique for hormones *per se*. If it is the Hacking-Foucault dynamic nominalism that is on offer, then apparently well-grounded experimental techniques for the isolation of substances with particular functions and even defined molecular shapes are producing results with no 'objective' value at all. The first option seems incoherent, and the second leaves us with no reason to even begin looking at and evaluating the use and manipulation of *specific* hormones, in this case, sex hormones.

Taken as a regional study of *sex hormones* the conclusion we can reach from *Beyond the Natural Body* is that the hormonal story is complex and has been used to illustrate and control particular gender conceptions. It does not follow that hormones themselves do not exist, nor that the paradigm of the hormonal body is false. Appeal to Hacking to solve problems of the experimental 'decontextualization' employed by scientists and industry is unhelpful and obscures the importance of the Foucauldian archaeology needed to extract the good science from the rest.

Haiku

Thoughts of the rational

Mind capture parts of the world,

But not the moment.

5 Conclusions

Epistemic things, experimental systems, ensembles thereof, and experimental cultures are thus concepts with which I try to delineate the frame for an epistemology of experimentation that neither concentrates on concepts in the traditional internalistic sense nor on institutions and disciplines in the traditional externalistic sense of the historiography of science. It is an attempt to understand the cognitive dynamics of empirical sciences in terms of the structure of the practices from which they live. Experimental cultures are not homogeneous spaces. They are as bricked and tinkered as the experimental systems they are composed of. But they are held together by a specific kind of glue: material, not only formal, interaction; epistemic, not merely theoretical, compatibility.[439]

i The argument

As I stressed at the beginning, throughout my discussions the aim would be to articulate the current status of biology since it is an important science to the current concept of our being. The focus has been biology as practice and its relationship to science as practice and knowledge. I now want to state explicitly what I think has been shown.

The general points are as follows. Biology can and should be taken as a realist science. Analysis of its practice and theory reveal a pluralistic and disunified ontology and a complex relationship with the physical and social sciences. That biology shares a number of important epistemological tools with other sciences, most notably experimentation, demonstrates a rational methodology for science, which, in part, underwrites our examination of knowledge claims for their supposed objectivity. This further supports acceptance of scientific realism as a product of a number of rational practices and procedures and pro-

[439] Rheinberger, H-J., (1995) 'From Experimental Systems to Cultures of Experimentation' in *Concepts, Theories, and Rationality in the Biological Science* (see footnote 181).

vides us with the context for discussions of the impact of good and bad science on our lives.

However, it does not follow that one could extract explicit methodological rules that apply across whole disciplines or science *per se*. The unity of *methodology* involved here turns on issues deeper than science itself and is related to our being rational beings in a material universe.

This leaves us with a difficulty. How is it possible to accept a world of many different kinds of things that can be described with increasing accuracy without producing a single prioritised ontology, and yet still claim that there are valuable ways of investigating that world, ways that are based on fundamental epistemic practices? Indeed, it seems that use of these methods provides support for pluralism and disunity, and this might appear puzzling. I do not think it is, and taken together the metaphysical and methodological points discussed produce a general picture of science that has well established philosophical credentials.

The fact that unified methods can support disunified beliefs about the world—inconsistent sets of beliefs—is made by Nicholas Rescher:

> … method pluralism is something stronger than belief pluralism. Given the generality and power of what is at issue in a *method*, with its inherent multiplicity of applications, it follows that when authentically different cognitive methods are at work, then at least some of the resultant beliefs are bound to be different as well. And conversely, a consensus of beliefs across the entire range would indicate that there is consensus on methods as well. For where the beliefs at issue agree altogether, then there will be no justificatory basis for any differentiation with regard to these methods that we employ in resolving cognitive issues.

> But the reverse does not hold: different beliefs do not necessarily involve different methods. Even when the cognitive methods are the same, the same beliefs need not result, since, for example, the selfsame scientific-inductive method will, when orientated towards different bodies of accepted data, quite appropriately yield different results. Belief dissensus accordingly does not carry method dissensus in its wake.[440]

I believe that I have outlined this possibility in application in biology. Before I spell out the consequences in more detail, let us look at the overall argument presented.

1 Experiments

Experimentation is a factor in more than just our *scientific* knowledge gathering activities. It is a feature of our world that intervening in it and manipulating it can tell us something about it, and a lot (but not all) of science makes use of this fact. *Why* it is a factor is answered by analysis of the practice itself—that is, as far as I can see, a transcendental argument is implicated here. We can discover about ourselves as much as about the world by paying attention to what we do to find out things. However, spelling out the how and why of science experiments is a complex activity. The only aspect of experimentation that is necessary *is* intervention. This is as true of science as it is generally. None of the current analyses of experiments capture this fact adequately whilst remaining flexible enough to cover a range of important cases. Bayesian theorising in particular misses out real possible situations of experimentation where there is ambiguity about the status of the entities and phenomena involved. Similarly, social constructivist stories about experiments lack appreciation or assessment of the recalcitrant nature of the world to our actions on it. In the main, the failure lies in attempts to fit experiments into static epistemological theories about science. A dynamic epistemology, such as that proposed by Kitcher,[441] much better

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[440] Rescher, N. (1993) *Pluralism Against the Demand for Consensus* Oxford: Clarendon, Oxford University Press.

[441] Of course, I believe that dynamic models are possible *without* the explanatory unification Kitcher advocates.

captures the open-ended nature of the epistemology of experimentation. Such an epistemology would allow us to fit the aims of research groups as well as individuals into a scheme that is still generally rational in the outcome.

Hacking recognises that intervention/manipulation is a highly important epistemic activity that is used to determine the nature of parts of the world in which we live. However, he presents a case for scientific realism that is sensitive to his own observation that our knowledge is often relative to our own interests—this kind of knowledge he contrasts with genuine objective knowledge. That is, in Hacking's terms, manipulation is only an argument for realism (the existence of the entities discovered) where the theoretical contexts of our investigation can be 'transcended' through the use of the experimentally produced phenomena in other independent contexts. Only experiments in theoretical physics seem to offer such a context for Hacking. There is also some difficulty in extracting his attitude to kinds and properties as manipulable. Consequently we are left with an impasse. Either, he wants us to ascribe the status of 'real' only to things that are theoretically-free bare particulars, which itself seems untenable, or there is to be privileging of physics which runs counter to his own espoused pluralism—he offers no other criteria for realism.

In all cases, analysis of experiments in biology is scant. So we need a picture of experiments that retains the insights of epistemically valuable practices, but which is sensitive to a range of contexts and applications. This raises questions about the relationship between what kinds of things there are and how we find out about them, the relationship between metaphysical and methodological pluralism.

By looking at real cases of experimental practice in biology we find some continuity and some differences with physics experiments. Intervention and manipulation remain constant whenever we can be said to perform experiments. Methodology seems complex, but minimally unified in this way.[442] It also seems that where we want to accept or reject the status of 'real' for (apparently) manipulated entities the choice does not rest on Hacking's criterion at all. What is required is a clearer picture of the kinds of things that biologists investigate compared with those falling in the domain of physics. That is, we need to have something to say about metaphysics before we can fully respond to Hacking.

I have not given an account of the *details* of the replacement epistemology of experiments implied in what I have discussed. I do not believe it is necessary for my purpose.

