WITTGENSTEIN AMONG THE SCIENCES

Philosophy and Method in the Social Sciences

Series Editor: Phil Hutchinson, Manchester Metropolitan University, UK

Engaging with the recent resurgence of interest in methodological and philosophical issues in the human and social sciences, this series provides an outlet for work that demonstrates both the intellectual import of philosophical and methodological debates within the social sciences and their direct relevance to questions of politics, ethics or policy. Philosophy and Method in the Social Sciences welcomes work from sociologists, geographers, philosophers, anthropologists, criminologists and political scientists with broad interest across academic disciplines, that scrutinises contemporary perspectives within the human and social sciences and explores their import for today's social questions.

Wittgenstein among the Sciences

Wittgensteinian Investigations into the 'Scientific Method'

RUPERT READ
University of East Anglia, UK

Edited by
SIMON SUMMERS
University of East Anglia, UK

ASHGATE

Published by
Ashgate Publishing Limited
Wey Court East
Union Road
Farnham
Surrey, GU9 7PT
England

Ashgate Publishing Company
Suite 420
101 Cherry Street
Burlington
VT 05401-4405
USA

www.ashgate.com

British Library Cataloguing in Publication Data
Read, Rupert J., 1966-
Wittgenstein among the sciences : Wittgensteinian investigations into the 'scientific method'. – (Philosophy and method in the social sciences)
1. Wittgenstein, Ludwig, 1889-1951. 2. Kuhn, Thomas S.
3. Science–Philosophy. 4. Social sciences–Philosophy.
5. Philosophy and science. 6. Science and the humanities.
I. Title II. Series III. Summers, Simon.
501-dc23s

Library of Congress Cataloging-in-Publication Data
Read, Rupert J., 1966-
Wittgenstein among the sciences : Wittgensteinian investigations into the "scientific method" / by Rupert Read edited by Simon Summers.
p. cm. – (Philosophy and method in the social sciences)
Includes bibliographical references and index.
ISBN 978-1-4094-3054-4 (hardback) – ISBN 978-1-4094-3055-1 (ebook)
1. Social sciences–Methodology. 2. Social sciences–Philosophy.
3. Philosophy and social sciences. 4. Wittgenstein, Ludwig, 1889-1951. 5. Kuhn, Thomas S. 6. Winch, Peter. I. Summers, Simon. II. Title.
H61.15.R4293 2011
001.4'201--dc23

2011040601

ISBN 9781409430544 (hbk)
ISBN 9781409430551 (ebk)

Printed and bound in Great Britain by the MPG Books Group, UK.

Contents

Preface

> [The very nature of philosophical investigation] requires us to travel over a wide field of thought criss-cross in every direction.
>
> Ludwig Wittgenstein, *PI,* Preface.

This book is a Wittgensteinian investigation of the concept of 'science', based around exemplars and cases.

Nevertheless, there is something inherently paradoxical about writing a *book* with the ambition of the present work. The paradox is of the same general nature as that that faced Wittgenstein, and that led him in his later period to not finish any work. The problem is that there is something reificatory, dangerous, about writing a book when one believes, as I do, that the book can have no 'positive' task. 'Negative theology' offers a kind of model here, but not perhaps a 100 percent happy one: My, resolutely/steadfastly-therapeutically Wittgensteinian approach (see e.g. my *Applying Wittgenstein*) similarly cannot exactly seek to *rectify* the many defects I think may be present in the many instances of philosophy/methodology/would-be-science that I aim to interrogate in the present work. That is: I cannot put them right by affirming/saying what the *right* theory is, or what the *right* answer is. I not only cannot tell you what God is really like, but not even what science etc. is really like … 'All' I can do is offer warnings, questions, perspectives, alternative possibilities. That is of the nature of the Wittgensteinian 'methodology'. And moreover, all I can do is offer these in the knowledge that different readers will differ in their need for them, in their desire for them, in their familiarity with them, etc. The Socratic and Platonic difficulty with the adequacy of *writing* as a vehicle for philosophising or for methodological reflection is writ large in Wittgenstein; and, I fear, and hope, in the present work, if such it is.

This puts a strain on the reader. I am sorry for this; but then again, it is unavoidable. What am I *saying*, in the present work? Well, nothing. That follows directly from my 'New Wittgensteinian' approach. (See for this Crary and Read's (2000) collection, *The New Wittgenstein*. Cf. also the later Gordon Baker's (2004) approach to philosophy.)[1]

So this book is constantly critical, in a way that might weary some readers, and may be seen by them or by others as wearily 'evasive' or merely allusive in terms of what it is positively putting forward. 'Guilty' as charged. To do justice to the subject-matter, to be true to Wittgenstein, there is no alternative. It's not clear what is being *defended* in this book: because nothing is. There is no positive doctrine

1 Cf. also Duncan Richter's (2009). Richter sees clearly the radically therapeutic/liberatory aim of my writing.

(cf. *PI,* 126–8),[2] no theory to defend. I spend much of my time fending off possible misunderstandings: because they are so 'natural' and because that is one of the main things that there *is* still to do.

To make things possibly even worse: this is a sketchbook, criss-cross-style, of varia. It is a gathered collection of writings and papers of mine, that myself and my Editor have more or less unified, we hope, around the themes that I will shortly endeavour to explicate. For this reason, added to what I have already said, it is perhaps very important to give the potential reader as much freedom as possible: Please feel free to skip Sections that don't seem central to you/that don't interest you.[3] 'Dip' in and out. Similarly, feel very free to ignore the footnotes, many of which contain rebuttals or asides which may be tangential to your concerns. Try to find within this book the book that works for you. That is all a philosophy book can be, after all, after Wittgenstein: a set of exercises whose therapeutic value will differ depending on where one is at, as compared to and contrasted with others. (This book has, I believe, a fairly strongly unified thrust, and I hope you will read it from start to finish; but I would *far* rather you read parts of it and profited from it than that you read all of it and were, ultimately, bored by it.)

Moreover; please don't expect this book to be something that it patently isn't (but which the main title might possibly have led you to think it is). This book is not an exegetical examination of Wittgenstein's remarks on science or the specific sciences, and nor is it even an examination of his brilliant and peculiar relationship to the scientific ideal or image. Still less does it consider Wittgenstein's own scientific heritage and scientific and engineering investigations.

What this book *is* for is above all for you and me to get in view some important putative differences between what we call natural and human sciences. (Part 2 of the book focuses on philosophical and *social/human* thought and action, in the broadest sense of those words.) This book employs Wittgenstein's and Wittgensteinian 'methodology' to wonder around the sciences with. And it relies quite heavily, in the course of doing so, on a pair of key 'surrogates' for Wittgenstein on whom I have previously published book-length works: Thomas Kuhn and Peter Winch, on my 'charitable' (broadly Wittgensteinian) interpretations of them.

What unifies the book above all is the *sensibility* which it aims to inherit and manifest. A sensibility that 'reads into' Wittgenstein's brilliant 'surrogates', Winch and Kuhn, the realistic *spirit* [4] of the therapeutic ('New',[5] and later-Bakerian) Wittgenstein. The book also at times employs some of the method (of paying attention in detail to unaware narrow projections of metaphor, and of developing a thoroughly 'embodied', non-metaphysical 'realism') of Lakoff and Johnson's

2 Henceforth *PI* indicates Wittgenstein's *Philosophical Investigations*.

3 For example: just skip 1.2, if you are not in the grip of nor interested in standard readings of Kuhn.

4 Diamond's (1991) term, following Wittgenstein himself.

5 See Crary and Read (2000).

(1999) and some of the sensibility of Iain McGilchrist.[6] McGilchrist's (2009) is especially relevant, in that it beautifully offers, implicitly (and at times I draw this out somewhat, in what follows in the present work), a way of hearing a difference between two profoundly-valid ways of hearing the world: roughly, those of normal science (viz. the left hemisphere of the brain), and those of the synoptic human being (the right hemisphere). And crucially, it suggests, in a way utterly respectful of science itself (and indeed deeply *based in* contemporary neuroscience) that and how it is literally disastrous to attempt to reduce the latter to the former, as most visions of 'social science' or of *'les sciences humaines'* tacitly or explicitly *do*. (This is one grave example of what McGilchrist describes as happening right across Western culture, in modern times: the displacement of the holistic and human approach of the brain acting as nature intended by a more limited atomised model which, quintessentially, *models* [7] what it sees.[8])

The present work has the following overarching structure:

In Part 1 I *remind* (cf. *PI* 89, 127) the reader of our paradigms of science: natural sciences. I outline briefly, that is, some key features of a broadly Wittgensteinian (Kuhnian, on my reading of Kuhn)[9] perspicuous presentation of science, and attempt to eliminate some key misunderstandings of that presentation and of natural science. Crucially, I show in some detail that and how 'Incommensurabilism' is *not* tantamount to Relativism; it is rather about understanding the *depth* of the difference between the two cultures being compared, and understanding thus how easy it is to fail to *see* one, through seeing it only through the eyes of the other culture. (This is a lesson that is of crucial import when one comes to subjects

---

6 I do *not* here offer any defence (or indeed any significant explication) of Lakoff and Johnson, or of McGilchrist, as I *do* offer these of Kuhn and Winch. I simply assume their general usefulness, and I accentuate the positive in their work for my purposes. If you are someone who doesn't find their work reliable, then you can just treat the moments when I refer to them here as themselves 'just' the developing of an appealing and possibly-useful *metaphor(s)*. Such moments are not a substantive part of my argument in this book. They are intended as illustrative. (For my own *critique* of McGilchrist, see my Review of his (2009), forthcoming in *Phenomenology and Cognitive Science.)*

7 Cf. my argument in 1.5, below.

8 See especially the final chapter of McGilchrist (2009), which is entitled "The master betrayed". (The 'master' is the right hemisphere, who has been suborned by his 'emissary', the left hemisphere.)

9 See Sharrock and Read (2002, 2003). The latter is reprinted with some updating as Section 1.5, below. In any case, in the final analysis the question of whether or not my Wittgensteinian interpretation of Kuhn is right is less important than the substantive philosophy-of-the-sciences question of the methodological use to which my Kuhn, this new Wittgensteinian Kuhn, can be and is below put. In the end, philosophy and methodology always trump exegesis, unless one is content to be *merely* a scholar. (See also the close of this Preface, below, and the Concluding Summary to the book, for development of this point.)

having human understanding as a key part *of their subject matter*: i.e. 'the human sciences'.) What Part 1 is supposed to give one is a better sense of the nature of normal science, and of its occasional transmogrification into extraordinary science. Ultimately, as befits a therapeutic work, *the reader has to come for themselves* to a decision as to how best to employ this value-laden term, 'science', and on what features of its employment in the domain in which its employment is relatively unproblematic are worth highlighting, emphasising, holding onto.

Then, in Part 2, I offer successive cases of putative sciences from the study of the human world, conceived not as simply and exhaustively biologically-chemically-physically comprehensible. The cases considered are most of the strongest cases of putative human sciences: economics, psychology and psychiatry, cognitive science. If even *their* status as 'real' sciences turns out to be mediocre, then that will be telling (It will *a fortiori* suggest worries concerning the alleged scientificity of sociology, anthropology, etc.). Here is where the reader's decision as to the most helpful or otherwise employment of terms such as 'science' will come to the fore. Your Wittgenstein, after mine, is here put to work among putatively scientific disciplines.

For the key to the book is to understand that these cases of human studies etc. are offered in broadly the same spirit as the 'cases' of language considered by Wittgenstein in the first hundred or so sections especially of the *Philosophical Investigations*. NOT as quasi-scientific models, NOT as approximations – in fact, on the contrary. (The prejudice that successful sciences (or, better: successful disciplines, successful subjects) would be things that deliberately modelled themselves on the natural sciences is exactly a prejudice that, following Peter Winch and Harold Garfinkel,[10] I want to hang a large question-mark over.) The key to Wittgenstein's liberatory/therapeutic method in the overture and opening movements of the *Philosophical Investigations* is to come to understand that the onus is being put on *you*, the reader, to decide whether these 'cases' are language or not. Is *this* enough to merit such-and-such a 'language-game' truly being regarded, on reflection, as language, or not? Or *this*? Or *that*? Eventually, one comes to think that perhaps quite a lot is needed in order for this term, language *simpliciter*, to be actually worth applying to something – not just the most primitive forms of language/of systems of communication, etc. . Indeed, 'Instead of producing something common to all that we call language, I am saying that these phenomena … are *related* to one another in many different ways. And it is because of this relationship, or these relationships, that we call them all language' (Wittgenstein, *PI* 65, underlining added).[11]

Somewhat similarly, the 'cases' I examine in (Part 2 of) this book are presented in such a way as to make one wonder whether a good deal more is needed to

10 See Hutchinson, Read, Sharrock (2008) for exposition of Winch and Garfinkel in this direction. See also the Lecture Transcripts, below, where I question the 'programmatic' ambitions of social science etc.

11 I defend the reading of *PI* 65 that is implied here in Read (2005).

justify the honorific term 'science' than most 'defenders' of/apologists for human or economic or social science seem to think. And also in such a way as to open up other (previously more or less occluded) possibilities for one's consideration: e.g., that the discipline in question has welcome features on its/their own merits that are not well-described as scientific, or that the discipline in question is rather more continuous with abilities of common life that are only revisionistically – and as a result of over-generalisation – assimilable to *scientific* abilities or practices. But the decision is up to us / up to you. It's not up to *me*. And what I present here certainly does not *define* science – and so, *a fortiori,* does not exclude 'human science' etc. *by definition*: As if a *definition* hereabouts could be of any real *use* to us (cf. *PI* 3, 79, 182). Rather, the reader is presented (in Part 1) with a reminder of some clear cases of science, and asked to reflect upon how they are related to one another such as to be worth calling 'science'; and then the reader is presented with some less clear cases, that the reader can gradually come to decide whether or not to call by the same name, and thus learns more (from herself) about the 'relations' that exist between various forms of inquiry. The result of the whole book should be a greater clarity on the reader's part on how to apply and how to understand terms such as 'science' to best effect, a clarity that will be far more useful and serious and hard-won than any definition could ever be.

So: This book does not seek to 'demarcate' science from non-science. It seeks to allow the reader to reach a greater clarity about the various epistemological etc. enterprises that human beings engage in, such that the felt *need* for a 'demarcation criterion' tends to evaporate. In that sense, it is (or: aims to be) a wholly 'therapeutic work.' Catch its author holding any dogmatic opinions, and he will instantly give them up. For it doesn't much matter whether you do decide in the end to use the *term* 'language' to talk about (e.g.) Wittgenstein's 'language-games' or not: 'it is as you please' (*PI* 14). Wittgenstein's broadly therapeutic conception of philosophy is such that what matters is your coming to self-awareness about the factors that lead you to do so (or not): 'Say what you choose, so long as it does not prevent you from seeing how things are. (And when you see, there is a good deal that you will not say.)' (*PI* 79, translation emended).

My aim in this book is the same: to put the onus on you: to give the power to you to determine whether or not calling disciplines such as sociology or economics or linguistics or what-have-you sciences will actually be fruitful, be *satisfying*. And, to see clearly the contemporary character(s) of those disciplines, *no matter what one calls them*, no matter which 'Faculty' one assimilates them to. Wittgenstein's philosophy is (about) liberation from the compulsion to believe that such and such MUST be the case; it is (about) no longer being unawarely *attached* to 'pictures'. It aims to provide a genuine and (as) lasting (as possible) means of satisfying one, by releasing one from incompatible desires – e.g. the desire both to regard a certain subject as a science and to acknowledge features of it which present obstacles to taking the first desire seriously.

The aim of the game is therefore not word-policing, but intellectual liberation, satisfaction, and clarity.

If I ever appear to violate the aim and ambition delineated above in the pages that follow, then please give generously of yourself: try if possible to interpret what you read there in the spirit of this Preface. For it is of *no* philosophical force what *opinion* you or I or anyone else has about whether sociology or economics or linguistics or what-have-you 'really is' a science (cf. (Wittgenstein: 1976: 102–3, 183–4, 14). What matters is the way that we can learn about ourselves (and about each other; and of course about the ways we study each other) when we think through these various cases. Such that we can go on, less likely than before to succumb to temptations to scientise where we shouldn't, or to apply (or to withhold) this that is widely-regarded as the greatest of labels of contemporary praise ('science!') where we shouldn't.

In sum: What do we want to mean, and what can and do we actually mean, when we call something a 'science'? And when and where ought we to honestly admit that such appellation is more trouble than it is worth?

Let's find out.