2 Pluralism, realism and reductionism

Disunified metaphysical pluralism is a defensible position. There is a long tradition that resists pluralism because of the perceived requirement that everything be (ultimately) explicable within a single scheme. There are a number of ways of presenting what is meant by this, but even in a 'modest' form it involves linking notions of reduction, explanation and the unity of science. Dupré's robust defence of pluralism and disunity helps articulate and reject the assumptions that underpin this apparent necessity. The key notions are reduction and, especially in the case of biology, how to understand natural kind terms. The rejection of determinism in a closed form that requires that complete causal descriptions of events should be given by accounts of the micro-structure of objects is the third component of Dupré's argument. I have not pursued *this* line in much detail, but embrace his critique of the assumption of the necessity of causal closure in explanation of high-level (biological) phenomena.

[442] A note on models: I have suggested that models themselves can be used as experimental contexts. In this way 'observational' sciences, such as astronomy and primatology also rest on the same general epistemic valuable practices as more obvious experimental sciences. Indeed, in this most general way, the social sciences may be 'experimental.' Having said this, of course it does not follow that all experiments are transparent or rational or well grounded.

Reductionism comes in a variety of forms ranging from a strong theory using bridging principles to smoothly explain all aspects of complex higher-level phenomena in terms of lower-level, simpler theories, to 'explanatory interfacing.' The background of all varieties is questionable. Even if it were not so, in the case of biology there are a number of good arguments blocking the reduction of biological phenomena. These include consideration of proposed reductions of Mendelian genes to molecular genetic information and the failure of ecological theories to reduce to lower-level ones.

On the whole reductionism fails because acceptance of the maturity of complex theories such as one finds in the biological sciences implies the acceptance of theoretical vocabularies and relations that do not exist elsewhere and which cannot be matched by other theoretical vocabularies, where 'matched' can take a number of forms. So we need to know what is involved in the acceptance of these vocabularies and relations.

Natural kind terms would fit neatly into one complete, coherent, nested scheme of kinds if the world described by science were to be unified. They do not. Biology provides an excellent example of this fact. Different schemes of kinds, different taxonomies fit the world equally well. Since we cannot reduce these schemes one to another we must either take them to be real or achieve unity by adopting an instrumental understanding of biology—thus there is disagreement over the acceptance of irreducible vocabularies. This is the heart of the rift between Rosenberg and Dupré. The disagreement is resolved by consistently accepting pluralism. Accepting localised nesting of kinds is of further value in articulating why scientific realism works here.

Rosenberg's instrumentalism trades on a cut off point for scientific realism based on apparent epistemic limits. This in turn reveals a form of realism that is unacceptably strong and which supposes that physical science can be held separate from the rest of science.

This is a typical strategy in attempting to maintain unity in the face of pluralist alternatives.

Disunified pluralism here means not that there are no true descriptions of the world available, nor that *all* descriptions are equally true. Rather it means that there are a number of correct taxonomical ontologies that are the product of rational enquiries. The enquiries themselves reveal that these ontologies are not (always) unifiable into a single master ontology of things and kinds. Donald Davidson's discussion of conceptual schemes shows there just cannot be completely incommensurable ways of talking about the world.[443] Accepting this means that we should not suppose that in discovering different ways of talking about things and kinds, that this is the articulation of a paltry form of relativism. But neither does it mean that we are always in agreement or that the world has to be unifiable in our best descriptions.

All this on its own would be too simple. It supposes that we never make mistakes or that human interests are always rational. There are cases where we *think* we have discovered something, especially about ourselves, but have only invented a new category of description.

3 Biology and social science

Biology seems to have a close relationship with social science too. This arises from our calling on significant biological background knowledge in a wide range of social sciences, from social psychology to criminology to economics. Hacking calls on a particular reading of both Kuhn and Foucault to maintain the kind of distinction between hard science and the rest that Rosenberg implicitly embraces. This reading is used as the general support for

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[443] Davidson, D. (1974) 'On the Very Idea of a Conceptual Scheme' *Proceedings and Addresses of the American Philosophical Association* 47, reprinted in Davidson, D. (1984) *Inquiries into Truth and Interpretation*, Oxford: Oxford University Press, Essay 13.

experimental realism since it does favour physics over other sciences—physics finds different ways of redescribing real entities, whereas 'softer' sciences make up the objects *as* the schemes of description. However, a different reading of Foucault is possible that does not contain such a sharp division of science, nor the exclusion of objectivity from science. This interpretation of Foucault is sensitive to the regionalism of Bachelard that underpins Foucault's archaeological method.

Questions about objectivity and scientific realism are to be answered in the context of a rational examination of all the components involved in each study, as a regional study. Experimentation is therefore given a context where it can be used as support for belief in the truth of theories. The fact that we need to be always vigilant for the incorporation of other political/power interests into knowledge production does not diminish the possibility of rational discovery of an objective world. There *are* epistemic tools that allow this. Without *this* belief there is no clear way of looking at science for its value at all.

The result shows us that a disunified pluralist metaphysics as revealed by science is itself consistent with the notion that we have available to us general ways of finding out about the world that have many and varied applications.

ii Consequences

1 Biology

The consequences for biology of this examination of some epistemological and metaphysical questions are, perhaps, limited. In terms of technical considerations I doubt that the practice of biology could be challenged by a thesis such as this! What might result by following up my discussion is a better way of examining the relationship between biology and its technological applications. It can only be by finding some standard for epistemic testing of the knowledge claims we make that we can open up for inspection the contingency of technology, public policy and institutionalised power based on them. I think it is better to try to demonstrate this rather than *just* producing the theoretical framework for such inspections—although a two pronged attack *is* required.

2 *Epistemology, (dis)unity and metaphysics (again)*

Despite my insistence on their being grounds for picking out valuable epistemic practices—practices that themselves are a demonstration of an assumed (and necessary) attitude to rationality and realism—insights into applications and power should be regional in the style of Bachelard, Canguilhem and Foucault. The very fact of the metaphysical disunity in science supports this conclusion. It is entirely acceptable to question practices and applications that share similar methods of investigation since methods alone are no guarantee of rational discovery. Some of the ontologies we accept may well turn out to be unreal. Accepting that the whole of the ontological world described by science need not fit into a single scheme makes examination of parts of science easier and also, more importantly, more necessary. But this also opens up the realm of science by removing some constrictions on what science should be. The world we engage with is disunified at every level, particularly the political. As Dupré puts the point:

> ... if science were unified then the legitimate projects of inquiry would be those, and only those, that formed part of that unified whole. ... only a society with absolutely homogenous, or at least hegemonic, political commitments and shared assumptions could expect a unified science. Unified science ... would require Utopia or totalitarianism.[444]

[444] *Disorder* 261.

I take it we do not live in either of these kinds of state.[445] What we are still struggling to understand in philosophical terms is the relationship between our evolving societal interests and the practices we employ to understand the world, ourselves and our place in the world. But these questions are the very stuff of philosophy as it has been practised for millennia. Consequently, philosophy of science has a real, important role in the whole philosophical enterprise. This then is the kind of philosophy of science that, although often apparently neglected, we cannot afford to do without. What *does* distinguish science from non-science? Answering this question, just *understanding* this question, is radically important at this time. The reason being that to deny that science has been an important part of our contemporary world view would be foolish. This world view informs our philosophy, and yet we are beset with an infinite variety of knowledge claims from sources old and new. In newspapers misinformed articles about human genetics can be found on the same page as adverts for 'lottery beating' strategies and daily astrological forecasts. What we take to be rational practice, rational thought, science *as* rational practice in knowledge gathering and philosophy *as* the general overview of ways that we might come to see our rational being and practice, all these are tied together in ways that are not obvious. Looking again at the problem of demarcation will be one way of approaching a satisfactory description of the relationship between these factors. Again, nothing is being given absolute priority here. I am suggesting that the question of demarcation is a neglected and important area of our philosophical theorising about science that can now be revitalised without lapsing into 'mere' analysis of terms.

[445] On a personal, indulgent note: I began my secondary education in 1980 shortly after Margaret Thatcher was elected Prime Minister and I finish my time as a student only a few months after the Conservative Government has finally, and conclusively, been swept aside. I have been able to take advantage of excellent educational opportunities throughout this period despite this fact, not because of it.

The fear that embracing a metaphysically disparate world view leads to the collapse of valuable insights into that world is unfounded.[446] Indeed it avoids the excesses and paradoxes of the alternatives and allows us to properly start with what we believe and do rather than having to rule out accepted beliefs and practices *a priori*. This can only be for the good since it allows a complete examination of what goes on in our theorising and practices and therefore tends to *support* valuable insights. Biology is no exception to this principle. It provides evidence for its adoption.

iii Putnam, Rorty and rationality

I could leave my account of scientific realism open, claiming that the questions raised about its dependence on a clear notion of rationality and how it might fit into the general philosophical issues of global realism about word-world relations, are beyond the scope of my project here. However, I want to show what I think these connections might be in a little more clarity than I have in the body of the thesis.[447] That is, let me shift the focus a little so that I can present the reader with a flavour of how the discussion of biology and science can fit with the picture of rationality and philosophy[448] that I have been hinting at throughout.

[446] See, for other different approaches to pluralism *per se*, James (1909) *op cit.* and Rescher (1993) *op cit.*

[447] I hope that the reader does not take the argument of the thesis to rest on the analysis and support I give to the current philosophy of Hilary Putnam. Should this sketch turn out to be ill-founded, I intend the rest of the thesis to stand alone. However, I do believe that it would be inappropriate for me *not* to discuss the connections between what I have argued for so far and other contemporary issues about realism.

[448] Notwithstanding the points about my motivations for engaging in philosophical reflection set out in the Preface, p. v.

There is a continuing debate between Hilary Putnam and Richard Rorty about the similarities and differences between their treatment of philosophy, particularly on their theories of meaning, reference, rationality and the general nature of realism—both "common sense" realism and "metaphysical" realism. Both philosophers claim to have been asked (more than once) where they disagree with the other and have become increasingly explicit in their treatment of the other's work. I wish to articulate my own position by looking at some of the elements of this debate (best represented in a paper by Rorty called 'Putnam and the Relativist Menace,'[449] and from Putnam's side in 'Realism without Absolutes,' 'The Question of Realism' and 'On Truth').[450] What they consider worth discussing in their shared aims to move philosophy away from problems centred on finding foundations for metaphysics and epistemology, will help me illustrate some important consequences for the kind of multiple realisation of scientific realism implicit in what I have said. Furthermore, I believe that Putnam's most recent papers present a position on epistemology, rationality and metaphysics entirely consistent with concern for the agent perspective on science practice I have discussed. I think that the disagreement between Putnam and Rorty *is* substantive, but not easily identified.[451]

[449] Rorty, R. (1993) 'Putnam and the Relativist Menace' *Journal of Philosophy*, vol. XC No. 9, 443-461.

[450] All in Putnam, H. (1994) *Words and Life* (ed. Conant, J.) Cambridge MA: Harvard University Press, pp. 279-329. Hereafter referred to as *W&L*.

[451] For comparison with my treatment of this discussion see Goodman, R. B. (1995) *Pragmatism* London: Routledge. In his 'Introduction' to the collection Goodman suggests that the differences between Putnam and Rorty are best expressed as a difference of temperament (p. 10) using the distinction that James draws at the beginning of his *Pragmatism* [(1907), reprinted (1975) Cambridge MA: Harvard University Press] between "the tough-minded" and "the tender-minded" philosophical styles. This is, as should become clear, to rather duck the issues at stake.

Both Putnam and Rorty are keen to place a 'moratorium'[452] on the gamut of philosophical questions concerning the status of an external world and human relations to it. In explaining why they deem this necessary they have both spent much time and energy attempting to dismantle what they consider to be key issues in theories about world-word relations in particular.[453] Traditional world-word theories, they claim, have out-lived their usefulness and have generated insuperable, largely incoherent problems. In what follows I shall try to sketch their attitudes to realism and fit them into larger, more general pictures to provide a context for comparison. It is important to get clear what Putnam is (and is not) in fact claiming—the comparison with Rorty is telling.

In 'Putnam and the Relativist Menace' Rorty selects five points from Putnam[454] as the core of what he takes to be their common ground. These are in outline that:

1. Language (or mind) is so much part of the world that there can be no account of ourselves as "representers" or "mappers" of something independent of that language (or mind). There is no God's Eye point of view from which such a programme could be carried out. Putnam has argued for this position several times, perhaps most notoriously

[452] Putnam, H. (1990) *Realism with a Human Face* Cambridge MA: Harvard University Press, 118. Hereafter referred to as *RHF*.

[453] I shall not discuss the changing accounts of mind-world relations that Putnam and Rorty have given over the last twenty years. With reference to the material I touch on here Putnam describes their shifts in the following way: '... it seems to me that while I have moved from versions of "internal realism" I put forward after I left physicalism to a position which I would describe as increasingly realist—though without going back to the latter-day version of fourteenth-century semantics known as "metaphysical realism"—Rorty has moved from his physicalism to an extreme idealism which teeters on the edge of solipsism.' *W&L* 306.

[454] *RHF* 28, 178, 210; Putnam, H. (1987) *The Many Faces of Realism* London: Open Court, 83. Hereafter referred to as *MFR*; *RHF* 171.

in *Reason, Truth and History*:[455] he calls a belief in the attainability of talk about an independent reality 'externalist' and 'the view from nowhere.'[456]

2. We are beings who will always have view points on the world because of various values. We cannot stop being valuers and having an outlook that is based on values and interests.

3. Objectivity is always possible within this account of our being 'interest relative' adjudicators of explanation and interpretation of the world, because within the conceptual scheme there are ways in which explanations and interpretations are correct. 'What Quine called the indeterminacy of translation should rather be viewed as the *interest relativity of translation*.'[457, 458]

4. If during any practical activity we use a particular point of view, a conceptual scheme, then we must be aware of the importance of the pragmatist notion of the supremacy of the agent point of view. That is, there can be no appeal to the idea of 'things as they really are.'

5. It is not necessary to assume convergence to one picture of the world to understand knowledge.

What does this common ground amount to? Herein lies the problem. Rorty believes and argues that, given these points of agreement, Putnam must take up a position very similar to his own form of pragmatism—a pragmatism that owes as much to Heidegger as to

[455] Putnam, H. (1981) *Reason, Truth and History* Cambridge: Cambridge University Press, hereafter referred to as *RTH*.

[456] *RTH* 49-50.

[457] Rorty *op cit.* 443.