But before we get underway, an important word on philosophy and exegesis.[12] I have enjoyed (if that is the right word) a sustained series of debates in print with Steve Fuller over his criticisms of Kuhn's philosophy of science. In his latest very lengthy critique of my criticisms of him on Kuhn, Fuller (2005) makes a number of disastrous interpretive bloopers and intellectual mis-moves, which I shan't trouble to try to correct. The very final page of his paper is however of some worth, and of interest in relation to our present topic. Fuller suggests that '[Read ought to take] more credit for his own arguments instead of performing feats of ventriloquism for Kuhnenstein' (Fuller: 2005: 497).

I accept this point. The interpretation of Kuhn offered in Sharrock and Read (2002) may well be wrong. I am inclined to think now that it is in fact not altogether unlikely that our 'therapeutic/Wittgensteinian' Kuhn is a partial reading, and that the more 'modest' (less Wittgensteinian) Kuhn of Jouni Kuukkanen or of Bojana Mladenovic may be truer to the historical Kuhn.

The real point about my Kuhn is that he is in most key respects *right*. (I include here his endeavour to be a basically therapeutic figure, not a metaphysician or quasi-scientist with a philosophically-controversial positive account). In other words: it matters little whether my (-and-Sharrock's) interpretation is true (and any such judgement is always only ever an idealisation, in any case – because philosophers/thinkers change; their texts are never 100% self-consistent; etc.); what really matters is its usefulness. What really matters is a sound philosophy/methodology of the sciences.

And the same applies to my Winch.

This book seeks to apply Wittgenstein to thinking about the sciences, especially (this is where the main 'pay-off' of the book lies) 'the human sciences'. Kuhn

12 My particular thanks to Bojana Mladenovic, for getting me to see how important this word may be.

and Winch are, as I've already noted, used at times within it as 'surrogates' for Wittgenstein, in this thinking, in this application. Whether I get Kuhn or Winch – or even Wittgenstein – interpretively right, is of less significance than whether I offer the reader a useful therapy that enables her to figure out for herself what vexes her *vis a vis* 'the sciences' and how to overcome that vexation.

Where the approach offered here differs from these figures, I am happy to take credit for the difference. I believe this book to be *profoundly* in the spirit of Wittgenstein (and of Kuhn and Winch); but, if ever forced to choose, I should give up that claim, and stick with the 'claims' (such as they are) that follow.

For the subject-matter of Part 2 of this book is in the end far too crucial to be contingent upon a claim of influence or philosophical historiography. What follows involves nothing less than an offering to the reader of the opportunity to liberate herself from the strong draw to scientism, where that draw has wreaked most mischief. Setting Wittgenstein to work among the 'human sciences' has a strong impact upon one's ongoing understanding of the disciplines in question. And in some cases has important consequences for science policy and even for government policy more generally, in relation to fields such as economics and psychiatry.

To sum up then this central point about my philosophy of exegesis: I offer the reader here a possibility to see Wittgenstein, Kuhn and Winch as I do; but if someone persuaded me (or if you, reader, are at some point somehow persuaded or convinced) that my *interpretation* was mistaken, I would still stick to the claims, methods, ideas that I derived/generated while reading Wittgenstein, Kuhn and Winch, because I think that these claims, methods, ideas are the right ones to have. The best interpretation is the *philosophically* best interpretation, (more or less) consistent with the text.

Obviously I do not think that this is the *only* responsible way to read other philosophers; but I want to argue that it *is* generally the *best* way to read them, for anyone interested in more than scholarly dust. I won't try to defend this view further here, in this already-rather-long Preface; but I hope that, at the end of having worked through the various Sections of the present work, you too will be convinced of it.

By their fruits …

Acknowledgements

This book discharges a debt that I incurred in the 'Afterword: Further prospects for applying Wittgenstein' to my *Applying Wittgenstein*: see pp.140-1. My greatest debt in the writing of this book is to my Editor, Simon Summers. Important parts of the conception of the *structure* of the book, for instance, are due to Simon. My thanks to him for his tireless work – and his thinking. The Editor of this new *series*, Phil Hutchinson, suggested I write this book, and has been a wonderful interlocutor with regard to the content of many Sections of the book (and also with regard to its overall architectonic). I also owe a huge debt to Wes Sharrock, whose thinking has greatly-influenced me in relation to the subject-matters of both Parts 1 and 2 of this book. In particular, Sections 1.2 and 1.3 emerged directly from the thinking that Wes and I did together for our *Kuhn* book, and, more crucially still, Wes generously allowed me to re-publish the article that is the basis for Section 1.5 (an article co-authored by the two of us) here. Bojana Mladenovic and Angus Ross (especially), Jouni Kuukkanen, Nigel Pleasants, Gavin Kitching, Davide Rizza, and Louis Sass kindly read the whole manuscript or major chunks of it for me, and have offered me a wealth of comments on it that have greatly improved it. In some cases, I have borrowed verbal formulations from them and these now find a place in the finished text. My deep thanks to them. Obviously, all remaining infelicities or errors are my own…

My students over the years have played an important role in the testing out of the material in and of this book. Naturally, I would particularly like to thank the Masters students who participated in the class the 'lecture transcripts' of which make I think a unique 'opening' to this book, and whose voices are preserved here in the questions etc. that they asked/offered in the course of that class. I'd like to thank the late Thomas Kuhn and the late Peter Winch; the contacts/meetings I had with them back when they were alive (though I wish there had been many more, and that they had lived longer to continue their great work) have stayed with me as direct influences, and have contributed to my way of taking them as 'emissaries' of Wittgenstein's, here. And I'd like to thank the late Nelson Goodman; again, the contact I had with him when he was alive (again, too little) contributed to my desire to write a book with the title *'Wittgenstein among the Sciences'*, a title intended as (among other things) a tribute to the range of his (Goodman's) influence. Also I'd like to thank Iain McGilchrist (who thankfully is very much *alive*, and vital) for electronic conversations that have contributed to the latest way I aim to shape the material which readers can find herein.

I wish to thank the following for permission to include the following previously published material in the present volume: Palgrave Macmillan, for Read, R.

(2009) 'Extreme Aversive Emotions: A Wittgensteinian Approach to Dread', in *Wittgensteinian Perspectives: Emotions and Understanding,* Gustaffson et al (Eds), London: Palgrave Macmillan. SAGE, for Read, R. (2008), 'The 'hard' problem of consciousness is continually reproduced and made harder by all attempts to solve it', first published in *Theory, Culture and Society*, 25(2). Taylor and Francis, for Sharrock, W & Read, R. (2003) 'Does Thomas Kuhn have a 'Model' of science' in *Social Epistemology* (17) 2/3. Jerry Goodenough, for Read, R (2004) *Kuhn: A Wittgenstein of the Sciences?,* originally included in *UEA Papers in Philosophy* (15).

There are many others who deserve thanks for helping in getting particular portions of this book to where it is; most of them are thanked in individual footnotes, often at the start or end of particular Sections. Anyone who I have left out – and there certainly will be some -- I apologise to now. Finally, thanks to my UEA School of Philosophy colleagues for their help in encouraging me to take time to write this book; and to Juliette, as always, and for always.

Rupert Read, Norwich.

Editor's Introduction

The conception of philosophy which is defended and deployed by Rupert Read is rooted in an increasingly influential family of interpretations of the philosophy of Ludwig Wittgenstein. The unifying theme amongst these interpretations is an understanding of Wittgenstein as a thinker with a 'resolute' or 'therapeutic' conception of philosophical activity; this activity should not be characterised as having technical, theoretical or metaphysical aspects and aims. This conception has informed the author's ongoing philosophical practice in recent years, and the present volume extends this into new territory.

The central question addressed by the book might usefully be said to be the following one: 'Is a good deal more needed to justify the honorific term 'science' than most defenders of human, economic or social science think?' The answer is not sought in any strict demarcation of the scientific from the non-scientific: rather, the reader is encouraged to reach for herself a greater clarity about the various enterprises that human beings engage in, such that the felt need for a demarcation criterion tends to evaporate. The book takes up the challenge of inviting the reader to such reflection by way of a searching examination of the actual nature of science itself, and a subsequent investigation of successive cases of putatively scientific disciplines.

Whilst there have already been a number of explicit attempts to understand the implications of Wittgenstein's thought in the context of the methodology of the social sciences, those attempts have all too often taken Wittgenstein to be putting forward substantive doctrines or theses. This monograph seeks to challenge such readings.[1] As an attempt to investigate the relation between reflections on the methodology of science, on the one hand, and the natural and social sciences themselves, on the other, the ambitions of the present book can be compared with Douglas and Hull (1992) and Gunnell (1998), both impressive and important undertakings. However, where the former volume represents an effort to understand Goodman's distinctive contribution, and the latter engages with the thought of thinkers as diverse as Cavell, Rorty and Foucault, the current volume's distinctively therapeutic approach aligns itself most intimately with the work of Peter Winch, and with Read's own previously authored and co-authored material.[2]

1 Such readings include McGinn (1984) and Bloor (1997).

2 The most explicit connection obtaining between the current volume and Read's own previously published work is with Sharrock and Read (2002) and with Hutchinson, Read and Sharrock (2008). The 'therapeutic' strategy in the former volume runs parallel to that developed in Part 1 of the present work; the present material on Kuhn, which is entirely Read's own (with the exception of section 1.5, originally co-authored with Sharrock)

The book begins with transcriptions of a series of Read's post-graduate lectures/seminars, originally delivered in 2007, which I attended, and which proved to be a dynamic forum for discussing and debating the difficult questions and deeply-felt concerns motivating the current volume. Part 1 of the book aims to establish a secure understanding of how we ought to understand what science is, from paradigm-uses of the term (i.e. the natural sciences). Part 2 then looks at a range of social and human sciences, including anthropology, psychiatry, economics, and cognitive science, in order to test out the degree to which it is helpful to try to apply the scientific method within them, and to treat these disciplines as sciences.

In Part 1, the principal focus is on the parallels between Wittgenstein's methods and those of Thomas Kuhn. Firstly, in §1.1 the author focuses on some key passages of Kuhn's *The Structure of Scientific Revolutions*, notably those passages which appear to present a problem for the 'therapeutic' interpretation of Kuhn. Read shows how, interpreted in the spirit of the therapeutic Wittgenstein, Kuhn can be said to have far fewer dogmatic theoretical commitments than most of his opponents have presumed. Thus, there is a real case for regarding Kuhn as a Wittgensteinian figure, in relation to the sciences. This sets the stage for Part 2, in which these concerns are explored in relation to some putatively 'human' sciences.

In Section §1.2, a powerful and pervasive interpretation of Kuhn's conception of 'paradigm-shifts', 'incommensurability' and 'world changes' is presented and assessed. From the vantage-point of this standard interpretation, Kuhn's attempts to understand scientific change 'from the inside' appear to leave him vulnerable to the charges of relativism and of advancing self-defeating claims. If Kuhn is read and understood in this manner, as simply another philosopher of science putting forward a set of falsifiable claims, then even his most sympathetic readers will struggle to defend him against the accusation that his 'theory', and the claims which constitute it, undercut themselves by standing *outside* the 'view from within' which they seem, paradoxically, to endorse. In response to this picture, §1.3 presents a challenge to the standard reading, proposing that Kuhn is attempting to enable us, his readers, to begin to understand periods of science in which we do not participate and, indeed, in which we could not possibly participate. Importantly, the disappearance of a period, or an activity, does not deprive us completely of the use of the language in which it is interwoven and which is interwoven in it, and as such is not reducible to 'meaning-incommensurability'.

§1.4 explores the possibility of reading into Kuhn a rather different account of incommensurability. Is there, in Kuhn, a notion of the absence of a common measure not only of what is meant but also of what is *believed in* – an incommensurability of *values*? Kuhn's suggestion of an incommensurability of values is not independent of his suggestion of incommensurability somewhere in the vicinity of meaning (though, as §1.3 takes pains to show, not 'semantic incommensurability' as usually assumed): rather, Kuhn's ideas on these matters

---

proposes to diagnose what goes wrong in standard interpretations of Kuhn, and the parallel between Wittgenstein and Kuhn is developed in detail.

stand or fall together. To assume that what is meant by 'incommensurability of value' could be quite other than 'incommensurability of meaning' would be to beg a range of interesting questions concerning communities and their differences, questions which are among Kuhn's real concerns.

§1.5 (originally co-authored with Wes Sharrock) questions the interpretation of Kuhn as providing a 'model' of science. Whilst acknowledging the simple schematic of terms put forward in SSR ('exemplar', 'normal science' 'revolution', 'incommensurability), it is proposed that such terms are not best understood as undergirding a model, but rather as heuristic devices, or *objects of comparison*, employed in the service of bringing to prominence key aspects of the 'scientific method' which may otherwise be hard to notice. If one imagines that Kuhn is providing a *model* – a reification of the features of a given period of science into a generalised model – one may easily take Kuhn to be engaging in a practice which fails to respect its own constraints. If such is Kuhn's project, it offers an outmoded or incomplete model at best, and, worse, risks falling into internal contradictions. Setting himself against this misleading and damaging interpretation, Read puts forward the alternative view that Kuhn is best understood as challenging late empiricist conceptions of scientific 'growth' and as offering a sustained and profound conceptual inquiry into the nature of scientific change.

In summary, whilst the 'standard' picture leaves us with a reading of Kuhn as a thinker who *appears* to fall into contradiction and relativism, the overarching aim of Part 1 is to oppose this standard picture, exploring the striking parallels between Wittgenstein and Kuhn. In so doing, the author seeks to persuade the reader that in reading Kuhn from a therapeutic perspective, one is perhaps most faithfully capturing Kuhn's considered view. By getting clear about incommensurability, the central issue in Kuhn's account of science, one puts oneself in the best position to gain a clear perspective on the putatively human sciences, within which incommensurability is often a live concern.

Part 1 puts forward an alternative and more sympathetic reading of Kuhn than standard readings permit, by bringing to light the crucial parallels between Wittgenstein and Kuhn, whereby both thinkers are understood as enabling us, their readers, to gain a more perspicuous view of the putative 'objects' of our investigations. Kuhn's and Wittgenstein's aims can thus be understood as therapeutically motivated attempts to show how they and their readers might overcome the metaphysical predilections which accompany philosophical and methodological theorising, by enabling the reader to see just what those predilections are.

In Part 2, after a short 'Inter-Section' that seeks to join the themes of Part 1 with those to come, Read puts forward successive cases of putative 'sciences' from the study of the human world. In each case, by applying the therapeutic thinking common to Wittgenstein and, later, Winch, Read argues that these putative 'sciences' are *not* best conceived as simply and exhaustively biologically, chemically, or physically comprehensible. The final judgement is left to the reader, who is asked to judge for herself whether 'science', as she wishes to understand it,

and as it is worth understanding, is present in these cases. In undertaking this task, Read does not shirk the hardest cases. The putative sciences subject to investigation are, principally, psychiatry, psychology, cognitive science and economics. If there are any human sciences, these are surely amongst the strongest candidates.

Section 2.1 introduces and reflects upon central themes in Peter Winch's seminal work, *The Idea of a Social Science and its Relation to Philosophy*, emphasising that Winch is best understood as questioning the *very idea* of social science, and of warning us against the inclination to interpret social practices, in all their vast complexity and subtlety, along scientistic lines.[3]

In §2.2, this Winchian method is applied to the 'hard case' of schizophrenia, specifically to the work of Louis Sass and his attempts to provide a Wittgensteinian interpretation of schizophrenia. The *interpretive* option which Sass puts forward is contrasted with Winch's (broadly) *descriptive* option, and it is argued that the latter resists the urge to interpret where interpretation is not possible, A 'taxonomy of interpretive options,' more generally applicable to cases where understanding is absent and is (apparently) called for in the human / social world, is presented and developed.

§2.3 offers a characterisation of the extreme aversive emotion of 'dread', whilst also calling into question any standard attempt to *make* such a characterisation, drawing upon Wittgenstein and Winch in an effort to persuade the reader of the hazards incumbent upon insufficiently radical attempts to comprehend extreme aversive emotions. In the course of §2.2 and §2.3, serious questions are raised for psychiatry's allegedly scientific status.

In Section §2.4, the method essayed in the preceding several Sections is applied to another key test-case, the conception of economics set forward in Friedman's epochal work, *The Methodology of Positive Economics*, showing that Friedman *assumes* the very model of human action/consciousness that the alleged economic laws he sets forward are supposed to occlude. In this way, Friedman smuggles into his guiding conception of natural science the tendentious vision of social, economic and human science that he subsequently foists upon his readers, begging the question against the view that there may be a difference in kind between the subject matter of the human/social 'sciences' and the natural sciences. By means of a close examination of key passages in Friedman's text, Read exposes the assumptions which give rise to Friedman's attempted justification of the claim that economics is a science in the same way that physics or biology are.