[458] There are Davidsonian connections here too, see Davidson, D. (1973) 'Radical Interpretation' *Dialectica* 27, 313-28, reprinted in Davidson (1984) *op. cit.* Essay 9.

James and Dewey. Rorty observes that, 'we seem, both to me and to philosophers who find both our views absurd, to be in much the same line of business. But Putnam sees us as doing something quite different, and I do not know why.'[459] So it is Putnam who contends that Rorty is playing a rather different game to his. The mainstay of his criticism in the past has been that Rorty is slipping into a form of relativism.[460] But with the development of his own position, he has added the charge that Rorty is guilty of adopting assumptions that are common to the very metaphysical realism he is supposed to be rejecting:

> Failing to inquire into the character of the unintelligibility which vitiates metaphysical realism, Rorty remains blind to the way in which his own rejection of metaphysical realism partakes of the same unintelligibility. The way in which scepticism is the flip side of a craving for an unintelligible kind of certainty (a senseless craving, one might say, but for all that a deeply human craving) has rarely been more sharply illustrated than by Rorty's complacent willingness to give up the ... idea that language can represent something which is itself outside of language.[461]

This needs some careful unpacking, but it is obvious that Putnam must see something in addition to the five common strategies that Rorty lists, or at the very least he considers there to be a way of interpreting them that involves neither the need for relativism nor Rorty's kind of pragmatism. Where to begin unpacking is difficult to determine; one must just start with one's own prejudices and commitments. Although it is to rationality that I want to finally turn let us look first at realism, since this has informed and been informed by much that I have discussed.

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[459] Rorty *op cit.* 458.

[460] *RHF* 19-20.

[461] *W&L* 300.

1 Metaphysical realism and God's cataracts

What is metaphysical realism? Some realists claim that realism is quite a boring thesis,[462] and it is only because alternative theses about word-world and mind-world relations are offered by anti-realists that anything needs to be said about realism at all. Putnam takes the metaphysical realist position to be that:

> ... the world consists of some fixed totality of mind-independent objects. There is exactly one true and complete description of 'the way the world is'. Truth involves some sort of correspondence relation between words or thought-signs and external things and sets of things. I shall call this perspective the *externalist* perspective, because its favourite point of view is a God's Eye point of view.[463]

Throughout the 1980s Putnam presented a number of arguments with the aim of showing that such a view was wrong-headed—because ultimately it had to be incoherent—and that consequently one should take up a position counter to this externalism: hence Putnam's 'internal realism' of *Reason, Truth and History*. So at first sight Putnam and Rorty appear to be denying that that particular position (metaphysical realism) can have any validity. However, the debate has become more complicated with the charge from Putnam that lurking behind this are some rather obvious anti-realist arguments on Rorty's side of the debate that themselves rely on many of the assumptions underpinning metaphysical realism, particularly in regard to there being only one complete and correct description the world.

Before proceeding any further I would like to clarify some points about my own use of the word 'realism.' Throughout this thesis I have been discussing scientific realism. The previous section showed how I take this to be a pluralistic and disunified notion that applies in a variety of different ways to all sorts of different aspects of science. That is, in so far as

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[462] Devitt, M. (1991) *Realism and Truth* London: Blackwell (2nd ed.), 1-25.

[463] *RTH* 49.

science can be picked out from other activities and parts of knowledge, realism about science cannot be described in a single, all-embracing theoretical framework, but neither can it be abandoned: science gets things right (and wrong) in lots of ways. When philosophers discuss realism about the whole of our experience, 'science' is often taken to mean any activity that goes beyond common sense. It is assumed that there is a distinction between science and other knowledge gathering activities and that science can be identified by either its methods, or content, or both without this distinction being spelt out. Look at how Devitt defines 'realism' and 'scientific realism':

> *Realism* Tokens of most common-sense and scientific physical types objectively exist independently of the mental ...
>
> *Scientific Realism* Tokens of most current unobservable scientific physical types objectively exist independently of the mental.[464]

Devitt's scientific realism is a species of realism.[465] However, as I have already stated, there are many ways that science can be picked out, but there are none that are necessary, except in so far as that activity is rational—not all fields of science rely on material manipulation of the world, different fields make use of different ontologies. There can be no single theory of scientific realism as such.[466] Given this diversity of methods and kinds one can only hope to produce accounts for the scientific reality of each separate kind of thing discovered and investigated by scientists. Again, this is not necessarily a pessimistic position in which to be. Since we can recognise the components of the discussion there must be much on which to agree. Realism, as agreement about there being a world about which we can disagree, and rationality, as the basis for the agreement, need not be disunified or plu-

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[464] Devitt, *op cit.* 303.

[465] Notice, also the physicalistic aspect of Devitt's realism. By now it should be clear that I absolutely reject this limiting of the real.

[466] That is, there just is no single theory of realism *about* science as a whole.

ralistic at all.[467] The relationship between scientific realism and 'general' realism can be investigated with more care, given this theoretical position. Of course this turns on further unpacking realism and its connection with rationality.

2 Rationality

A substantial part of the Putnam-Rorty divide is seen in what they have to say about rationality. Rorty believes that there is not a separate account of rationality to be had—separate, that is, from what we already do. He calls his theory a naturalist one, although to be consistent, he cannot really talk about rationality at all since he wishes to manage without such 'foundational' philosophical concepts. He says in *Objectivity, Relativism and Truth* that:

> To be a naturalist ... is to be the kind of antiessentialist who, like Dewey, sees no breaks in the hierarchy of increasingly complex adjustments to novel situations—the hierarchy which has amoeba adjusting themselves to changed water temperature at the bottom, bees dancing and chess players check-mating in the middle, and people fomenting scientific, artistic, and political revolutions at the top.[468]

His model for inquiry is that of the 'recontextualising' of what we know and accept with new situations and environmental stimulation. There can be no sense in accessing the level at which we would want to ask by what rules of reasonableness and rationality we perform this task. They are in flux, as the current cultural needs for this process of recontextualising in a Quinian network of beliefs change. What it is rational to do at any one time is simply what is acceptable practice to the socio-political order of the day. 'Simply' is an important word here because, for Rorty, there is no clear sense in which this order can be

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[467] There are Davidsonian connections here too. And we can also find further support for Putnam's rejection of the possibility of naturalising rationality. If scientific realism cannot be isolated as a single theory, there arises a difficulty in finding a context to even discuss rationality since it is not obviously a scientific concept at all, except by assumption.

[468] Rorty, R. (1991a) *Objectivity, Relativism and Truth—Philosophical Papers 1* Cambridge: Cambridge University Press, 109.

reversed—the grounding for the accepted socio-political order cannot itself be called into question. Scientists, for example, do not have any unique ways of getting at the truth:

> The habits of relying on persuasion rather than force, of respect for opinions of colleagues, of curiosity and eagerness for new data and ideas, are the *only* virtues which scientists have .... [There is no] intellectual virtue called 'rationality' over and above these moral virtues.[469]

The consequence of avoiding any talk of rationality *per se* is a deep sense of conservatism in Rorty's philosophy:

> We Western liberal intellectuals should accept the fact that we have to start from where we are, and this means that there are lots of views which we simply cannot take seriously.[470]

James Robert Brown has paraphrased this as 'We white, middle-class, males are happy to stay put and thumb our noses at other views.'[471] Unfortunately, if Rorty is to maintain his 'radical' edge regarding philosophy such surprisingly backward political moves seem necessary. So he makes the characterisation of rationality unimportant in his own construal of pragmatism. He also seems to be making the claim that the idea that rationality is unimportant and not separately discussible can be used as part of his defence against the charge of replacing a realist epistemology with relativism. This can only make any sense within the broader political picture he attempts to paint, which I shall not explore here.