In §2.5, Wittgenstein is put further to work among the putatively 'cognitive' sciences, and the therapeutic method applied to the so-called 'hard' problem of consciousness, arguing that the problem, along with the broader 'mind-body' problem itself, is kept alive because its 'solution' is placed tantalisingly out of cognitive and epistemic reach in contemporary discussions. The therapeutic approach to the hard problem is to argue that this 'separation strategy', and the

3 This Section thus sets up the important role of Winch in Part 2, as a background figure, analogous to the role played by Kuhn in Part 1.

elusiveness of a solution which will satisfy us, bringing about an end to our theorising on the matter, is in fact a structural feature of the way the problem has hitherto been approached.

The book closes with a concluding summary by the author, followed by a short interview which I conducted with Rupert Read, during which we clarify some points and discuss a number of potentially controversial issues arising from the book.

Simon Summers

Lecture Transcripts: 'Theories and Non-theories of the Human Sciences'

Week (1) Introduction

We begin this course by looking briefly at natural science and we ask the following initial question: is 'natural' science the model on which 'social science' has been based? This is a question which it is very natural to answer with the answer 'Yes?' My own view is that that answer is roughly correct.

Then we ask the question: is that okay? Is it alright that we have built disciplines of sociology, political science and economics etc on the model of science? What are the consequences of our having done so? I will take you – in some detail – through some of the people who have argued most strenuously that there is something really problematic about conceptualising the study of the social world on the model of the study of the natural world, Peter Winch in particular.

It looks like the social sciences (what most of you are doing/studying) have been modelled on successful enterprises in natural science. Is that okay? Are there alternative ways of doing social 'science' which do not fall prey to some of the difficulties associated with the natural scientific model as a method of social enquiry? Indeed, are there ways of doing social enquiry which are not social *science* at all which should be preferred?

This leads naturally to a question which is in my opinion an even more interesting one: are there methods of doing social enquiry which are deservedly peculiar to the academic world or disciplines such as sociology, economics and political science *at all*, or are the methods that we use that actually work most effectively (in terms of showing ourselves something about ourselves or the social world) in fact the methods of 'ordinary people'? To put it slightly bluntly, in a way which I hope will make you reflect upon what you are actually doing and studying here: if you study these (social scientific) disciplines successfully, are you actually an expert in anything that ordinary people – people who have not studied those disciplines – are not *themselves* experts in? As I say, I think this is really quite an interesting and important question, and it is one we will be addressing.

A first pass at the question would be that there is *of course* something worth calling expertise in matters non-scientific: in various hermeneutic traditions, for example; in history of a period; in art-appreciation. A competent, even college-educated person does not know everything that an expert knows; for example, a

musician (especially a musically-educated one) hears music differently than an ordinary pleasure-seeking concert-goer. So: Sociologists could perhaps be experts in society (in sociology) without sociology being a 'science' under some definition of science.

But great care is needed hereabouts. For while undoubtedly an intelligent reader reads novels differently than a literature PhD does, does the latter inevitably read novels better? I think not: in fact, I think that what is called 'expertise' can sometimes get *in the way,* in a case like this, of actually being able to read (in this case, the novel). Are there experts in philosophy? Yes and no. There is a sense in which I know a lot more philosophy than you do. But there is another sense in which I truly believe, following Socrates and Wittgenstein, that *I don't* know *anything about philosophy* that you or anyone else doesn't know.

But before we go further with this, let us now switch for a moment, away from thinking of the social world, and towards thinking of us as part of the natural/physical/biological world. In the course of addressing the question just raised (about expertise), I will offer you some good reason(s) for believing that 'social science' is in fact mostly just the knowledge that ordinary competent social actors always already have anyway. If that case gets proved, then does that mean that we are committed to some form of idealism, and are abstracting away from the material, physical natural nature of human beings? Not necessarily. Let me explain:

An important and often neglected theme when we think about science and social science is the question of our place in the natural world, considered from an ecological or an environmental point of view. Thinking about us as fundamentally part of the natural world is the most natural way to think about human beings from a natural scientific point of view, i.e. as biological/physical entities, *and* as part of ecosystems. How (if at all) does/must this contrast with a conception of us as fundamentally 'social' or indeed fundamentally *human* beings (as political animals or as *Homo Economicus*, for example?)

If one reacts *against* the way of thinking about human beings implicit in ideas such as *Homo Economicus,* namely if one thinks there are important aspects of human life which are *not* easily captured through the lens of scientific enquiry, then it is perhaps easy to lose sight of something which it is arguably *extremely* important to keep in clear sight: that *whatever* we are as social beings, we *are* beings embedded through and through in our physical and biological environment. Without that environment, it doesn't make sense to think of us existing *at all* unless one is an extreme idealist or solipsist who doesn't really believe that the world (really) exists. (That is, of course, a position that some philosophers take up, which we shall not dwell upon here.) The most natural way to think of the natural world is: 'it is fundamental; and it is just there.' We might think that perhaps it just doesn't very naturally open itself up to our understanding. That it requires detailed and obscure scientific work, to render it comprehensible.

A *Realist* approach to thinking about the natural world says that the world is just exactly as it is, completely independently of us. Our scientific endeavours are our attempts to latch on to or to 'capture' that reality, though we may, of course,

fall short of capturing it. The main alternative approach is Anti-Realism, which has many variations (instrumentalism, positivism, empiricism etc). What all these latter ideas have in common, at least *when they are translated into practice*, is that they suggest that we cannot say (cannot know) anything more about the natural world than what can be measured of it. (Some anti-realist ideas even question whether there *is* anything more to the natural world than what we can measure in it.)

Realism is an attitude to the natural world and to science which suggests that science is an attempt to capture the nature of reality in-itself, and that our attempts to capture it may fall fundamentally short of that reality. In contrast, Anti-Realism implies that reality has to be measurable: whatever isn't measurable isn't real, or might as well not be. These ideas, which seem so different from each other, may turn out not to be so different when it comes to their actual application to the question of what *social* science is or could be, for example. A Realist would say: 'the social world is just what it is, irrespective of our 'understanding' of it: how it is may completely outstrip that understanding.' An Anti-Realist's position would be: 'the social world is (as good as) not real, except insofar as it is measurable. If it cannot be measured and quantified, it can't be spoken about, and perhaps does not even exist at all.' This is influentially expressed in the history of social science through ideas such as behaviourism. Some forms of behaviourism, especially those that have actually been realised as scientific research programmes, say that whatever is not observable, measurable or quantifiable behaviour does not really exist (or at least that we can't really say anything about it). What Realist and Anti-Realism as just explicated have (very much) *in common* is that they fundamentally presuppose the scientific attitude, or an attitude that leads to being (or *looking*) scientific as regards their subject matter.

The fundamental questions I will be asking are: is there *something missing* from these approaches, if we want to take seriously the fact that we are thinking about human beings and the human/social world? Is there some sense in which, if we try to think of the social world as in some fundamental way not fully open to our understanding, we are 'missing' it? Isn't there something very *odd* about the idea that the social world is *not* open to our understanding? Don't we start in the social world as *insiders* far more fundamentally than in the natural world? (For the social world is truly, ongoingly *made* by us, in a way that the natural world isn't.) But, if we do, is this 'insiderhood' *anything like* what anti-realists have in mind, if they take what is not scientifically measurable as not real? Being an *insider* to something could in fact be suggested to be fundamentally antithetical to having a *scientific* attitude or approach toward it (which demands outsiderhood). We live and experience it: we are not alien to it. In fact: We start to see now how such insider-hood need not be anything whatsoever to do with a scientific outlook (and thus need not be hand-in-glove with any form of 'anti-realism'). *On the contrary.*

Again: In thinking about such questions as these, it is vital to be careful (some have not been careful enough in the past, such as the idealists) to always acknowledge that the social world is nothing without its *embeddedness* in the natural world or, expressed another way, in the environment.

But let us focus more directly on the social world now: How can it possibly make any sense to try to take up a scientific attitude here? Why would we want to quantify and to begin to understand something as though it begins by 'being other,' if the object of our enquiry (we might claim) is something that is, from the very first instance, *open* to our understanding? Something that we are 'inside' and that is 'inside' us, something that is closer than close?

If you want to understand what a 'bus queue' is, for example: are you in a good position to do so if your attitude to it is to treat it as a 'quasi-natural phenomenon,' to be approached without any pre-conceptions at all? Or perhaps your approach is to quantify: to find out how many people are in the queue, how many minutes has it been there etc. … Isn't the way that we ought to approach the question of thinking about what a queue is rather more like trying to understanding *what it means* for somebody to be in a queue? Is somebody in a queue if they don't know that they are in a queue, for example?

If this *is* the right way to think about being in a queue, then that immediately suggests a fairly fundamental difference from the natural sciences or to an (allegedly) scientific approach to the natural world.

Student: Doesn't that happen in science as well? You establish categories and meanings which inform the theory?

Yes. Of course that's right … that's the kind of thing that Kuhn was deeply concerned with, and Popper as well: we'll come onto this in Lecture 2. But isn't there a fundamental difference, in that, in the case of natural science, the categories are our categories *as theorists*, whereas if you're talking about people in a queue, it looks like the world in question is through and through from the beginning *open* to our (and to their) understanding of it? We understand what it is like to be someone who is queuing – the people queuing knows that they are queuing – the categories *come from them (/us)*, right? Whereas, to put it a little colourfully, there isn't any question of an atom, a molecule, a mass or even a gene *knowing what it is doing.*

Student: Is consciousness required for 'society' in this sense? Can we say that ants are not truly social? And so: Is there an observer effect?

That's a really nice question, and I'd like you to think about it! Certainly, what I'm saying is that there is some reason at least to think that the answer to all three questions you've asked there *may* be 'Yes', and that *that* immediately poses some pretty serious problems for a whole host of perspectives in social science. Indeed, it poses problems most directly for the perspectives which have taken themselves to embody what it is to be scientific. For example, it immediately raises questions for behaviourism, or for positivism in economics. It should perhaps make us begin to worry about what we are committing ourselves to in committing to the pursuit of social science, and whether we want to think of ourselves as being on a secure path to science, or entitled to use the term science – even to regard it as *an honorific word*.

Why is that we think it would be a good thing for there to be a 'social science?' Obviously there is some sense in which science has been vastly successful. But: Aren't there other things that one can be successful at? Is there only one way that one can be successful as a human knower?

Week (2) The Philosophy and Methodology of Science.

When I say 'philosophy of science,' I mean in the first instance the philosophy of all those disciplines where it is very widely assumed that the term 'science' is an appropriate term to use of them.

But: the term 'science' does not translate uniformly into other languages. In German, for example, the closest equivalent, *Wissenschaft*, is used much more generally to refer to any kind of systematic study. In English the term has a somewhat specific meaning, and there is, I think, a good reason for that. A reason that ties in with the title of this lecture, to some degree …

What is science really about? What is the point? The philosopher Ian Hacking put it very well in saying that one has no proper account of science without the idea of 'finding out' about the natural world. Science involves discovering things, it involves knowing facts about things and it involves knowing how things work, and so on. If an account of what science is ends up being an account of 'words' or 'ideas' alone, Hacking suggests we don't have an account of science *at all*.

For example, there is an important sense in which mathematics is a borderline case. Is mathematics really a science? Does it involve 'finding out' about anything, or is it simply the working out of a series of ideas? This is a controversial question in the philosophy of mathematics. It is an interesting comparison-case to consider. There is another sense in which one might think that mathematics is the purest of the sciences, and other senses in which one might not think of it as a science at all. Does it, for example, have at its heart any finding out about how things are or how things stand in the world?

Let's turn our main attention to our paradigm cases of science. To what is obviously science, and to how philosophers have understood it. So we are talking about things like physics and chemistry. (In fact, I think a fairly useful definition of science is: Physics and things like that. This definition may look unsatisfactory to a 'scientific' eye. But such an eye is looking scientifically at what it is unobvious it makes sense to look scientifically at. Why think that science itself ought to be susceptible to a 'tight' definition, a definition without an open-ended similarity-rider?)

Here are the main three rival 'models' of science. The three main attempts, in the last century, to *take* what science is:

1. Model 1 (Empiricist/Positivist) The scientist accumulates observations/facts, then forms explanatory/predictive hypotheses. The scientist's task is to confirm or refute these hypotheses. (On this model, 'looking and seeing' can simply be done, and then hypotheses formed.)

2. Model 2 (Falsificationist) The scientist begins with a hypothesis (a framework). Explanations and predictions are made on the basis of the hypothesis. The task of the scientist is to refute the hypotheses. (On this model, 'looking and seeing' is done only through the lens of the hypothesis/framework).
3. The third (Kuhnian) account, is perhaps not a model, but rather a *picture* of science? According to this account, normal/everyday science is something like Model 1, and science in moments of crisis something like Model 2. (On this 'model', 'looking and seeing' can only be done through the lens of a framework so deep that it is not fully accessible to the scientist themselves (and thus cannot be in any sense called a hypothesis), and only becomes fully accessible when challenged, when in serious crisis (when no longer even just one thing …). Such frameworks, Kuhn famously called 'paradigms'. Kuhn argued that without a paradigm one cannot actually have science at all. Model 1 thus fails to understand even normal science, because it does not see how nothing at all can be seen except according to a paradigm. And Model 2 fails to understand even extraordinary science, because it does not see how there can be no extraordinary science except against the background provided by normal science and by the paradigm. Thus Kuhn can be seen as offering *for the first time* a potentially-successful account *of science as a whole.*)

Thus I suggest that Kuhn's work is a dialectical synthesis of the Logical Positivist/Empiricist and the Falsificationist (Popperian) models that preceded him. Kuhn takes seriously the actual nature of science, and its historical reality. This is the argument of my book, *Kuhn.*

You must form your own opinion (based on your reading, etc.) on whether or not you agree with me and Kuhn, or whether you are more attracted by one of the other two models, or indeed by something else altogether. The question for us, now, is: Do any of these accounts enable us to understand 'social science' properly?

Now, it might seem as if the Kuhnian account goes hand in hand with a standard 'scientific' apologia for/account of social science. For what Kuhn seems to say is: 'Get yourself a paradigm, and you have a science.' So perhaps that is all that social studies need, in order to become scientific, a paradigm.

But here, two important points:

1. Kuhn emphasises that a paradigm is not something that can be hunted for. Rather, you just work in your field, and if you are fortunate a paradigm *emerges* from your work.
2. A 'paradigm' doesn't just mean any framework you like. For Kuhn, it crucially includes what he calls 'exemplars': Examples of great work in a field *the exploration, replication and elaboration of which can constitute a research programme.*

Thus Kuhn does not lay down any prescription for the social studies to become scientific. In fact, paradigms are themselves social 'objects'. In order to understand paradigms and things like them (in order, that is, to achieve social understanding), *why presuppose that one needs already to have and to operate according to the kind of thing that scientists need and use and rely on*? In order to catch a gene, or whatever, one needs (according to Kuhn) a paradigm. But it might very well be that in order to catch a paradigm, what one needs is not anything much like a paradigm at all.

The question we will need therefore to come onto next is whether there are in fact good grounds to think that there is or will be such a thing as a social science. Put another way, and to return us finally to the title of this week's lecture: Is 'social science' actually a part of science at all? Does the philosophy of science carry with it any positive lessons at all for subjects such as Economics, Sociology, Anthropology, Political Science, Linguistics, Psychology etc?

Week (3) The Philosophy of 'Social Science'

A good place to begin today is through asking questions such as the following: 'is it reasonable to expect that there should be sciences of everything? – of all the kinds of 'things' that there are?' Does anybody have any views about that? Are there any things it would not be reasonable to expect there to be sciences of?

Student – Surely we can't measure things that have already happened, and are now cut off from us.

We *can* have palaeontology of dinosaurs… Now, when it comes to human history, Collingwood says that 'history is the history of thought:' an intriguing idealist view. Presumably, if it is possible to 'gather' other people's thoughts at all, it is possible to do so historically by referring to what they wrote and what they said, and so on. The issue would revolve around why there should or shouldn't be a 'scientific' way of doing that.

This immediately raises once again the question of why we would want to use the word 'science' here. What do we gain from it? What I suggest is that perhaps the best available way of approaching the question of understanding what it is for something to 'be' science is by asking whether the positivists' or Popper's or Kuhn's approach can be applied to the domain we are thinking about – the domain of history, for example. What would it *mean* to have a 'paradigm' (roughly in the sense in which Newton's laws constitute(d) a paradigm in natural science) in the study of history?