Putnam, on the other hand, goes to great pains to say something about rationality, and how it is to fit into the scheme represented by the five points listed earlier. For Putnam, rationality is too fundamental to escape primary treatment in a theory about how we talk about

[469] *ibid.* 39.

[470] *ibid.* 29.

[471] Brown, J. R. (1994) *Smoke and Mirrors: How Science Reflects Reality* London: Routledge, 31.

the world (which is what he is trying to make coherent). He thinks that 'rationality is not an easy thing to give an account of.'[472] No theory that tries to naturalise rationality will do, Putnam claims, because it would require an account of the ways in which it becomes embedded in our social practice and that is where he sees a problem:

> If there is such a thing as rationality at all—and we commit ourselves to believing in *some* notion of rationality by engaging in the activities of *speaking and arguing*—then it is self-refuting to *argue* for that position that it is identical with or properly contained in what the institutionalised norms of the culture determine to be instances of it. For no such argument can be certified to be correct, or even probably correct by those norms alone.[473]

What does this self-reference amount to? Putnam calls such theories about rationality 'criterial,' that they give a framework for the verification of rationality. However, no theory itself could ever fulfil whatever criterion was specified, he claims; the thesis that 'nothing is rationally verifiable unless it is criterially verifiable'[474] is false. What Putnam means is that if the theory about how we are to test and recognise rationality, a theory that is grounded in criteria of such-and-such practice, is rational itself, then it cannot be shown to be because the theory is not part of that practice. This is how things stand in *Reason, Truth and History* where every other aspect of Putnam's enterprise is overlaid with a verification principle about meaning (there being no externalist position in *RTH* from which meaning can be acquired and examined). *RTH* offers no genuine account of why rationality *should not* be criterial other than his vague fears about relativism and 'continental' philosophers. Having said this, Putnam's theory of internal realism does go some way to placing rationality more precisely than at first might appear to be the case. He does some-

472 *RTH* 103.

473 *ibid.* 111.

474 *ibid.*

thing similar with talk about justification, and again Rorty finds the reason for this difficult to understand. If Putnam believes that 'our norms and standards of *anything*—including warranted assertability—are capable of reform,'[475] Rorty wonders what 'idealized rational acceptability' can mean in Putnam's theory of a surrogate idea for truth except,

> ... acceptability to an idealized community. Nor can I [Rorty] see how, given that no such community is going to have a God's eye view, this ideal community can be anything more than *us* as we should like to be.[476]

And by 'us' Rorty means, of course, 'educated, sophisticated, tolerant, wet liberals.'[477] So Rorty thinks that in talking about rationality in the same breath as 'warranted assertability,' which Putnam wishes to use as the standard for the testing of claims to user-friendly knowledge, rationality should be as ethnocentrically locatable as anything else. But this would only be the case if the five points of agreement required the pragmatism Rorty espouses. Why they need not is the heart of Putnam's internal realism. To agree to 1-5 does not mean that Putnam wants to duck out of the issues that arose in the old realist picture. And why this is so will become clearer if we look at the motivations behind Putnam's thinking about realism and rationality.

Since *RTH* Putnam has moved away from the verificationism that informs his argument there and has radically improved his position. His most recent writings have tended towards a deflationary notion of realism, truth, representation and rationality. In many ways this has involved clarifying what he originally claimed. He now says:

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[475] *RHF* 21.

[476] Rorty, R. (1993) *op cit.* 451.

[477] *ibid.*

> ... if "realism" is understood simply as the idea that thought and language can represent parts of the world which are not parts of the world and language, then no one should be convinced that realism in that sense is an incoherent idea by the mere thought that we do not and cannot have "direct access" to the world outside of thought and language.[478]

And his argument about realism with Rorty has been glossed by James Conant—who echoes and refines almost all of Putnam's current work—in the following way:

> We pass from the (metaphysical realist's) perception of us being *able* to step outside of our skins to a perception of us as being *unable* to do so. We see ourselves as forever sealed *within* our skins: confined, as it were, to *our* forms of language and thought. Rorty ... trades on such a confinement ... This sense of confinement—of being trapped inside something (language, thought)—draws its life, Putnam now suggests, from the temptation to express the failure of metaphysical realism in terms of something we cannot do.[479]

With this most recent Putnam I am in agreement. Putnam now thinks that we can find fault with the claims about what metaphysical realism can do—it is indeed an incoherent theoretical position. But it does not follow from this that we should give up all our everyday notions about acting rationally based on the evidence of our being able to describe features of the world. To do so is to mistakenly believe that because metaphysical realism is incoherent its 'opposite'—the idea that we are stuck with no notion of reference or truth in a common sense way—must be correct. So whether his argument is really any good or not, Putnam's position is very different from Rorty's. Putnam is making a real claim about the nature of certain philosophical issues that places them beyond a simple naturalised picture that can be embedded in our culture's norms and language and fixed there. This, I think, is precisely the kind of background that makes sense of the ideas I have been following throughout. We need a means of recognising that we can describe the world and that we

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[478] *W&L* 299.

[479] *ibid.* xxvi.

can make mistakes. We also need a way of seeing that a diverse set of ontological commitments is possible for beings with shared epistemic means of understanding the world. Naturalism across the range of philosophical commitments involved would not help, since naturalism implies that things in general are explicable in the same unifiable scheme.[480] Rorty's pragmatic naturalism is yet another theoretical framework that draws the teeth of any critique of current practices and concerns.

Part of the problem had been that Rorty read Putnam's talk of 'internal realism' as just the kind of confinement Putnam now wants to explicitly avoid in this reading of his position. For Putnam, internal realism was a 'philosophical perspective' or 'temperament' designed to provide a framework in which each of the five 'agreed' points could make sense without recourse to naturalism or relativism. In *RTH* internal realism is:

> ... a realism which recognises a difference between '*p*' and 'I think that *p*', between being *right*, and merely thinking one is right without locating that objectivity in either transcendental correspondence or mere consensus.[481]

The Putnam of *RTH* rejects metaphysical realism as the evil demon that allowed us to speculate about evil demons and brains-in-vats as genuine sceptical worries in the first place, and the cause of various tangles over reference. Metaphysical realism is a three headed beast. The three elements of it—a fixed mind-independent world of objects; a single true account of this world; and truth as correspondence—are rejected by Putnam explicitly. He also claims that all these three parts of metaphysical realism have to be taken together, despite the fact that realists, such as Devitt, have consistently argued that we should sepa-

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[480] I take it that there is no clear notion of naturalism to be had in any case, above and beyond a belief that our theories about our psychological states and the nature of the physical world are enough to fully describe, explain and support all other concepts we employ.

[481] Putnam, H. (1983) *Realism and Reason—Philosophical Papers, Volume 3* Cambridge: Cambridge University Press, 225-226.

rate ontological, epistemological and semantic issues as far as we possibly can in order to make sense of what realism is about.[482] In *RTH* Putnam thinks that the issues cannot be independent because no part of human debate can be freed from the fact that language structures what is discussible and meaningful—hence Rorty's claim that they are so close as to be practically in agreement.

To digress for a moment, Brown picks out three different ideas that are crucial, he claims, to the definitions of realism in the work of Boyd, Dummett, Newton-Smith, Papineau, Sellars and van Fraassen. On top of the independence thesis above, he lists the idea that we can make rational (though fallible) choices among rival theories, and that science aims at the truth.[483] In *RTH* Putnam's criticisms of metaphysical realism only focus on the independence thesis. Internal realism is set up as a cure for the problems and paradoxes generated by the belief that theories are true or false, and that what makes them that way exists completely independently of us. Metaphysical realists, he claims, fail to appreciate that the usefulness of talking about representing the world to ourselves in thoughts or words (a seventeenth century construction according to Rorty[484]) was just a way of solving certain old fashioned philosophical problems in the first place. Internal realists:

> ... hold that *what objects does the world consist of?* is a question that it only makes sense to ask *within* a theory or description ... that there is more than one 'true' theory or description of the world. 'Truth' ... is some sort of (idealized) rational acceptability ... There is no God's Eye point of view that we can know or usefully imagine; there are only the various points of view of actual persons reflecting various interests and purposes that their descriptions and theories subserve.[485]

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482 Devitt, M. (1991) 'Introduction'.

483 Brown, J. R. (1994) 81.

484 Rorty, R. (1979) *Philosophy and the Mirror of Nature* London: Blackwell, Part II.

485 *RTH* 49-50.

So the theoretical structure behind internal realism is not, *RTH*-Putnam claims, a clever form of relativism. He believes he escapes such a charge by his 'idealized rational acceptability.' He embraces the second part of Brown's characterisation of realism, that we can make rational choices amongst alternative theories, because, as was suggested earlier, Putnam tries to avoid pinning rationality to any particular socio-political community in the way that Rorty does. The rightness of our claims to knowledge *can* be objectively tested (against the notion of 'idealized rational acceptability') without recourse to mere consensus. Unfortunately all Putnam can say about 'idealized rational acceptability' in *RTH* is that it is like a frictionless plane, something we can never obtain but can approximate to for calculation and predictive purposes. Once again this point is clarified by Putnam's recent adoption of a deflationary and pluralistic reading of his earlier position. (Although it should be noted that he does not embrace a simple disquotational theory of truth, as 'On Truth'[486] demonstrates.)

But if all the epistemological, ontological and semantic issues involved in internal realism are tied together, Rorty cannot see how Putnam fails to take the road towards a picture of rationality as an identifiable part of our whole socio-political, ethnocentric account of culture in which philosophy has a small role to play. Internal realism raised many questions for Rorty, which Putnam has endeavoured to answer, whilst refusing to adopt the form of cultural relativism espoused by Rorty.

> Meaningfulness in a public language is indeed a culturally relative property; but warranted assertability cannot be identified with a culturally relative property any more than truth can be ... [487]

[486] *W&L* 315 ff.

[487] *ibid.* 324.

The reason being, as already noted, Putnam does not believe that a naturalised notion of truth is available—with no closed account of truth or warranted assertability they cannot be relativised to anything. And as we have seen, Putnam's shift away from the more confusing parts of *RTH* have helped to clarify exactly why Rorty has been missing the point here.

3 Deeper still—realist rejoinders

Before I indicate my own stance on the debate, I want to look at how defenders of the kind of realism that both Putnam and Rorty attack might respond to their criticisms.

In *Rationality and Science*[488] Roger Trigg argues that only by grounding rationality in reality, in the way things are rather than in descriptions of the way things are, can we ever hope to make sense of our interactions with the world and our attempts to gather knowledge about that reality. This is a typical realist claim, and it is typical of post-Wittgensteinian philosophy of language and metaphysics in that it is precisely that distinction, between reality and a description of reality, which cannot be drawn. Both Putnam and Rorty both appear to be the Wittgenstein camp. They believe that the failure to make sense of a God's eye point of view means that there can be no way of accessing what reality is like apart from our schemes for talking about it. But, Trigg states, it seems odd that we have talk of 'our' schemes here at all. He says:

> There has to be something radically wrong with a metaphysics that claims to be about reality but turns out to be about how we engage with it.[489]

Trigg's point is that to set up realism as a theory about how we engage reality (which is what Putnam does with internal realism), is to miss what is being proposed by the realists.

[488] Trigg, R. (1993) *Rationality and Science* London: Blackwell. 117.
[489] *ibid.* 117.

The same point is made by Devitt when he says that realism is an ontological thesis at heart, and that as a working assumption in looking at it we must separate the problems of engagement from the questions of existence.[490] It is for this reason that Trigg is also puzzled by Putnam's claim that the search for a foundation for Being has failed[491]—for realists Being needs no foundation. Once again, there is a confusion here in reading the *RTH* Putnam to be closer to Rorty than is in fact the case. If one really accepts the Wittgensteinian line then there cannot be a position where there is no escape from language—exactly what Putnam points out about Rorty's position and what the realist respondents fail to recognise in Putnam's philosophy.

4 Attitudes

Rorty sees the realist position as part of a whole attitude to philosophy in which human beings try to make sense of their lives by placing them in a larger context of a non-human reality. He contrasts this with the context of human activity itself. He calls the first attitude a striving towards objectivity, the latter a move towards solidarity.[492] He says that the liberal West dispensed with religion because it required human beings to be humbled before an external power; objectivity is the continuation of this unnecessary trend in which God is replaced by reality. His whole approach in grounding objectivity in solidarity is so that philosophers who follow his so-called pragmatist line,

> ... do not need an account of the relation between beliefs and objects called 'correspondence', nor an account of human cognitive abilities which ensures that our species is capable of entering into that relation.[493]

[490] Devitt, M. (1991) *op cit.* Chapter 11.

[491] Putnam, H. (1990) *RHF* 19.

[492] Rorty, R. (1991a) 21.

[493] *ibid.* 22.

Once again, one wonders how accurate this picture can be since it is hard to find *any* area of human activity where this picture of cosy agreement holds true; and the idea of wet, western, liberals ignoring anyone who fails to join the club seems somewhat incompatible with what I generally assume being tolerant and liberal is all about.

Rorty calls the realist position about the independent existence of the tokens of common sense and science 'a banal anti-idealist thesis' and 'as no more than out-moded rhetoric.'[494] He also explicitly says that there can be no sense in the Kuhnian notion that people with different theories live in different worlds.[495] Why he rejects this thesis is not clear, although it is most probably because it is too much in the mode of the older antirealist tradition.[496] The deconstruction of the sceptical problematic in *Philosophy and the Mirror of Nature*, by the demonstration that there is no need to talk of mental or linguistic representation, leaves the question of the independent existence of objects untouched—but unknowable in the sense that we cannot see how well we are doing in describing anything. The theory he claims to share with Putnam (i. above), that there is no view from nowhere, is about what we as human beings cannot do, given that we *are* human beings, and is not about reality at all. In fact no realist would want to say that they were attempting to find a view from no-where—as Rorty and Putnam would agree, such an account of the world would be of value to no-one.

Rorty's major positive programme tries to make the more substantive questions traditionally related to realism/anti-realism (questions about epistemology, he would claim) fall free of our philosophical enterprise. Devitt's claim that Rorty is a realist (because of his failure

[494] Rorty, R. (1991b) *Essays on Heidegger and Others—Philosophical Papers 2* Cambridge: Cambridge University Press, 354.