Let me give you some more extreme examples, to see whether they are helpful. So, let's take astronomy, the science of very large objects. Although many of them so far away that we can't directly carry out experiments on them, it is nevertheless possible to have paradigms for the study of those objects, which Ptolemy,

Copernicus and others have provided. These are paradigms for astronomers to 'fill in the details of' in terms of how the solar system and the universe beyond it is structured.

What is astronomy? The science of astronomical objects – very roughly, of everything beyond the earth, and of the earth considered as one more object which, from the point of view of other objects in the universe is something 'beyond them.' So, what about having a science, *astrology*, of the influence of these astronomical objects on us? Is that a reasonable thing to expect?

Student – It is tempting to say that astrology is speculative in a way that astronomy is not.

Yes – though I'd be tempted to say rather worse things about astrology than that it was speculative!

Student – Perhaps it would be okay if the science of astrology were restricted to a concern with the influence of those objects on things that we could measure.

Yes, that's an extremely interesting point. We could imagine a realistic, scientific astrology which *really did* try to figure out the influence of astronomical objects upon us – though its results (i.e. the effects, the influence) would be almost negligible, presumably.

If astrology *was* speculation, it wouldn't be so very badly off, would it? There are clearly speculative elements in *astronomy*. Isn't the point that astrology goes *totally* beyond that kind of speculation – so far beyond, that using the word 'speculation' is somehow misleading? Astrology as it actually exists in the world today involves the positing of *completely* unwarranted connections. It's a whole different way of seeing the world and the universe, a way which is, one is inclined to say, *completely* without foundation, and not merely a way which is 'not well-enough empirically tethered to justify the speculation.'

If we were to say that the problem with astrology was that it was speculative but that it *could* ultimately be proven correct, then it would appear to become a candidate-science, which it fairly clearly is not. Real scientists don't begin to take astrology seriously. It isn't a candidate. It isn't even in the game.

Student – Is that how we draw the paradigm line?

You could say something like that, yes. The idea of large but very remote objects influencing us is just the wrong sort of place to look for a science, when the idea is of their feasible, *actual* effects (e.g. gravitational effects). That raises the question: what are the right places to look? Defenders of science will say that in order to find out whether something can successfully be science or not, one needs to do research, to build up a body of work and so on. The suggestion I am making is

that *that* doesn't sufficiently capture the fact that astrology, for example, is simply a non-starter.

If a scientist were to say: 'okay, let's see what the astrologers come up with,' it would be an *odd* thing to say. To take seriously the idea that our lives could be *fundamentally* ruled by astronomical bodies, as genuine astrologers apparently believe, is something that I have no idea how to even begin to *test*.

Student – Isn't this beside the point. Astrology has no claim to be scientific – it's really just what's left over from ancient religions, isn't it?

That may well be right.

Though there have it would seem been people who have claimed that astrology *is* a science. All I am doing here is suggesting that this issue may be interesting to compare with the issue we are interested in, which is the issue of social science. The issue of whether we say 'okay, let's start our enquiry; let's just get on with it' or whether we say 'is that the right place to look *at all* for the construction of a scientific enterprise?' Is it the right kind of target?

Some say that the social studies are just what is left over from 'ancient' sciences, or philosophies. Here are some further possibilities. What about a science of objects and things that begin with the letter E? Would anybody take that as a reasonable basis for a science?'

Student – Doesn't it all depend on the hypothesis? We could for example say that all things beginning with the letter E have a 'magical' property?

Intriguing; but I'm not entirely sure that's right, because this now sounds very much like the case of astrology, doesn't it? In order to make it appear that you could get started, you *built in* the idea of magic, which seems to be an idea which is fundamentally incompatible with scientific investigation in the first place. Of course, there are various hypotheses we *could* come up with, but surely most of them would be so risible as to be immediately refuted (i.e. completely refuted before one even really *began*). Regarding our science of things beginning with the letter E, in order for it to even look as though we have a project which can get off the ground, it seems as though we have to build in *odd* assumptions which take us out of the 'game' of science – the idea that all things beginning with the letter E are linked together in some *mysterious* way, for example.

I'm not claiming a straight analogy. I'm *not* claiming outright that the social world is as heterogeneous as things that begin with the letter E. Nor am I claiming that the idea of there being 'things' which can be scientifically ordered in the social world is as strange as the idea of there being large remote objects which have a mysterious influence upon on. But I urge you to reflect upon these would-be sciences as reflective 'objects of comparison.' They can unveil *possibilities* which might not otherwise occur to you, *vis-à-vis* the standing of 'social science.' In

other words: I am trying to provoke in your mind at least the possibility of seeing society as not the right kind of 'thing' to approach scientifically.

Week (4) Peter Winch's Philosophy of 'Social Science'

Let me try to set the scene by pointing to some key moments of *The Idea of a Social Science and its relation to Philosophy (ISS),* firstly the Epigraph, a quote from Lessing:

> It may indeed be true that moral actions are always the same in themselves, however different may be the times and however different the societies in which they occur; but still, the same actions do not always have the same names, and it is unjust to give any action a different name from that which it used to bear in its own times and amongst its own people.

The closing section of this quote, I think, offers us a key to Winch's argument in this famous polemical little book. Lessing uses the word *unjust*, a term Winch doesn't employ very much, though it perhaps lurks in the background. Winch is interested in critiquing the *very idea* of social science, and is suggesting that 'social science' may occlude certain things from our fields of vision.

Social science may make it harder rather than easier to understand social actions, particular societies and so on. Lessing takes that one step further, in saying that it (social 'science') is not only congenitally inaccurate, but *wrong*: this is a question of justice. There is something *wrong*, Lessing claims, in giving any action a different name 'from that which it used to bear in its own times with its own people.' I think Winch would be inclined, in a certain important sense, to agree with that.

One reason for which I think he would agree relates to the idea of 'moral actions,' an idea Winch is very interested in: 'moral action' in the sense that *all* action is moral action. There is an effort in social science (typically) to 'strip out' the values of the observer in a way which is questionable, and perhaps absurd. There is an effort to pursue a 'positive' program of social science, and not a normative program. But what if in order to understand actions we *have to* take their moral, immoral or amoral dimensions seriously? And what if doing so inevitably related the morality or otherwise of the observed with that of the observer? This would tie the epistemological and the hermeneutic closely together with the moral and the justificatory in a way which would be intensely challenging to the 'idea of a social *science*.' (We'll return to this point in the next Lecture, in relation to a parable of mine, of the drowning children and the social scientist …)

If *all* social action (including, for example, the act of choosing not to interfere with an act but simply to observe it) has a moral dimension than what does that imply for the project of understanding *what action is* in a value-free way? There

is a lot to think about in Lessing's quote: it tells us a lot about where Winch wants (us) to go.

Student: If Winch is saying that we should judge according to how events were perceived 'in their time' is he saying for example that if we should not judge something such as the practice of Witch burning to be barbaric?

That is a very interesting question. Presumably, if the answer is 'yes,' it is deeply worrying. How can what Winch is saying be distinguished from or defended against such an interpretation?'

Student: The first half of Lessing's quote seems to attempt to head off that criticism. It seems that if something is wrong, it is wrong 'for all time.' Isn't Winch talking more about how the action is described?

Yes. Perhaps what we can start to say there is that Winch and Lessing are drawing attention to some important dimension of justice or morality that is relevant to social study or social understanding. Nevertheless, it remains the case that the task of understanding is *conceptually, ideally* distinct from the task of evaluating. For example, sometimes when we understand better (than we did before) the reason why someone has done something, *we come to evaluate the action more harshly.* For example, if you are first under the impression than somebody has unintentionally snubbed you, and then learn that they have *intentionally* snubbed you, you will understand their action better and will be very likely to evaluate it more harshly than you would have before you 'understood' it.

To understand witch-burning is not to forgive witch-burning. But you can neither forgive nor condemn 'witch-burning', if you know not what they do (they who do the burning).

Now let's turn to Winch's Preface.

Winch says: 'the understanding we already have is expressed in the concepts which constitute the form of the subject matter we are concerned with' (p.*x*). These concepts are considered, understood and debated by the people who are *themselves* 'the objects of study': this is *not* the case in natural science. There is no sense in which genes, atoms and forces have any 'truck' with the concepts by means of which they are understood. They are *entirely indifferent* – and even *that* is an underestimation of the difference … !

What Winch wants to do is to attack the prevalent conception of the relation between the social *studies*, philosophy and the natural sciences. The concepts of a people are a kind of philosophy already. They embody already a social understanding. Winch wants to suggest that there is a close relation between the social studies and philosophy. Exploring the very desire to have 'social science' is part of philosophy and (at least some of) what happens under the heading 'social science' is an attempt to pursue philosophy by other means.

Just as 'war is diplomacy by other means,' so 'social science' is philosophy by other means. It is not necessarily a good idea to go to war in this way … You will not necessarily get what you bargained for, or what you wanted. Winch thinks that what a great deal of what the social sciences are attempting to accomplish is covertly philosophical: philosophy must take into account the social dimension of human beings, and what we 'know' (what *we know our way about*) as students of society.

We might think of an analogy with the work of Thomas Kuhn here: social science perhaps needs philosophy only in 'times of crisis,' and after that ceases to be relevant. But Winch continues: 'in my view, it is *wrong* to say such a thing of sociology and its cognates (political science, economics etc).' Winch sees philosophy as enduringly relevant to the social studies. (Because they are *always* 'in crisis'?)

This is an overview of the problem, but what solutions does Winch actually propose?

Whose rules is 'the social scientist' concerned with? As we have already implied: surely not in the first place with the rules employed by the social scientific investigator as she conducts her investigation – surely it has to begin with 'them' (or, as it may be, 'us'), the human beings who are being studied. This is not the case in natural science. In natural science the 'rules' are only the rules governing the procedures of investigation in the science itself. (Genes, atoms etc don't obey rules; they merely are.) In Kuhn's terms, the rules can be understood (roughly) as structuring and organising the 'paradigm': crucially, following those rules will require a considerable degree of training, which leads to the following question: What is analogous to the training of the natural scientist, in terms of understanding the social world?'

Student: Is there a part of the training which is 'taught,' and a part which is 'lived?' By 'taught', I mean a body of knowledge which is passed on.

Good question. There is, for example, the vast amount of moral 'training' (I use the scare-quotes advisedly) we receive as children, and education is an explicit *and* an implicit learning of rules (amongst other things). What aspect(s) of social reality do we learn about as part of our education? Do we learn about the social world only in our social studies or sociology lessons? No, of course not: we learn how to 'follow the leader,' how to be honest, how to cheat, how obedient (or otherwise) people are, and so on. We learn these things also in the playground and even in our physics classes.

These reflections may be helpful is considering what the analogue is (and if there is really much of one at all) between training in the natural and social sciences. My point being: to some (limited) extent, most of one's life is training in natural science, but training in natural science is also and absolutely crucially something specific. Someone who just knows that apples tend to fall down can't yet be said to be trained in physics *at all.* With social science, I'd suggest to you, the boot is

on the other foot: one's life is one's main training, and specific academic training is really just (at best) a secondary elaboration of and on this. However, if *that* very specific training is principally part of something far broader, then the word '*training'* seems misleading in respect of the latter. As your question suggested, social attunement and education is arguably primarily *living*, not specific *training*. Are we 'trained' how to be good human beings? Can we be put on tracks that reliably end up with us being good? There is a sense in which it is *absurd* to talk about being 'trained' to be a good human being. Being a good human being is something which must go *beyond* any training.

Consider the relationship between a trainee and a trainer. The traditional social scientist sees himself as in a position of superiority as regards the objects of his study. He considers that he will understand those 'objects' better than they understand themselves. In an important sense Winch reverses that order of priority: what the social scientist should be trying to do is to reach the position that those 'objects of study' are already in. *They* are in a position to 'train' the social scientist. (This is a startling conceptual shift. It discomforts us, as participants in a formal educational setting, nominally becoming 'Masters' of something …)

Winch says (p.88) that 'A historian or sociologist of religion must himself have some religious feeling …' This is an intriguing and controversial claim, turning the traditional idea of 'objectivity' on its head. Winch suggests that similar and equally far-reaching claims apply in both aesthetics and economics, for example. It is interesting to contrast Winch's remarks on Economics with the line taken by Milton Friedman regarding the axiomaticisation of profits and returns, for example.

Expanding upon Winch's remarks, one might say that economists' uses of terms such as 'liquidity preference' *describes* and *connects* with key problems quite well. However, does economics fully *understand* the problems it describes? Are the descriptions it gives prejudicial when they don't return to (as Winch puts it) what they are 'logically tied to?' If not, does economics have the resources to offer solutions (or indeed: how *could* it)? It is possible that its solutions and explanations may in fact *entrench* problems rather than solve them, as arguably happens when problems are addressed through the lens of a rational choice framework, and a solution such as increasing the degree of free choice people have is proposed. Might not additional 'freedoms' make a problem worse?

Let's briefly examine some of the ideas of a leading 'positive economist', Milton Friedman, with questions such as these in mind. The central idea in monetarism was that if the money supply was controlled, this would benefit the economy in a number of ways. Thus, Monetarism depended upon the possibility of controlling the money supply in order to bring about such benefits. However, the central problem in Monetarism (which was invisible to positive economics though would *not* be invisible from a 'Winchian' point of view) was the problem of the *concept* of money. Monetarism took 'money' to be something entirely tangible and measurable, and thus, assumed that various policy measures could be used which would control the amount of this 'stuff' which was circulating around.

However, the way that people understood the concept of money was (and is) rather different. They didn't think that money was what the monetarists told them it was: they thought it was whatever they could *use as money*. (Circular definitions are often the best kind, in understanding society, as opposed to in practicing science!) What tended to happen in economies run on monetarist principles was that as soon as the government identified something as 'the money supply' and attempted to control it, people began to find ways of using other things *as money*. Thus, that which was identified as *the* money supply ceased to be *the* money supply.

Money is not *stuff*: the amount of money in the world can't be counted, however counter-intuitive such a point may seem. Money, one might say, is its use. (As an aside here: Look at section 120 of Wittgenstein's *Philosophical Investigations,* a work that greatly influenced Winch.) One might think about this in terms of credit card use, debt etc. One might say that monetarism was eventually abandoned because the way that people understood and used its central concept (namely 'money') continually moved away from the way that the economists were trying to use it, and the model could not account for this. Indeed, the model *caused* its own failure.

This is a phenomenon that knows no counterpart in (natural) science. In natural science, there is no such thing as a scientific law being undermined by its own assertion.

Week (5) A Hard Case: Winch on Anthropology

Winch's article, '*Understanding a Primitive Society*' (*UPS*) is specifically concerned with the issues raised by the would-be social-science of social anthropology, though of course it has a wider potential interest.

For Winch, Azande beliefs are a genuine *mystery* – the Azande's belief that some of their members are witches, for example. This is not something that can be understood by means of a straightforward analogy with something already known by us. It will take something special to understand (or at least avoid badly misunderstanding) a primitive society. The Azande, Winch says, hold beliefs that we *cannot possibly* share, and they engage in practices that it is 'peculiarly difficult' for us to comprehend.

Therefore, if it is true that in trying to understand the social world we have a genuine head-start, in virtue of being competent social actors ourselves, there are nevertheless cases (like the case of the Azande) where we need to ask some very serious questions. The Azande thus constitute a hard case, a crucible in which to test the ideas of someone like Winch, and find out whether or not they are wanting. Winch looks at the Azande precisely in order to see whether what he (Winch) has been saying is actually going to work, outside the examples in relation to which he developed his own argument. You might very roughly say that he is taking a Popperian attitude towards his own hypothesis (the 'hypothesis' that the very idea

of a social science is questionable, and that social understanding is pre-eminently non-scientific) …

The question of difference is of profound importance to Winch, and the problem in the case of the Azande is one of understanding people who are at a great distance from us. The 'tempting' or 'natural' thing to do when one encounters a genuinely puzzling social phenomenon is to try to find ways of quickly bringing it closer to one: to finds ways of inserting it into a familiar conceptual frame of reference, thus enabling one to (seemingly) understand or explain it. That is what Winch thinks Evans-Pritchard is doing, as any *social scientist* would do, and that is what he thinks (in a case such as the Azande) it is a mistake to do. A 'mistake' of heroic proportions.

Winch warns us against assuming that, when encountering a social phenomenon that is distant from us, we should instantly try to bring it *closer* to us. Find an analogy for it within our own experience. Find something that it can be modelled on. Perhaps in order to get anywhere in understanding the social world, we have rather to take *difference* very seriously. Perhaps difference even needs sometimes to be *exaggerated.* Perhaps that is a sound 'methodology' to adopt in such cases. In contrast, trying to understand Azande practices by direct analogy with our own scientific and technological practices is an attempt to make them appear too alike (to us), as well as (obviously) making it difficult to avoid looking down on them and seeing them as systematically plain stupid, when this is unlikely perhaps to be the case. Therefore, such an approach risks occluding their real difference, and, ironically, makes it impossible for us to understand those practices as they really are, to understand where they actually are and are not akin to our own.

Thus, for Winch, Azande magic is *not* failed science. Those who asserted that it is were taking a 'Whiggish' approach – exactly the kind of approach that *Kuhn* took his predecessors in the philosophy of science, unhelpfully, to have taken! In order to genuinely understand past science, one has first to understand it on its own terms, and understand thus its very great difference from the science of our own time, and not see it merely as a failed first pass at our science. Similarly, in order to understand genuinely alien cultures, one has first to understand how they hang together on their own terms, and not see them as a failed first pass at our culture. In order to understand such practices, in sum, we need to put them at a real distance, and we need to ensure that they *remain* at that distance for a good while.

Winch's is thus a profoundly similar idea to Thomas Kuhn's, when he talks about incommensurability. To detail the similarity a little more, because this is at the very heart of what we are doing in this course: For Kuhn, understanding the history of science involves understanding that there are scientific *revolutions*, eruptions in the history of thought. These revolutions are incomprehensible to scientific empiricists (who see only accumulation, normal science) and to falsificationists (who cannot see enough normal science and paradigm taken-for-granted in order to see revolutions against that background; indeed, for the followers of Popper, it is as if there is only revolution … but this is a picture of philosophy (or perhaps sociology!), not of (real) science). The way that chemists

thought before the chemical revolution was fundamentally different from the way they thought afterwards. Aristotelian physics is fundamentally different from Copernican physics. One cannot rightly conceive of the Aristotelian attempting to do what the Copernican is doing and yet somehow failing or *not yet* succeeding at doing it. One must attempt to understand the old scientists on their own terms. There is therefore a serious analogy between Kuhn and Winch.

While social anthropologists such as Evans-Pritchard take our scientific and technological knowledge for granted, and then try to understand (for example) Azande magic, Winch suggests that in order to *understand* Azande magic, one has to 'put aside' that scientific and technological knowledge – to think around it or behind it in order to have a chance of understanding such deeply puzzling and seemingly incomprehensible practices.

To return for a moment to Winch's remarks in *ISS*, the task of the would-be social scientist is not like the task of the scientist, but like the task of the person trying to comprehend, e.g. the trainee scientist. Again: we must in a certain sense *let ourselves be trained* by those we seek to understand: senior scientists, in the case of natural science; *those we seek to study*, in the case of 'social science' … Thinking of Kuhn's suggestion that we should not conceive of science as inevitable progress to where we are now, or of 'old' scientists as failed or immature versions of present day scientists, helps us see the strength of the Kuhn/Winch analogy. When one is being a historian of science as opposed to a practicing scientist, one should attempt to notionally put aside the scientific knowledge one has.

Winch is saying something similar. In studying the Azande, for example, one needs to attempt to imaginatively enter into their different world view. Only then may one be able to see how it makes sense.

Student – Do we have to 'think around' our conception of rationality? – or just our conceptions of technology and science?

That's a vast and interesting question. In part two of *UPS* Winch suggests that one needs a concept of rationality in order to take any investigation into *anything* seriously. We might here distinguish between *concept* and *conception.* Of course, if this distinction is just meant as piece of philosophical sleight-of-hand then one might not be impressed by it; but it might in fact be genuinely very useful. We do not, of course, want to abandon the whole concept of rationality – that would be crazy. However, we might think seriously about the fact that there can be *different conceptions* of reality and different conceptions of what rationality is. What we are trying to understand in the case of the Azande is an alien *conception* of rationality.

I might answer your question by saying that what Winch wants to do is to keep open the possibility that there is a conception of rationality at play among the Azande and not merely a *failure* to be rational. The right approach may be to look at Azande practises as though they *did* hang together, Perhaps one should seek out the nature of the Azande's game as it is played. We can contrast such an approach

to that of treating such practices as though they could not *possibly* hang together, because (e.g.) they do not push certain ideas to their logical conclusion.

Student: Winch has been accused of being a relativist, and even of considering that primitive society is in important respects superior to our own.

That raises an interesting question: the question of whether there are some desirable features of 'primitive society' which we have lost. For example, one might consider how many primitive societies consider themselves more tightly connected to, or part of, *the land* or *the earth.* Is this is an old superstition which we have grown out of; or is it something we have actually *lost*? This connects to some of the wider themes I have asked us to address: for example, if we think about the important differences which (I suggest) exist between natural and social science and therefore we think about the social differently from the way we think about the natural, does that mean that there is a divorce between the two, as has often been argued? That would be unwelcome. How does this relate to the notion of the eco-system (or 'the environment'), that we raised in Lecture 1? Can *that* notion unite the two? One might consider the thought that it is in some sense *easier* for a primitive society to think environmentally or ecologically than we do. Crucially, Winch *isn't* trying to say that we can't criticise, and still less that we can't disagree with the Azande. The possibility of criticism and disagreement of course remains. It would be very odd, having read *UPS*, to think that one should become like the Azande and adopt their practices.

But a sense of closer-than-close connection with the land; that is something that it would be better I think not to have lost. What we have wisely lost of old conceptions of the sacred is perhaps their sense of the *specificity* of sacred land. We cannot take seriously that such and such a *particular bit* of land is the Promised Land, or such like. But if we cannot take seriously that the land *as a whole* is in some sense sacred, in some sense *us*, and if we cannot easily therefore think of the physical and the social (the human) as *one*, then we have I think unwisely lost something.

However, these broadly ethical or spiritual questions are in some sense secondary, from our point of view in this class. What Winch is primarily interested in is the question of *understanding.* He is questioning the social scientific desire to explain, and worrying that the desire to explain can *get in the way* of understanding.

The issue is: what have we established about the Azande by means of social-scientific explanation? Have we been able to establish that the Azande are not to be 'followed' by means of a social scientific explanation? Not at all, Winch suggests. The task of social anthropology should be to attempt to reach genuine *understanding*, not just a rephrasing of our prejudices. We don't *need* social anthropology to tell us we are not going to adopt the Azande way of life. However, social anthropology would be doing something useful if it helped us to understand what that way of life actually *is*. Winch believes such understanding is possible if one moves beyond the approach of someone like Evans-Pritchard, for example.

As regards the charge of relativism, Winch himself argues strongly against it. He wants to suggest that reality is what it is independently of our *talk*: if one maintains that reality does depend upon our talk, then one is left with a kind of philosophical idealism, of which relativism is just a sort of 'pluralised' case. Thus, what one is trying to do in trying to understand others in the social world is not trying to uncover the nature of reality, but trying to uncover the nature of people's *accounts* of or *ways of taking* reality. Therefore, again: understanding society is more akin to understanding scientists than it is akin to understanding science/understanding the natural world.

In *UPS*, Winch addresses and takes seriously the possibility that there can be coherent universes of discourse which are not 'like science.' Winch is challenging the assumption at the heart of traditional social scientific accounts (such as Evans-Pritchard's) that the only valid way of thinking about society is to produce a coherent universe of discourse 'like science' about the society in question.

Winch suggests that we should engage in a process of reflection in order to understand things which are genuine puzzling. If we do so, we *must* be open to the possibility that we will be *changed* by such a process. If Winch is right, then there is a sense in which the student of society has to *participate* in what it is that is being described or observed and so on – and how odd it is that this was not always *obvious:*

These are human beings in a social setting, after all. Isn't there something fundamentally misleading (perhaps even fundamentally *unethical*) in assuming that one would not be *affected* by what one saw?

Consider the example of someone who *observes* some children drowning in a pond. It is in a crucial sense only a difference of degree between *that* and what the social anthropologist, the sociologist or the economist does. How could a 'social scientist' justify 'simply observing' the society in question? As if they were a Martian. Winch is telling us something which should have been obvious: and yet the ideal of social science and the ideal of objectivity makes it seems as though the opposite should be true.

So now, thinking of more or less coherent universes of discourse that are not like science. Still, we have to see how the discourse in question can be justly termed 'coherent'. A question at the heart of *UPS* is: how do we cope with the fact that it seems that the Azande are committed to a contradiction (their particular conception of witchcraft)? We are not, after all, talking about people who have radical hallucinations in the same sense that some psychotics experience (at least temporarily) a different world. Yet, somehow, their world is profoundly different from ours. (A useful comparison here is: the intriguing verbal formulations that Kuhn resorts to in the section of *The Structure of Scientific Revolutions* called 'Scientific revolutions as changes in world view.')

Winch (and Wittgenstein in his *Remarks on Frazer's 'Golden Bough'*) reminds us that Azande practices are not intended to *substitute* for science and technology, and that in thinking that they have found the counterparts to science

and technology in these practises, social anthropologists such as Evans-Pritchard are perhaps looking for explanations in the wrong places.

Winch asks, in effect: 'To *whom* are Azande practices allegedly unintelligible?' He is surely not asserting that their practices are entirely impenetrable to us. But, if you make certain assumptions about taking thoughts through to their logical conclusions. One can argue that there are inherent contradictions at the heart of our most fundamental practices (such as arithmetic), and that in ignoring these contradictions, mathematicians are simply not taking their thought to its logical conclusion. For instance: on what grounds is division by zero disallowed? Isn't this just a refusal to pursue arithmetic 'to its logical conclusion'? *This is just the same kind of observation that Evans-Pritchard makes of Azande practices.* Winch suggests that when the Azande appear to resist 'taking their thought to its logical conclusion' one should see it not as a failing, but as itself an integral part of a coherent universe of discourse, just as the discourse of mathematics does not include division by zero, and yet remains coherent and reasonable precisely by virtue of such non-inclusion.

By way of conclusion, a Winchian question: Isn't it Evans-Pritchard, in attempting to 'press Azande thought where it would not naturally go' who is guilty of a misunderstanding, and not the Azande themselves? This is what Winch means by talking of Evans-Pritchard as, roughly, committing a 'category-mistake'.

Week (6) Wittgenstein among the Human Sciences

In a discussion between two people concerning whether or not there is an aeroplane overhead, there can be fairly unproblematic agreement or disagreement between them. The participants in the discussion, are in this case, we might say, fairly *near*. However, a disagreement over a religious matter between a religious and a non-religious person is not of the same nature. The superficial resemblance between the different sentences used in the discussion should not lead us to think that the participants in the discussion are *near to* each other. For example:

> There is *x* overhead.
> There will be *y.*

If x and y are *rain* or *an aeroplane overhead*, one can debate the matter in a (more or less) scientific way. However, what if x is *an aeroplane overhead* and y is *a last judgement*? The two sentences still look superficially similar, but Wittgenstein suggests, in his Lectures On Religious Belief, that there is a great distance between the person who says there will be a last judgement and the person who says that he is 'not so sure.' In such a case, can one not be misled by the superficially similarity of the sentences?

One way of addressing the issue is to consider the standard way in which we think of belief in God. We think, perhaps, that there are three possibilities:

1. Theism (the belief that there is a God);
2. Atheism (the belief that there is not a God);
3. Agnosticism (neither the belief that there is God nor the belief that there is not).

According to this model, it appears that agnosticism is a 'don't know' position between theism and atheism. However, according to the conception of these matters put forward by Wittgenstein, things surely appear to be rather different. If one stops thinking of the above options as *theoretical* claims, and considers them more as belief *in* or trusting *in*, or perhaps as a *commitment to* something, then agnosticism and atheism start to look a lot more similar to each other. The theist is the person who has, uses and commits to a certain set of pictures, which neither the atheist nor the agnostic has, uses nor commits to. According to this conception, the only difference between the latter two is that the atheist sets his face against those pictures, and the agnostic just says 'I don't know.' Thus, the idea of agnosticism as an intermediate position begins to look somewhat misleading. There is, rather, a gulf between the *commitment(s)* of the theist, the religious believer, on the one hand, and the *absence* of (any such) commitment of both the atheist and the agnostic, on the other. (This in turn implies that there is something fishy about the standard way in which agnostics take themselves. They take themselves to be respectably half-way between two 'dogmatic' alternatives, to be wisely uncommitted; but actually, they are already committed. From the believer's point of view, they have already chosen very firmly where to stand and (more importantly) where they are not prepared to stand.)

Wittgenstein tells us that in believing or not in a judgement day, the expression of belief may play an absolutely minor role. The expression of belief here refers to the belief *that* so-and-so is the case, or believing *that* so-and-so exists. Thus, in an important sense I cannot contradict the religious person when she says 'I believe there will be a last judgement.' Because whatever *I* deny belief in will not be what *she* believes in. Because what she believes in is not something that can be put in terms of a simple '[I believe that] Such-and-such is the case' clause, as if it were a scientific claim or a simple factual remark.

These remarks of Wittgenstein's are, I believe, of crucial importance to the social studies. For example, if one actually tries to *understand* what the religious person is saying in such a case, rather than simply theorising about it and assuming that one already knows what they mean, it will potentially deeply involve one *as a person*, not just as an external observer, an outsider.

One might want to say that the impossibility of contradiction implies the impossibility of understanding: however, Wittgenstein's remarks suggest rather that coming to understand involves recognising that there is a great deal of complexity in the idea of contradiction (and also, I would suggest: of understanding) in some/such cases. Coming to understand, in other words, may involve roughly the kind of effort and work and indirection required of someone trying to understand Zande witchcraft (or Aristotelian physics). What one is doing (in the expression of belief

in a last judgement, for example) is not, Wittgenstein suggests, akin to making a quasi-factual claim, open to straightforward contradiction.

Practices such as burning effigies, or the probably-more-familiar one (to us) of kissing a picture of one's beloved, are obviously *not* practices performed in the belief that there will be some specific causal effect on the object that the effigy or picture represents. It is perhaps in picking the right objects of comparison that we can begin to see that we are not *so* deeply different from (for example) the Azande after all.

Perhaps we exaggerate the extent to which our lives are ruled by the ideals and methods of science and technology. These reflections can perhaps help us to better understand what Winch is attempting in *UPS*.

It is interesting to note that both Wittgenstein's criticisms of Frazer and Winch's criticisms of Evans-Pritchard suggest that those anthropologists are (in a sense) *more primitive* than the people they are interpreting, in that *they* are making the scientific – or superstitious – errors which they accuse the people they are interpreting of making. This is an interesting way of highlighting the radicalism of the critique that both Wittgenstein and Winch are engaged in. The danger of doing social science when it is not appropriate is that one may end up doing something *primitive* in the guise of doing something sophisticated and epistemologically 'superior.' One may *impoverish* one's existing understanding, and preclude better.

Winch proposes a radical picture (not a theory) of human beings as being 'internally related' to each other. This 'picture', of course, has antecedents, in Wittgenstein and elsewhere. Human beings are not (according to this picture) related to each other *as separate things*. The scare quotes around 'internally related' are significant, in that, in an important sense human beings are not *related* to each other *at all*, because the idea of an internal relation implies the idea of inseparable parts constituting a greater whole. (Once again, the notion of insiderhood may be helpful here. We are, one might say, inhabitants of and inhabited by each other, and perhaps of and by language, too …)

Thus, any talk of 'relation' implies the idea of separation, and the sense in which Winch talks of human beings as 'internally related' to each other is that they are intrinsically one and the same thing. *Not* separated.

However, can one leave things at that? One might even accept the idea of human beings as internally related, whilst continuing to think of human beings as *externally* related to the world. Such an assumption is challenged by a genuinely ecological point of view, in which one considers human beings as embedded within an eco-system to which we are internally (and fundamentally) related, as well as to each other.

Such issues are of fundamental importance in thinking about how we might go beyond the traditional natural/social science dispute. Do Wittgenstein's and Winch's ideas have a deeper potential application? How are we to think about the environmental sciences in the context of such ideas? If such ideas were extended and applied hereabouts, then environmental science would perhaps no longer be understood as being a matter of understanding something which is essentially

outside of or external to oneself, as is the case in biology or physics. I can study my own liver as if it were an organ in another's body. But if I study human or other beings as if they were not in a dialectical and co-constitutive 'relation' with their ecosystem, then I fail to study them properly at all. To *be* scientific, in this connection, demands being and knowing oneself to be on the inside …

Such a possibility as just described seems to me one of the most potentially fruitful upshots of a Wittgensteinian perspective on 'the human sciences', a perspective which (as we have seen) is further developed in the work of Kuhn and Winch. If this course of six lectures has left you in a good position to consider such a possibility, then it will have more than achieved its objectives.