[495] Rorty, R. (1979) 324.

[496] Davidsonian considerations urge agreement on this point.

to explicitly reject the independence thesis) is a claim made from within a framework that Rorty denies is of any use.[497] Rorty wishes philosophy to become cultural hermeneutics, realism is boring and trivial:

> ... my strategy for escaping the self-referential difficulties into which 'the Relativist' keeps getting himself is to move everything over from epistemology and metaphysics to cultural politics, from claims to knowledge and appeals to self-evidence to suggestions about what we should try.[498]

Finally, we see that if Rorty did not make the move of emphasis to the shelter of 'us' western, liberal intellectuals, he would justly be accused of being the relativist that Putnam has claimed he is. But since the socio-political picture that results may be conservative and potentially easily exploitable by those not so keen to be in solidarity with him, one wonders whether it might not be worth just sticking with the relativist label.[499] All these considerations support Putnam's analysis of Rorty's failure to rid himself of the Cartesian scepticism informing the realist's theorising. Devitt's rampantly realist musings on Rorty are more revealing than they might at first seem—relativism and realism being two sides of the same coin.

5 The pragmatist, the realist and I

Let me now say what I think all this shows. From a survey of literature it initially appears that Rorty wants to drop the whole area of epistemology and metaphysics involved in the realism/anti-realism debate via a deconstruction of the problematic that generated it,

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[497] Devitt *op cit.*

[498] Rorty, R. (1993) *op. cit.* 457.

[499] It is worth noting, however, that one response to this charge might be something like the following. Liberalism is as much a part of the cultural game as everything else: that the whole game of 'liberating' suppressed groups is as much a socio-politically determined notion as every other political overview. Consequently there can be no way of externally judging whether Rorty gets a hold on 'liberalism' or not. There isn't a God's Eye point of view for that either. I suspect that such a defence by Rorty might hold water only if his overall picture holds water in dealing with the deeper philosophical worries raised here.

namely scepticism; Putnam, on the other hand, is keen to demonstrate why the traditional picture fails and show how it is related to each part of that traditional account as a new and better story. From what has emerged, it should be clear that it is precisely because Rorty has *not* escaped the sceptic's problematic that Putnam wants to distance himself from Rorty.

> ... why is Rorty so bothered by the lack of a *guarantee* that our words represent things outside themselves? Evidently, Rorty's craving for such a guarantee is so strong that, finding the guarantee to be "impossible," he feels forced to conclude that our words do not represent anything. It is at this point in Rorty's position that one detects the trace of a disappointed metaphysical realist impulse. I think the trouble here comes when one does not properly explore the sort of "impossibility" which is at issue when one concludes ... that such a guarantee is indeed impossible. What I want to emphasize is that Rorty moves from a conclusion about the unintelligibility of metaphysical realism ... to a skepticism about the possibility of representation *tout court*.[500]

Now, as the last section indicates, this points us further away from some of the technical difficulties so far discussed to issues connected to the general approach to *philosophy* that Putnam and Rorty adopt.

Rorty accepts a broadly naturalistic picture on a number of issues. In particular, he firmly believes that, given the prevalence of natural science in our/his culture, a physicalist account of the world is quite acceptable as a theory that we do work with usefully. However, there is some doubt whether the kind of eliminative materialism he espouses can be understood without the background of the debate about how mind fits with the world, a background Rorty seems to want to do without. Somewhat more consistently, Putnam is unhappy with such theories since he sees in them the kinds of naturalism he is trying to avoid elsewhere. He believes that any attempt to place mind in the natural world, fitting it into

[500] *ibid.* 299-300.

some ecological niche, presents the danger of having to talk about what the evolutionary benefits of mind could be; he then sees only a small step to having mind map the world and the regeneration of a correspondence theory of truth.

Putnam is prepared to make claims for philosophy that take it beyond the cultural hermeneutics that Rorty wishes to see. Putnam continuously points out that we cannot let the norms of the day have a definitional role in dealing with the very tools of reasoning itself. He believes that philosophy has more than just a socio-political role in cultural debate. Philosophy *can* use transcendental arguments to show how assumptions about reality and realism are misguided and incoherent; we cannot just move the debate over to politics, because the key concepts at the heart of philosophy as we practise it—reality, rationality, justification and belief—cannot be found elsewhere. He states that:

> ... no philosophical position of any importance can be verified in the conclusive and culturally recognised way I have described.[501]

And he later highlights the kinds of impulses he sees motivating Rorty (and his similarities to Auguste Comte) and himself:

[501] *RTH* 111.

> The sort of philosophical reflection I have been engaging in is just the sort of reflection that both Comte and Rorty see as *pointless*. For Comte such reflection is a throwback to a prescientific age; for Rorty, a reluctance to fully enter into a postmodern one. ... what is common to Rorty and Comte is the idea that much of what we know cannot have the status it seems to have. For Richard Rorty the recommended response is to take a more "playful" attitude to what we think we know; and for Auguste Comte it is to sternly restrict ourselves to "positive knowledge." But understanding the temptation and seductions of the idea that Comte and Rorty share, so that we can live with those temptations and seductions without succumbing to them, is far more important—and more valid as a response—than pretending that the world is either just a playpen or just a scientific laboratory.[502]

Putnam cannot claim that internal realism is about deconstructing the whole of the problematic about scepticism and realism but is, rather, an answer to some of the problems there. Perhaps Rorty, in the end, is providing these answers too, but I believe he tries to take the more extreme philosophical route to close down whole branches of philosophy as normative activities. In this task he fails, for the very reasons Putnam highlights. Putnam just wants these areas limiting to ways he considers coherent.

A final way the tension in the debate can perhaps be captured is by noticing that Putnam often lumps Rorty together with Foucault. If one is prepared to accept similarities there, despite the different historical backgrounds to their philosophies, then there may be a case for saying that in the Putnam-Rorty discussion we have all the components of the analytic-continental discussion in miniature. Consider, Putnam accepts that he is involved in detailed linguistic games to produce clarification of important concepts, of philosophical arguments that do real work; while Rorty talks of cultural hermeneutics and socio-political solidarity. That is, if from a 'continental' perspective they can get so close as to agree on the five points I listed above, and yet still remain at logger-heads, then one suggestion

[502] *W&L* 309-10.

might be that movements towards a more unified picture of the whole of western philosophy will also remain incomplete. However, the reading of Foucault I have given[503] should scotch this and suggests that Putnam could add to his own position by a more careful consideration of Foucault.

I promised at the beginning of this section to say something about my own approach to rationality and realism. In the main I agree with the most recent Putnam, and I hope the debate between Putnam and Rorty, although given in a somewhat sketchy form, shows why I do not consider this a rejection of anything one wants to call 'realism' in a pre-philosophical engagement with the world. The context always determines the best ways of proceeding—the best ways are what are the most rational, of course. For example, in investigating a world that intervenes in our physical and biological being, structured physical and biological intervention is usually best, that is experiments are usually best. Rationality itself cannot be fixed by simple reference to our current best descriptions of our psychology or socio-political circumstances without being circular. But neither can we suppose that any philosophical theory will pin it down in something metaphysically transcendent. Philosophical analysis will not be able to complete the task of capturing rationality.