PART 1
Wittgenstein, Kuhn and Natural Science

Science: A Perspicuous Presentation

It is fundamental to [the concept of 'necessary distance'] that it is what actually brings one into connection with that from which one is appropriately distanced; it is not a distancing that separates. Necessary distance is what makes empathy possible.

Iain McGilchrist, *The Master and his Emissary*: 282.

1.1 Is Kuhn the Wittgenstein of the Sciences?[1]

> To the different networks correspond different systems of describing the world. Mechanics determines a form of description by saying: All propositions in the description of the world must be obtained in a given way from a number of given propositions – the mechanical axioms.
>
> Ludwig Wittgenstein, *TL-P*, 6.341.

In this Section, which is among other things an 'introductory' Section to and of this, Part 1, I remind the reader of the explicit presence in Kuhn of elements of a therapeutic understanding of the process of science itself, at times when science gets 'ill'. I then offer a reading of the most controversial chapter of *Structure of Scientific Revolutions* [2] (on 'world-changes') highlighting the delicately therapeutic manner in which it is *written*, a manner of careful composition which draws the sting from the still-widespread uncharitable reading of that chapter as committing Kuhn to (what *would* be clearly un-Wittgensteinian) a substantive semantic relativism. If my reading is successful, it defeats the strongest textual evidence there is for a problematically relativist reading of Kuhn.

Thus 1.1 assembles crucial evidence for Kuhn as a therapeutic thinker in a more or less Wittgensteinian mold, and suggests some reason for thinking of Kuhn as a truly Wittgensteinian philosopher of science. This sets the scene for the remainder of Part 1, and to some extent for the book as a whole, insofar as what the book is concerned with is an assessment of the extent to which it is helpful to regard a *variety* of disciplines as 'science(s)'.

This book is called *Wittgenstein among the Sciences*. In relation to the philosophy of (natural) science, I propose to take Kuhn as a kind of proxy for Wittgenstein. So, in making such a proposal reasonably initially-plausible, it is reasonable for the reader to expect me to have something at least to say about the following question: How was Kuhn influenced by Wittgenstein?

The evidence suggests that Kuhn learned a huge amount from the greatest Wittgensteinian philosopher of our time, and just possibly the greatest living philosopher, his own-time colleague, Stanley Cavell. Look for instance at page

---

1 The present Section contains within it a revised version of some bits of material originally presented in T. Kuhn, 'The Road Since *Structure*', *British Journal for the Philosophy of Science,* 55:1 (2004), 175–178.

2 Henceforth *SSR*. All subsequent page references are to the following edition: Kuhn, T.S. (1962/1996) *The Structure of Scientific Revolutions*. 3rd ed. Chicago: University of Chicago Press.

297 of the interview with Kuhn in *The Road since Structure*: '*extraordinarily* important' (emphasis in the original), is how Kuhn describes Cavell's early influence on him, while they were both young bloods. (It's true, Cavell was mostly 'Austinian' then, in those young days; but he was already understanding Austin in a fairly Wittgensteinian way (as we New Wittgensteinians, for example Crary (2002), believe Austin *is* best understood). Kuhn picked up on Wittgenstein from Cavell, from Hansen, from Feyerabend, and from the general intellectual zeitgeist of the time and place. One should bear in mind also the crucial moment when Kuhn *cites* Wittgenstein, a few chapters into *SSR*, in the section called 'The priority of paradigms'. This is a really important influence, in itself: because of the utter centrality of paradigms to Kuhn's philosophy of science. Kindi (1995) and Jean-Paul Narboux are among those who have rightly made a great deal of this connection. The point that Kindi and Narboux both make at length is not just about affinities. It is about how crucial that explicitly Wittgensteinian moment in *SSR* is for Kuhn's whole – broadly Wittgensteinian – project.

But, more than these historical points, the affinities between Kuhn and his great predecessor Wittgenstein are, I want to say, *evident*. Take for instance the closing paragraph of 'Reflections on my critics' in Kuhn (2000):

> What each participant in a communication breakdown has.found is.a way to translate the other's theory into his own language and simultaneously to describe the world in which that theory or language applies. Without at least preliminary steps in that direction, there would be no process that one were even tempted to describe as theory choice. Arbitrary conversion (except that I doubt the existence of such a thing in any aspect of life) would be all that was involved. Note, however, that the possibility of translation does not make the term 'conversion' inappropriate. In the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world. That sort of transition is, however, not one which the terms 'choice' and 'decision' quite fit, though the reasons for wanting to apply them after the event are clear. Exploring an alternative theory., one is likely to find that one is already using it (as one suddenly notes that one is thinking in, not translating out of, a foreign language). At no point was one aware of having reached a decision, made a choice. That sort of change is, however, conversion, and the techniques which induce it may well be described as therapeutic, if only because, when they succeed, one learns one had been sick before. No wonder the techniques are resisted and the nature of the change disguised in later reports (Kuhn, 2000: 174).

This passage is exemplary of the concentrated brilliance of Kuhn's writing, and of the depth of his own quite distinctive contributions to the philosophy of science (e.g. the explanation of why the history of science tends to read as if there have not been scientific revolutions). It also evidences something that will be important to us in the Sections to follow: Kuhn's (sometimes slightly desperate) wish to make

himself comprehensible (on their own terms) to the 'Analytic' philosophers by whom he was most harshly criticised.

The passage *also* makes *visible* a whole series of inheritances from or (at the least) deep parallels with/affinities to the philosophy of Wittgenstein:

- The emphasis on the real possibility of communication breakdown, but further the possibility that *some* such breakdowns are productive of a *new* understanding;
- the open willingness to entertain or utter words (phrases, sentences) that are provocative or even paradoxical, together with the repudiation of immodest, dogmatic readings of those words;
- the repudiation of objectivist fantasies and the permission of conceptual difference, without the commission of relativist theorising;
- great care over the words we do use, and over the words we *want* to use – and over their limits;
- the prioritisation of practice, even when what is being practiced is a theory, or theorisation;
- and lastly, and most strikingly of all, a metaphor of illness, an emphasis on the variety of methods by which one may try to cure oneself (or others), and an explicitly therapeutic conception of such cure.

The passage helps one to *see* the fundamental point that Sharrock and I were urging in our Wittgensteinian reading of Kuhn, in our book: that Wittgenstein can help one to understand that Kuhn doesn't threaten the objectivity of science.

We should pause a moment, however, before proceeding to identify Kuhn and Wittgenstein *too* closely, and perhaps even calling Kuhn a 'Wittgenstein of the sciences.' For while Wittgenstein more or less identifies *himself* as a midwife of change in philosophy (in oneself), as a therapist (albeit one who, like Freud, has to cure himself as well as, or perhaps before, curing others), the analogous or parallel figure in the quotation just examined is not *Kuhn*, but rather *the scientist* at the point of crisis and transition. It is the scientist at a moment of extraordinary or revolutionary (conceptual) change who may describe his earlier self or his old paradigm as having been sick, monstrous, or an unhealthy or unholy mess.

This of course should not actually surprise us: Kuhn always made it clear that, if science ever resembled philosophy, it resembled it somewhat at the moments of crisis; whereas (for example) Popper wanted science to be like philosophy, in the sense of always starting from ground-zero, and always being revolutionary.

So: *Is* the new, radical, (non-)philosophy of Ludwig Wittgenstein to be found, 'in disguise', in Kuhn's philosophy of the sciences? That is the overarching question of this Section.

Kuhn's primary reputation is as the great leveler, reducing natural science to the level of all other disciplines. And, in other words: as the great relativist, holding that whatever view of the world works for a given discipline at a given time is the truth. Kuhn has certainly gone down in sociological and post-modern

circles in roughly this way[3] and so, very often, has Wittgenstein, such that it might well seem that *Kuhn is indeed a Wittgenstein of the sciences*. My argument is that there is at least some good reason to hold the italicised thesis to be the case – but absolutely *not* for the reason so far sketched in this paragraph.

Kuhn is a very different figure from the almost cartoon-character that his 'foes' and 'fans' both depict.[4] In my understanding, Kuhn's fundamental task was simply to understand – or rather, better, to find a mode of presentation that would ultimately avoid *mis*understanding – the nature of science, including, of course, scientific change both minor and major. He wanted, if you like, to return us to science, (yet) to leave science as it actually is;[5] science as we are returned to it is probably not science as we ever succeeded in seeing it before reading Kuhn. Yet it *is* science as it (actually) is. Kuhn, for the first time ever, offers us successfully, I believe, a historically-valid picture of the whole of science. Not just a formalistic caricature of normal science (as with Positivism), nor a formalistic caricature of revolutionary science (as with Falsificationism). Rather, a non-caricatural, historically based, philosophically subtle and modest account of (the totality of the concept of) science.

The main thing I want to address here – in this Section, *and* in the remainder of Part One of this book – is the issue which, to many readers, has appeared to be the biggest *problem* with the attribution to Kuhn of a modest, properly 'Wittgensteinian', approach, of a leaving of science 'as it is', of a refraining from substantive metaphysical or even epistemological commitments. That big problem is encapsulated in the famous moments in *SSR* when Kuhn has appeared most strongly to violate such a counsel of modesty: namely, in his discussion of 'world changes'. For many of his readers, in talking of the world changing when science changes – through 'scientific revolution,' – Kuhn has fallen into some kind of metaphysical relativism, or pluralistic idealism.

Most of what I suggest below is in fact very elementary. I am simply going to read some of the most troubling passages in *SSR*, (Section X), *Revolutions as changes of world view*. The reader may find it helpful to have the book open in front of them, at the relevant pages. I shall attempt to see if whether those passages can be understood in the modest way I have suggested, without committing Kuhn to a form of metaphysical relativism or such like.[6]

Let us begin with pages 110–111 of *SSR*. This is where Kuhn first starts to say things that have sounded very strange to many: 'I have so far argued only

3 For detailed criticisms of the reading of Kuhn to be found among Kuhn's 'followers' in the social sciences etc., see Read (2001b).

4 I think the same of Wittgenstein. I think it is clear that he is no Relativist, no Idealist (but also no Realist), etc. See for instance *PI* 402.

5 My suggestion, therefore, is that Kuhn is practising 'therapy' by presenting something to us in a way that engages our (including his own) temptations to misunderstand it, and tries to work through them.

6 My deep thanks to James Conant, for inspiring this reading.

that paradigms are constitutive of science. Now I wish to *display a sense* in which they are constitutive of nature as well' (Emphasis added).

Note what Kuhn does not say here. He does not say, for instance, 'Now I wish to explain that paradigms constitute nature, as well.' He says he wishes to get at *a sense* in which paradigms may be said to constitute nature. And he wishes not to set that sense in stone, but to *display* it to his readers, so that it is temporarily figural and so does not get completely missed. I think that if we fail to attend to the niceties of Kuhn's linguistic expression, then that will be in the end only to our own disadvantage. When read carefully, Kuhn's aim here already sounds more modest than has usually been allowed. He goes on: 'Examining the record of past research from the vantage of contemporary historiography, the historian of science *may be tempted* to exclaim that when paradigms change, the world itself changes with them.' (*SSR*: 111, Emphasis added).

When a temptation comes along – for instance, when one is offered illegal drugs – one doesn't necessarily immediately give right in. (And if one does, one may come to regret doing so before too long!) In other words: Kuhn is *not* simply urging us here to exclaim that the world changes, when paradigms change. Again, he does not simply write, 'When paradigms change, the world changes with them.'[7] That is what I want to make crystal clear: Kuhn is not himself in the business of saying, 'Scientific revolution, therefore *world change*!'

Further down the opening paragraph of this Section, we find the following passage. And see for yourself how different it sounds, with the emphases falling where I have indicated, from the way Kuhn is usually heard, or assumed to be:

> It is … as if the professional community had suddenly been transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well. Of course, nothing of quite that sort does occur: there is no geographical transplantation; outside the laboratory everyday affairs usually continue as before (*SSR*: 111).

There is no necessity for *any* world change at all to actually occur. Kuhn is speaking almost exclusively of something relevant to scientists, to small professional communities, the world of the relevant community of scientists (Compare: The world of stamp-collectors, the world of politics, etc.):

> … paradigm-changes do cause *scientists* to see the world *of their research-engagement* differently. In so far as their only recourse to that world is through what they see *and do*, we *may want* to say that after a revolution *scientists* are responding to a different world (SSR: 111, Emphasis added).

7 This has not stopped people claiming that just that is what Kuhn writes. An egregious example is to be found in Gill (1996). Gill (on page 136) misquotes Kuhn so as to remove all his (Kuhn's) care, caution and qualification.

It is scientists who respond to a 'different world', not the rest of us. Most crucial of all is the formulation 'we may want to say …'. This, at the end of this influential opening paragraph of Section X of *SSR*, echoes the opening of the paragraph, which we looked at above: 'the historian of science may be tempted to exclaim.' That we may want to say something does not entail that we should say it. And it strongly suggests that we should at least be wary about the consequences of saying it, if we choose to do so.

In other words, Kuhn is warning us, through his carefully chosen terminology, and by his repeated indications that there is something potentially dangerous in these 'temptations' and 'wants' that we are subject to, that it is easy for philosophers to find themselves speaking nonsense. And wouldn't it certainly seem like nonsense, to speak of paradigms (for example) as constitutive of nature? One is reminded of Wittgenstein's remark, ''Don't *for heaven's sake,* be afraid of talking nonsense! But *you must pay attention to your nonsense*.' (Wittgenstein, 1980: 56, Emphasis added in final line)

Some have thought Wittgenstein pathologically anxious to avoid speaking nonsense himself, and all too keen to dole out the critical epithet, 'Nonsense!' to others. This is mistaken; 'nonsense' is indeed a very important term of criticism, for Wittgenstein, but the point is not to pathologically avoid speaking any, but to be aware of when anyone (including oneself) is apparently speaking nonsense, and of why they want to. Crucially, judgements of 'Nonsense!' are provisional: they depend upon convincing the speaker themselves that they did not in fact have a clear intention, and were 'hovering' between different possible uses of their words.

One will *need* to speak nonsense, often, when doing philosophy, if one is not to stay on the barren heights of cleverness, and to fail to actually effectively engage with the temptations of oneself (and of others) to *mire* oneself (themselves) in nonsense. (This is the real point of *Tractatus* 6.53.) That is to say: Kuhn, like Wittgenstein, when the philosophical stakes are highest, *engages our temptation* to mire ourselves in nonsense. He doesn't shy away from it.[8] Kuhn wants one not to be afraid even of speaking nonsense, for the sake of better understanding the nature of science; but he urges that one had better be aware of just what one is doing at every stage. One can see that right-minded urge throughout his writing.

Let us go further in exploring this parallel with Wittgenstein, who felt that to reach clarity, to cure ourselves, to take part in an effective philosophical therapy,

8 At least, this is true of Kuhn in *SSR* (and not just in the examples I am writing on here. I think one could run a similar argument, for instance, on Kuhn's intriguing picture (see especially Section II of SSR) of pre-paradigmatic scientists as undoubtedly scientists, even though the product of their labours was somehow less than science). Mostly regrettably, though quite understandably, Kuhn does later become shier of speaking nonsense: having seen the horrendous misunderstandings he was subject to at the hands of his 'foes' and 'fans' alike, and having in particular been calumnied by his philosophical 'foes' with talking trash. Kuhn tried to avoid being so misunderstood and calumnied again, and so backed away from some of the richest (and also, admittedly, riskiest) moments in his oeuvre.

it was necessary to go through the crucible, the test of investigating one's own variegated inclinations to speak nonsense (e.g. to feel that there is something right in metaphysical Realism, and/or in Kantianism, and/or in Idealism, etc.). Wittgenstein pursued this strategy in radically different *ways* – his *tactics* in the *Tractatus* bear little resemblance to those employed in *Philosophical Investigations* – but the overall strategy, the overarching aim, was the same throughout.[9] That aim was to get the reader to overcome metaphysical predilections by means of enabling the reader to see just what those predilections are (and how radically unclear they are).

In the *Tractatus* for instance, as I hope the reader will have spotted in the epigraph to this Section, Wittgenstein offers the conception of a 'network' of scientific concepts and of a set of 'axioms' that, together, might very roughly be seen as prefiguring embryonically the concept of a 'paradigm' that Kuhn famously gave us. This is one of the key final stages in teaching differences that the early Wittgenstein offers the reader on the journey up the ladder. I think that Kuhn, in terrain where it is very hard to say anything useful – in the vicinity of fundamental conceptual change in science – is doing what Wittgenstein did in similarly difficult terrain, namely helping us to have a clearer view by taking *a necessary journey through nonsense*. We may want to say that after a revolution scientists are responding to a different world. If we do say that, we will be saying something very strange indeed. However, understanding why we want to say it, and what the consequences of saying it are, may at least give us some insight into what is going on in, for example, the Chemical Revolution – at *least* in the historiography etc. of the Chemical Revolution.[10]

Let us move on to another key moment where the 'world changes' idea rears its head, and see if the same considerations can apply. In *SSR*, on page 118, we find the following:

> [A]s a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he 'saw

---

9 Detailed justification for this claim can be found in Crary and Read (2000), especially in the Introduction. See also my paper in my and Lavery's (2011).

10 For here is an important point: Kuhn came later to doubt that the 'gestalt-switch' metaphor for scientific change applied very directly to the scientists themselves, but he never gave up the thought that it applies to the (good) historian of science, one willing to work to find (what McGilchrist calls – as in the epigraph that opens Part 1) the necessary *distance* from which to see the worldview of previous scientists. The scales can fall from one's eyes, almost literally, when one suddenly gets (e.g., Kuhn's favourite example) what strange kind of physicist Aristotle might have been, if he was not a very bad or stupid physicist. Insofar as our interest is in our own understanding of fundamental scientific change, this reflexive understanding of how it is like seeing a different world remains of great value, even if it risks misleading us somewhat as to the actual experience the scientists of the time had.

> differently', the principle of economy *will urge us to say* that after discovering oxygen *Lavoisier* worked in a different world (Emphases added).

Once again, what we feel an *urge* to say is not simply something we should say, as though it were unproblematic and simply true. Kuhn is drawing attention to the potentially nonsensical nature of his own urgings, just as much as he is drawing attention to the uselessness (if one wants to avoid misunderstanding how science actually works) of metaphysical realist desires to speak of the fixed nature that is there throughout all of scientific work. And so, we move to the most famous and tricky passages of them all.

> Is there *any legitimate sense* in which we can say that [Galileo and Aristotle, Lavoisier and Priestley] pursued their research in different worlds? (*SSR*: 120, Emphasis added).

Hardly a ringing claim: this *question's* phrasing signals explicit awareness that saying so would be odd indeed. In other words, Kuhn is agreeing with us that to think that we should resist using the expression, 'different worlds', is quite reasonable. He goes on, however, to argue that such resistance has led us into a dead end in the philosophy of science, and indeed in a number of related fields. He mentions (on page 121) also 'psychology [the gestalt school], linguistics [Whorf], and even art history [Gombrich].' We may have to risk using apparently extreme phrases, nonsensical sentences, if we are to avoid the aridity and irrelevance of most existing thought about science. In order to accomplish the modest task of putting ourselves in a better position to see the natural sciences as they are and were, we may have (self-awarely) to say some immodest-sounding things. How then does Kuhn answer his own question?

> I am … *acutely aware of the difficulties created* by saying that when Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum. *The same difficulties* are presented *in an even more fundamental form* by the opening sentences of this [S]ection: though the world does not change with a change of paradigm, the scientist afterward works in a different world. Nevertheless, I am convinced that we must *learn to make sense of* statements that at least *resemble* these. (*SSR*: 121, Emphases added)

Kuhn is aware of the trouble one can court, the absurdities of idealism for instance, when one talks this way. Nevertheless, he urges one to take the risk. Specifically, he suggests that we ought to (work to) find a sense for these words, or at least words somewhat like them. We ought to learn how to make sense of these words, words that as they stand *do not make sense.* What could be clearer? Kuhn is deliberately speaking nonsense here, when he makes paradoxical statements such as that the scientist works in a different world after a paradigm-shift, even though the world

has (of *course*) not changed. He cannot be accused of *lapsing* into nonsense. He quite *deliberately* speaks it.[11]

One way of getting Kuhn out of this 'predicament' that he has *voluntarily* entered into would perhaps be favoured by Hoyningen-Huene (1993) and others who read Kuhn (as he himself perhaps became increasingly inclined to do) through Kantian spectacles: namely, to find a way of apparently making sense of Kuhn's strangest statements without having to alter in any significant way our traditional ('received') image of science. For example, we might read the crucial sentence above as follows: 'Though the *noumenal* world does not change with a change of paradigm, the scientist afterward works in a different *phenomenal* world.' This quasi-Kantian 'disambiguation' of 'world' perhaps cleans things up somewhat.[12]

This move is therefore helpful, to a degree. But we have already seen indications that Kuhn wasn't keen to speak of 'the noumenal world' at all, for good post-Kantian philosophical reasons: primarily, that speaking of 'the noumenal' makes it sound like we are not in nonsense, not in paradox, any more; yet surely *we still are*. How, for instance, can there be any such thing as speaking of the unspeakable, the unsayable? Kuhn doesn't use the quasi-Kantian formulation that I have suggested a 'sympathetic' reader like Hoyningen-Huene would perhaps urge on us. Kuhn puts down a bald, seemingly *absurd* sentence. He then urges that, if we want to find an alternative to the epistemological views which have led to a crisis in Logical Empiricism, in falsificationism etc., then we should attempt over time to make some sense out of that sentence (or to put some sense into it, even?).

Wittgenstein's methods, his tactics, would of course look rather different. He would highlight what we can learn from the way in which what we want or even 'need' to say dissolves on us. Kuhn looks to find, over time, a less paradoxical way of saying what he wants to say; Wittgenstein would tend to make it more paradoxical, more *patently nonsensical*. But this difference in style and method, while not unimportant, does not necessarily connote a signal difference in overall strategy, or philosophical aim.[13] Like Wittgenstein, Kuhn wants to teach us differences, especially the deep difference between a puzzle-solving move and

---

11 In this regard, Kuhn is like Heidegger as well as Wittgenstein. Carnap notoriously criticises Heidegger for falling into metaphysical nonsense when he (Heidegger) speaks of 'the nothing itself noth-ing'. But Carnap's criticism falls flat in part just because it is hardly as if Heidegger does not know that this (speaking nonsense) is precisely what he is – deliberately – doing. At the end of his life, a more sophisticated Carnap was a great admirer of Kuhn's book. Had he perhaps rethought somewhat his own, troublesome attitude to the employment of nonsense in philosophy?

12 Or perhaps it merely gets us deeper into nonsense, through appearing to provide the form of a straightforward solution to our difficulties?

13 The reader who finds this remark implausible is directed once more to the close of Kuhn's 'Reflections on my critics', where his use of the idea of us being ill (when in 'crisis'), and needing treatment (e.g. through 'extraordinary science' of the kind practiced by Copernicus, Hertz, or Einstein), is strikingly similar to Wittgenstein's.

the full gravity of a major conceptual change, a difference which he thinks the theoreticians of and popularisers of science more or less sytematically efface. So again, I think we can *see* Kuhn hereabouts *as* a kind of therapeutic thinker, a *kind* of Wittgenstein of the sciences.

I hope that the above is already enough to show that even Kuhn's apparently most troubling writing is actually extraordinarily careful and self-aware, and does not support outrageous theories or theses (which his 'foes' and 'fans' alike have tended to attribute to him), and is at least reasonably compatible with how Wittgenstein might approach the philosophy of the sciences. Much as Wittgenstein traffics in nonsense in some of what he says for instance about 'the woodsellers',[14] so Kuhn traffics in nonsense in what he says about Aristotle and co. He ought to be praised for this, not buried for it. The way to greater philosophical enlightenment lies in understanding what we say and why we say it, even when it feels uncomfortable, and not in banning us from saying certain things.

Let us look now for comparative purposes at a particularly salient discussion of Wittgenstein's, in looking at what is happening when one tries to get enlightenment about one's concepts, *or* the concepts of others who may be separated from us by a conceptual gulf (e.g. by a scientific revolution). Although I have urged in this Section that Kuhn is not the metaphysical relativist he is often take to be, it remains the case that Kuhn can and does intelligibly hold onto something modest and necessary (and even meaningful!) which one could perhaps (if one wished to) call conceptual relativism, or better, conceptual relativity. 'Conceptual relativity' would involve bringing out the deep difference (though *not* of course an absolute gulf of incommunicability) between (say) phlogistic and modern chemistry. We can see better, I think, how this can be present (and correct) in Kuhn's philosophy when we see it in Wittgenstein's philosophy, in the conceivability (and actuality) of a sense of 'concepts' by means of which it is intelligible to speak of different concepts – and thus of 'partial communication' – without falling into semantic nonsense and/or self-refutation. The most crucial passage of all on this in Wittgenstein himself, runs, in *PI*, as follows:[15]

> I am not saying: if such-and-such facts of nature were different people would have different concepts (in the sense of a hypothesis). But: if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize – then let him imagine certain very general facts of nature to be different from what we are used to, and the formation of concepts different from the usual ones will become intelligible to him.

14 See Cerbone's and Crary's pieces in Crary and Read (2000) for discussion.

15 In this paragraph and the one that follows, I draw on (and adapt) passages from Read (2002a).

> *Compare a concept with a style of painting.* For is even our style of painting arbitrary? Can we choose one at pleasure? (The Egyptian, for instance.) (*PI* Part II: 230, Emphasis added).

'Our' style of painting or writing (e.g, poetry), with its distinctive kinds of effects and 'aspects' is not arbitrary, and no more is our style of science, our scientific sensibility. Kuhn can be seen, I am suggesting, as intelligibly pointing to how different aspects have been seen, have been acted upon and have been realised at different points in the history of different fields. He is concerned that we will do bad history and philosophy of science *if we take* our *concepts to be* '*absolutely the correct ones*',[16]or indeed even if we more 'modestly' take the concepts of a future 'finished science' to be 'absolutely the correct ones.'

Kuhn is contesting *the very idea* of some set of concepts being absolutely the correct ones, whether or not we ourselves claim to be currently in possession of them. In a nutshell, Kuhn is interested in *real* cases; those cases where the formation of concepts different from ours has happened, where those concepts have 'been the case.' [17]

If the connection I have drawn here is right, then Kuhn is certainly, among other things, enriching Wittgenstein's diet of examples by discussing in detail real examples from the sciences, of what Wittgenstein has already indicated an interest in, by means mostly of the (mostly) fictional examples which play such an important role in his later work.

We might even compare here the important paper, '*A Function for Thought Experiments*', in Kuhn (1977). As I read that paper, Kuhn is trying to get rid of a prejudice: a prejudice existing when we refuse to look, but merely think. The prejudice that thought experiments cannot possibly be part of science, and work, unless they are *real* (physical, etc.) experiments, not *thought* experiments. Thought experiments involve working upon one's assumptions, etc. In this respect, there *is* a parallel (though one I cannot investigate further in the present context) to be drawn with Wittgenstein's 'thought-experiments'.

And so, in sum: I think that there is a fair case for calling Kuhn something like 'a Wittgenstein of (the philosophy of) the sciences'.[18] In the remainder of Part 1 of this book, I will attempt to develop further my reading of Kuhn in this light, deepening the reading presented previously in my book, *Kuhn*, and focusing

---

16 Again, a danger should be borne in mind here: One should not start to think of the 'different concepts' to which Wittgenstein refers as potentially forming an ontology 'wholly alien' to ours. To do so risks falling straight back into an over-strong semantic relativism, justly criticised (though not justly found in Kuhn) by e.g., Davidson. (See David Cerbonne's work for discussion)

17 This is one point at which Kuhn's and Foucault's methodologies and aims are closely-aligned.

18 And I think that that case largely survives the (highly pertinent) worries expressed by Angus Ross, in his reply to me on this, in *UEA Papers in Philosophy*.

primarily upon the key area already touched upon here: how to understand what *incommensurability* actually is. This is key to seeing how Kuhn's philosophy of (natural) science can be viable; and thus key to making progress on what will be the central issue of this book: whether the 'human sciences' are in any meaningful sense – in something like the sense present in Kuhn's rendition of the (natural) sciences – *sciences*.

1.2 Kuhn and Incommensurability: An Interpretation

> One can deceive a person for the truth's sake, and (to recall old Socrates) one can deceive a person into the truth. Indeed it is only by this means, i.e., by deceiving him, that it is possible to bring into the truth one who is in an illusion. Whoever rejects this opinion betrays the fact that he is not over-well versed in dialectics, and that is precisely what is especially needed when operating in this field. For there is an immense difference, a dialectical difference, between these two cases: [a] the case of the man who is ignorant and is to have a piece of knowledge imparted to him, so that he is like an empty vessel which is to be filled, or a blank sheet of paper upon which something is to be written; and [b] the case of a man who is under an illusion and must first be delivered from that … Assuming then that a person is the victim of an illusion, and that in order to communicate the truth to him the first task, rightly understood, is to remove the illusion – if I do not begin by deceiving him, I must begin with direct communication. But direct communication presupposes that the receiver's ability to receive is undisturbed. But here such is not the case; an illusion stands in the way … What then does it mean 'to deceive?' It means that one does not begin directly with the matter one wants to communicate, but begins by accepting the other man's illusion as good money.
>
> Soren Kierkegaard, The Point of View for My Work as an Author, 39–41.

> One must start out with error and convert it into truth.
>
> That is, one must first reveal the sources of error, otherwise hearing the truth won't do any good. The truth cannot force its way in when something else is occupying its place.
>
> To convince someone of the truth it is not enough to state it, but rather one must find the *path* from error to truth.
>
> (Ludwig Wittgenstein, 'Remarks on Frazer's *Golden Bough*', 119 in *Philosophical Occasions*)

In the previous Section, I made the best possible case – a strong case, I believe – for considering Kuhn 'a Wittgenstein of the sciences,' and I rebutted the apparent difficulties raised for that case by the apparently most Idealist/Relativist moments in Kuhn. But: In my more pessimistic moments,[1] I sometimes see Thomas Kuhn as stranded half way between the mainstream American philosophy to which he wanted to belong but into which he doesn't

1 Moments largely unexplored in Sharrock and Read (2002), except in the Conclusion to that book.

quite fit and the more resolute and consistent Wittgensteinian dissolutionism to which he is often close. In other words: I worry, as others have worried, that perhaps the / my 'Wittgensteinian' interpretation of Kuhn is *too charitable*.

That worry, I explore here. I will eventually conclude, as 1.1 argued, that it is only (I suspect) on an inflated version of incommensurability, one which it looks like Kuhn is putting forward only when it looks like he is chiefly in debate with mainstream American philosophy,[2] that the widespread version of Kuhn considered in the present Section seems salient. The deflated / deflationary (Wittgensteinian) version of incommensurability presented in Sections (1.3,1.4) *is*, I would argue, closer to Kuhn's heart, and in more optimistic moments, I think that *this* reading of Kuhn quite clearly embodies his considered view.

The purpose of this Section is to *inhabit* for a while the most popular (and problematic) and worryingly-widespread interpretation of Kuhn on incommensurability. Only thus may we find the path from error to truth, hereabouts; only thus may we deal with what is not an ignorance of fact, but the product of philosophical delusion. For what I aim to do here is *precisely what Kuhn (and likewise the therapeutic interpretation of Wittgenstein) calls for*: to understand 'from the inside' this influential interpretation of Kuhnian incommensurability, so as to *really understand the attractions of it* as an interpretation (and the disastrousness, ultimately, of it as a 'position').[3] I aim to be as charitable as possible toward this reading of Kuhn (as hoist on his own petard, as self-refuting), and to learn in the process as much as we possibly can from it. I aim, that is, to accept provisionally 'as good money' the interpretation of Kuhn that has been most popular, to date.

Kuhn Applied to Kuhn

Kuhn wants to be able to understand scientific change 'from the inside', to be able to understand the past as (what) it was, not only as it is as seen from now. He thinks this is the best – even the most accurate – way to do it. He thinks that if we look back 'Whiggishly', giving an account of history as if it had always been teleologically aiming toward the state of the present, then, whether our subject-matter is social history or the history of science itself, we'll miss out on understanding the subject(s) in question. We ought not in effect to guarantee that the scientists of the past look like dull or misguided versions of our own scientists, aiming at the same truths but inadequately so. We ought rather to allow

2 Ironically, Kuhn's great misfortune was to have got hold of the attention of the mainstream of American philosophy, including some very major individual figures. If that hadn't have happened, it is likely that 'inflated' conceptions of incommensurability would not have arisen much in the first place.

3 Only when one has truly understood something's attractions/temptations can one potentially lay them to rest. This is a central insight of Wittgenstein's, as of Kierkegaard's.

how very different the scientific worldview of (say) Aristotle was from our own, in order to be able to see, from the right distance, how it can (could) after all hang together, and *be* science of a kind, constitute a way of seeing the world and a way of practicing science in it, initial appearances of its disastrous failure to be and to allow so notwithstanding.