A disunified, pluralistic metaphysics fits perfectly well into such a picture and leaves us with scope to demonstrate that metaphysics. It also provides its own test for the scientific theories we produce to articulate it.

I leave the final word to of this section to Putnam:

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[503] p. 172

We make up uses of words—many, many different uses of words—and the senses of "agree" in which our various sentences "agree" with reality, when they do, are plural indeed. Yet for all that, some of our sentences *are* true, and—in spite of Rorty's objections to saying that things "make" sentences true—the truth of "I had cereal for breakfast this morning" *does* depend on what happened this morning.[504]

[504] *W&L* 302.

Haiku

Waking and stretching.

Unfinished pillow dreams

Cooled by the frost.

6 Postscript

> We must take more responsibility for the Nature (and the biology) we construct. We do not, however, manufacture either our own natures or Nature out there as detached, God-like subjects. Our responsibility, then, is not the responsibility of unmoved movers, absolute originators projecting order on chaos. Rather, the construction is mutual: it occurs through intimate interactions. By the same token, we do not simply record facts about external Nature, any more than we are simply manifestations of an internal nature encoded in some genetic text. "Information," that is, is not given independently of us, and because this is so, we cannot disclaim a kind of ownership. Our cognitive and ethical responsibilities are based on our response-ability, our capacity to know and to do, our active involvement in knowledge and reflection.[505]

We are biological beings. We are also social, political, sexual, creative, destructive, emotional, rational, physical, chemical, psychological, spiritual, ethical, finite, ... beings. This investigation has been into the nature of biology as it stands in relation to other epistemological practices and procedures associated with it. There are other important questions about biology. Evolutionary theory and the process of natural selection are fascinating and have consequences for both epistemology and metaphysics. In conducting this investigation I hope I have provided at least part of a context for the discussion of these kinds of questions. However, this has not been my direct intention. I have been striving here to begin a study of a small aspect of our contemporary selves: our being biological. In no sense could this be a complete study of human being—my hope is only to generate a new perspective on our biological nature and perhaps anticipate some of the consequences of taking this perspective. What this perspective might be is still unclear to me, but the study that will be the support to it is, I think, well worth undertaking. Having said

[505] Oyama, S. (1991) 'The Conceptualization of Nature: Nature as Design' in Thompson, W. I. (ed.) *Gaia 2 - Emergence: The New Science of Becoming* New York: Lindisfarne Press, 179.

this, it is entirely possible that we lose more than we gain. So be it—that would be valuable too.

Harré has conducted a series of studies of human being from three perspectives under the general heading *Ways of Being*.[506] The three perspectives are the social, the personal and the physical. Whilst Harré's studies are incomplete and perhaps in parts politically unacceptable (by my own lights at the very least), his general strategy is attractive—a philosophical examination of the complex and multiply related concepts that make up our current thoughts about our selves and our culture. It retains a generally rational approach—the role of philosophy with which I began is recognisable in many of Harré's points. But I want to take this kind of philosophical study further into more speculative territory with regard to our biology. Writers obsessed with science as the sole guide to our epistemic practices have looked at biology in many ways. Dawkins' attempts to analyse whole swathes of our culture in terms of an evolutionary/biological model,[507] are radically unattractive to me. There are both reductionism and scientism here, and his epistemology is fatally flawed by its simplicity. In any case, such a theory does not help us decide what place biology and its attended technologies can and should do in our lives. Something much more general and philosophically broad is required for that.

Biotechnology is technology. Like all technology its development is contingent.[508] There is nothing of necessity built into any technology. The contingency rests on the metaphysical and epistemological assumptions that inform the actual paths we choose to take. This is especially true of biotechnology. What counts as a biological property? What determines

[506] Harré, R. (1979) *Social Being*; (1983b) *Personal Being*; (1991) *Physical Being*; all three, Oxford: Blackwell.

[507] Dawkins, R. (1982) *The Extended Phenotype* Oxford: Oxford University Press.

[508] For a history of this technology in the twentieth century see, Bud, R. (1993) *The Uses of Life: A History of Biotechnology* Cambridge: Cambridge University Press.

biological properties? What are the boundaries of biological kinds? What can we know about such a complex and diverse subject matter? What is a function? What is life? How might it be manipulated? I am convinced that it can only be through a continuing pushing at the edges of what these current assumptions are that we will cope with the possibilities and the contingency. Included in the study would be contemporary contingent concerns with health and body images,[509] and the connections between biology and morality.

But life is always a difficult subject:

> Supposing that knowledge is one of the things that is fine and valuable, and one kind rather so than another either for its accuracy or by being of better or more wonderful things, on both these grounds we would be right to place the inquiry into the soul among the first kinds of knowledge. But knowledge of the soul is also held to make a great contribution to the complete understanding of the truth and especially towards that of nature. For the soul is, so to speak, the first principle of living things. We seek to contemplate and know its nature and substance and then the things that are accidental to it. Of these same are held to be affections peculiar to the soul itself, and others belong to the animal as well as in virtue of the soul. In general, and in all ways, it is one of the hardest of things to gain any conviction about the soul.[510]

Aristotle's *psyche* is best translated as 'animating principle' or 'principle of animation.' *De Anima* is an investigation of the idea of life itself. As arcane as such thinking seems there is still much to be learned from it. As I mentioned in the Preface, there is growing philosophical interest in artificial life research in order that the mechanisms of living systems can, after millennia, find some formal description. I am not necessarily suggesting that formalism answers the fundamental questions that are posed by this work, but it does highlight the possibility of taking up Aristotle's enquiry with some of its motivation intact. I would like to suggest, therefore, that the next stage of the general enquiry I have outlined, should

[509] Notice how ageing has become a health issue in last few years!

[510] Aristotle (1986) *De Anima* trans. Hugh Lawson-Tancred Harmondsworth: Penguin Books, 126 (402a).

be an open survey of the concept 'life,' how it is understood in relation to biology and ourselves, to death, to the natural world and our responsibilities to it. I think such a study would be a more appropriate way of approaching questions about human beings as the product of evolution and accident. The details of this study will involve some quite technical material, but as with this thesis the aim will be to comprehend the key components and their consequences. This would be impossible without understanding what we can know and how we can know it when considering our given examples of living things—for myself I now have that understanding in outline. Consequently, the next stage of my study will be an investigation of the concept 'life.'

One other issue that I think could provide fruitful future research as part of my project is the demarcation of science from non-science. Dupré raises it,[511] as do commentators on Foucault, as I mentioned earlier. Fresh insights such as these could re-invigorate the important issues here and help clarify the context of investigation of interests and values in science and philosophy of science.

Science and its connection to our concept of rationality is fascinating and, I believe, a vital aspect of human development and progress. But science is not all that our culture is. It is limited and investigating its inveigling nature is a task that is neglected at the cost of this progress. Human beings are not passive observers of an ordered and perfect world. Such a picture must be dispatched. Aside from its restrictive and enslaving consequences, it is deeply unimaginative and stifles exciting futures where there is hope for human and environmental relations. Wonder and liberation are close at hand if we pay attention to what we think we know.

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[511] *Disorder* Chapter 10.

I hope I have fulfilled my own goals of experimenting a little with some ideas to help keep alive our responses to the parts of the world we assume to be real. and that philosophy (of science) is still exciting!

17 November, 1997

(Final)

7 Bibliography

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