However, this 'anti-Whiggish' approach of Kuhn's often results in him being taken to be a Relativist. In particular, he is attacked, even ridiculed, within mainstream philosophy of science for being a hard-line relativist *about truth,* for (supposedly) thinking that what is true for us cannot literally be said to have been true for Ptolemy, for Copernicus, and even for Newton.

Kuhn is *not* adequately characterised as any kind of hard-line Relativist about Truth. (He might be legitimately interpretable, as I will explain in 1.3, as (in a certain sense) a *very modest* 'relativist' about truth, and likewise about rationality, about what it is rational to think and do at different stages in the development of a science.) But: this still needs some further showing …

The question will be put to any (alleged) relativist: how can they put forward their own ideas as true, as rational, as deserving of attention, when they themselves teach that all ideas are true only in *context*? Why should we believe Kuhn when he makes claims on behalf of the truth of the doctrine that ideas are true only in context? How can he argue for the *superiority* of *his* view over the views of others with a straight face? Wouldn't e.g. the logical positivists be right *in their context?*

This is a cheap and unrewarding way to hang the 'relativism' label on Kuhn. Kuhn is not, as I shall show, a 'serious Relativist' about Truth, though he is certainly no classical Correspondence realist. Thus he does *not* have to say that his 'theory' cannot be generally true because it is a product of its times.[4] Kuhn may more plausibly *seem* to be relativistic about the *meaning* of theories in different paradigms; and *thus* about (the meaning of) 'scientific progress.' Kuhn certainly sometimes sounds as if he thinks that paradigms are mutually impenetrable (and thus that one cannot prove from an external point of view the superiority of one over another). Does Kuhn really think this?

Well; *what would be more natural, for a philosopher, to try to figure this out, than by looking at how Kuhn himself sounds and at how his emergence looks, from a Kuhnian point of view?*

It is hard to overestimate how much can be learnt and appreciated (about the quality and nature of Kuhn's contribution to the philosophy of science) by playing close (and, ultimately, reflexive …) attention to the question of 'Kuhn

4 Does Kuhn's own claim to knowledge pretend to a 'trans-paradigmatic' status? Well, before being worried by this question, which may be another way of too crudely setting Kuhn up for self-refutation, one should be careful here not to assume that Kuhn's own view can rightly be characterized as (the basis of) a paradigm. I explore this further below, especially when I consider Kuhn's claims self-applied in relation to the Philosophy of Science itself. In very brief: arguably Philosophy of Science is not (and is not strictly continuous with) Science.

applied to Kuhn'.[5] The reflexive question for Kuhn might give rise to a form of Relativism and an attendant self-refutation and is therefore potentially serious. Kuhn's very ability to describe paradigm-shift might then seem to undermine the 'thesis' of incommensurability. Kuhn doesn't need to explicitly be a radical Relativist about Truth for there to be a key sense in which his alleged picture of the 'different worlds' in which scientists live seems like a kind of relativism – what is sometimes called his 'conceptual relativism', or 'relativism about meaning'. If people live in different worlds, then doesn't Kuhn himself do so? Don't Kuhn's own ideas undercut themselves? *What happens if someone tries to take incommensurability about meaning generally, and seriously, and does so, as consistency might seem to demand, even with regard to what they themselves are saying/doing?*

The history of both the production and the reception of *SSR* does go some significant way toward *exemplifying* his theses. Arthur Danto has said [6] that that the special feature of *SSR* is that it can account for its own production and effects, including the resistance to it, in a manner that no previous philosophical picture of science could do. For example, all Karl Popper can say concerning the resistance to his own theory is that many philosophers and others are too scared to be bold thinkers, etc. – but this is essentially an external fact to Popper's picture, a psychological happenstance; whereas it is internal to Kuhn's account that there will be a great effort to maintain the prior paradigm in the face of a revolutionary incursion. This effort will even extend to the revolutionaries themselves. Thus, Kuhn emphasises not only revolutionaries' frequent reluctance to face the broader and ultimate consequences of their own innovations, but even to recognise that they truly *were* major innovations, and (moreover) to making those innovations in the first place. Kuhn finds Planck, for instance, to have been a much more reluctant revolutionary than has traditionally been thought. Kuhn quotes with apparent approval Planck's own telling remark that scientists rarely manage to convert to a new paradigm: a radically new scientific truth does not triumph by convincing its opponents, but because they eventually die.[7]

Kuhn emphasises the important role sometimes played by people who come into a discipline from outside (as he himself did – his training was as a physicist,

---

5 Though the *nature* of the learning, as I shall explain, shall in some respects by the opposite of what one expected. (And cf. also p.80 of Flyvberg's (2001), for some important hints as to why.)

6 In talks and in personal conversation.

7 This citation occurs in *SSR* on page 151. It might be asked at this point: if people are generally not 'converted', but just die off, doesn't that mean that scientific revolutions must generally see two or more schools co-existing, at least for a while, in a supposedly paradigmatic discipline? Well, yes. But this doesn't count against Kuhn; he is explicitly prepared to see allow for such situations. He explicitly allows that it is not uncommon in the course of scientific revolutions for the discipline's appearance to revert temporarily to that of its pre-paradigmatic days.

he then became a historian of science, and he was most influential in philosophy and methodology of science). Kuhn's idea here is that scientific revolution quite often occurs in cases which work out (of course, most such cases *don't* work out, and the results are merely pseudo-science, crankery, etc.) by means of people coming from outside the discipline who are not therefore so 'constrained' by the disciplinary matrix and who therefore have the capacity to think in fresher ways. Those ways often include the ways that were current in the discipline from which they have come.

It is important to be clear that this is very far from a methodological recommendation, in Kuhn. It is an observation based upon the reality of the development of science – but it could not possibly be a general recommendation, because *most* such importations of analogical models from elsewhere are disastrous / useless. And, normally, the best recommendation for what to do is: just do more normal science. (This is of course already assuming that the area in question *is* a science. We will look at this issue in Part 2). But one has to allow that sometimes revolution must occur, when the paradigm becomes 'monstrous' (Copernicus's word). And it is just a fact that revolutions have been helped by outsiders to the discipline in question, quite often. But that is all: there is no implication *at all* that (for example) it would necessarily be helpful to the project of 'making economics more scientific' to import into it (say) mathematicians with new ideas.

So then, following up this thought of Danto's that we have just mentioned: think of Kuhn compared to the philosophers of the Vienna Circle on science; in particular to Carl Hempel. Hempel's attempt at producing a Confirmational logic for science led to the desperate anomalies of his and Goodman's paradoxes[8] – but his own account of science has nothing to say about why any of this should have happened. Whereas Kuhn can point to the state of 'crisis' that philosophers of science were gradually, reluctantly realising their 'discipline' was in; and to the revolution that 'reluctantly' ensued when he radically displaced (*not* refuted/falsified!) the orthodox wisdom on the relevance of the history of science for the philosophy of science, on the dubious utility of the context of justification *vs.* context of discovery principle, and so on. (In this regard, Kuhn is like Freud, whose theory predicted that it itself would give rise to critical responses – unlike in the case of Hempel or Popper, this fact was *internal* to Freud's theory.)

Kuhn can explain his own emergence and the resistance to his views, including even his own resistance. A revolution in the philosophy of science should be expected only at a time when there is some perception emerging of anomaly or monstrosity; one should expect its fomentor to have his/her feet very much in the

8 We should note that recent research (see especially work by Thomas Uebel; cf. also Alexander Bird) is indicating that Rudolf Carnap, at least in his final years, was significantly less far from Kuhn than was the general Positivist line. However, as I indicated in the previous Section, this in my view likely to be a specific indication of the extent to which *Carnap's* views moved away from where he started, even to the point of being actually slightly influenced by Kuhn himself.

old tradition, and not even to realise at first the revolutionary effects of his/her own views; there should be communication difficulties of a major sort between the newly emergent paradigm and defenders of the old verities, and so on.

On this last point, of course, Kuhn made some capital from the problems which both he and the Falsificationists had in debating with each other – a point that infuriated his rationalist audience.[9] Kuhn endeavoured to present the 'cross-purposes which he discerned between himself and his critics as 'confirming evidence' for his picture of partial communication, the talking-through-each-other that characterises discourse between incommensurable points of view'.[10] It is important not to be deceived by the possible availability of a 'moderate' reading of these phrases and remarks. The term 'partial communication' might appear to imply only a careful equanimity concerning the allegedly-limited degree to which 'complete understanding' between humans is ever possible. Kuhn goes on to say that 'The inevitable result [of paradigm-shift] is what we must call, though the term is not quite right, a misunderstanding between the two competing schools' (*SSR*: 149). The term is 'not quite right' because in everyday language a 'misunderstanding' is correctable, however, in this case it is *not*; it is inevitable, intrinsically unavoidable. 'Communication across the revolutionary divide is *inevitably* partial.' (*SSR*: 149, emphasis added). This is a qualitatively different situation from everyday human situations. Kuhn is apparently implying that there is a signal and powerful form of 'incommensurability' here. Even when pre- and post-paradigm claims are made using the same words, there is no possibility of normal mutual understanding, *for those words must inevitably mean something different in one mouth/text than they do in another.*[11]

Kuhn's view of scientific development can be *reflexively* applied – we can productively *see* the philosophy of science *as* experiencing something like a paradigm shift. As Martin Hollis used to put it: Kuhn's thesis is revolutionary.

---

9 It is notable that Popper later conceded some ground to Kuhn here, possibly in part to draw the sting from the way the failure in the debate between them was grist to Kuhn's mill. Thus Popper (1994:63) admits that, after Kuhn's *Reflections on my critics* and after the Post-Script to SSR (2nd ed.), he realized that he had been attacking only views attributed to Kuhn, not Kuhn's own, when he castigated 'normal science' etc. He nevertheless maintained that the view he was attacking, including the 'myth of the framework' itself, was an influential view.

10 Lakatos and Musgrave, (1970: 231-2). See also Kuhn's remark: 'Inevitably, the term "cross-purposes" better catches the nature of our discourse than "disagreement".' (Is Kuhn entitled to these assertions? How does he magisterially know the nature of this dispute that he is a part of? That's the seeming-problem: that Kuhn is a part of history but claims to be able to see it too, to be able to reconstruct a part of it that *ex hypothesi* he is alien to. We get to this crunch issue shortly.)

11 For exemplification, see Kuhn's discussion of the shift from Newtonian to Einsteinian physics. In the second half of Sharrock and Read (2002), we suggest that there is a tenable moderate way to understand 'partial communication', provided one does not think that meaning – the product of meaningful use of words – is itself in principle what can only be partially communicated.

There are also key respects in which the 'Kuhnian revolution' does *not* support Kuhn's own picture. But this ought perhaps not to overly surprise us – for whoever said that philosophy was a science, a science which has for some time been at a normal science stage of development? (Well, of course, many philosophers have in fact tried to say that; though their grounds for doing so are, I would claim, thin in the extreme).[12]

The respects in which Kuhn's picture does not effectively apply to its own production and reception are respects which are quite comprehensible once we take fully into account that philosophy is not best regarded as a science, but rather as 'pre-paradigmatic' (to use Kuhn's potentially-quite-misleading term).[13] Kuhn's subject matter is essentially those disciplines with (single) paradigms. And philosophy (even of science) is not such a discipline. (Furthermore, as will be tentatively suggested in Part 2, nor are 'the human sciences', at their best.)

But Kuhn *himself*, as we shall see, does seem keen to self-apply his work. Where the real and immediate problems for Kuhn can seem to begin are if we think about applying his more radical, perhaps relativistic, philosophical claims to his own historical accounts and examples drawn from the sciences, and the history of the *philosophy* of science.

This is also why I said above that it is hard to overestimate the utility of thinking Kuhn applied to Kuhn. Here is the essential difficulty: If the defence of Kuhn offered in 1.1 above is rejected (or not considered) or found to be psychologically unavailing (that is, if one just can't bring oneself, at the end of the day, to believe it), if what can appear to be Kuhn's stronger 'claims' are correct, then pre – and post – revolutionary scientists live 'in different worlds' (*SSR*: 121). The 'incommensurability *thesis'*, if such it is, divides them. The previous paradigm becomes incomprehensible to them, even while they absorb parts of it into their new practices and textbooks (see *SSR*: 139), such that the revolution becomes 'invisible' (*SSR*: 136f.).

Now, it might be argued, there is no compelling reason to think that students of (the history of) science can effectively do something that the scientists themselves *cannot* (or if they can, then we just have some form of absolutism on our hands, anyway). At best, Kuhn *et al* may be good at doing something which scientists are not so good at doing; the grounds for attributing to them a skill which is cognitively *closed* to practising scientists are, by contrast, negligible.

Now, given that Kuhn (unlike, say, Feyerabend) insists firmly that one can *only* live in one world,[14] that a change of paradigms is a definitive 'conversion-

12 For discussion, see the Interview, below, that rounds out this book.

13 For Kuhn's most careful reappraisal of 'pre-paradigms', and of how what he is referring to here could be less misleadingly conveyed, see Lakatos and Musgrave (1970: 272). In Sharrock and Read (2002), we suggest instead the term 'disciplines without a paradigm', and labour to lay stress on how there is no teleology implicit in this notion.

14 See Lakatos and Musgrave (1970: 232) and SSR: 111-128; and below. As previously mentioned above, it can seem at the moment of crisis as if one is living in two

experience', it follows directly that one cannot mix and match between different paradigms. But then surely, this seems directly to imply, the historian or philosopher of science *cannot understand* what came before a scientific revolution. But then how can that student –how can Kuhn – efficaciously describe the state of 'the world' according to the old scientists' beliefs before the revolution? The historian can't then, it seems, really know what it would have been like to have lived and practiced in the world of the pre-revolutionary scientist.

An irony results from the above; the historical accounts of revolutions, supposedly Kuhn's key examples seem, by the logic of his own argument, cognitively inaccessible to him. He cannot understand the 'exemplars' which ought to have been the basis of his theory. If his 'theory' is correct, then, it seems, he cannot point to any examples to confirm *that* it is correct! For the better Kuhn's historical descriptions, the less plausible becomes his general view. This might be taken to explain why Kuhn's examples in fact invariably *do* seem to fall some way short of what have been taken to be his stronger philosophic 'claims'. It may also possibly explain why Kuhn sometimes implied or even (in his later work) explicitly argued that in fact he didn't *absolutely require* examples to evidentially justify his claims, and that sometimes they could even be illustratively unhelpful or misleading:

> [O]ne can reach many of the central conclusions we drew with hardly a glance at the historical record itself.[M]any of the most central conclusions we drew from the historical record can be derived instead from first principles. Approaching them in that way reduces their apparent contingency, making them harder to dismiss as a product of muckraking investigations by those hostile to science. And the approach from principle yields, in addition, a very different view of what's at stake in the evaluative processes that have been taken to epitomize such concepts as reason, evidence, and truth. *Both these changes are clear gains*. (Kuhn, 2000: 112, italics added)[15]

I want to go further: It seems that the effectiveness of Kuhn's philosophy of science *contradicts* the effectiveness of his historical descriptions and examples, *if* that philosophy of science is rightly understood to involve a theory of 'meaning-incommensurabilism' as standardly understood.[16] And the problem I have now laid out seems a pretty serious problem, which, can presumably be solved or mitigated

---

worlds; but this kind of schizophrenia just means that the paradigm is breaking up, and that one is at present barely able to 'live' (scientifically-speaking) at all.

15 Of course, there is a terrible risk in Kuhn's approach here of his undercutting exactly what he suggested in the justly-famous first sentence of SSR: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."

16 This is essentially Davidson's famous objection to Kuhn. We shall come to Davidson shortly.

(if at all) only by adjusting our understanding of the 'empirical (?) status' of Kuhn's 'theory'. Let me here anticipate it.

Kuhn has suggested a change of aspect, not made a set of (say) falsifiable claims.[17] Kuhn is arguably giving us – can perhaps only coherently be giving us – as Feyerabend almost alone recognised, *not* a general historico-sociological theory of scientific development, but rather a critique of abstract and formalist philosophy of science, intertwined with some partial and provocative suggestions toward a genuine philosophy/methodology of the sciences, moving away from the fairly disastrous fully-abstract and anti-historical perspectives which he found dominating the scene when he began writing. Importantly for the present book, we can sum this up by saying that *Kuhn would be better thought of as a methodologist than as a philosophical theorist.*

However; we must also – in thinking seriously about the question of whether Kuhn can make sense of himself, and of whether he is caught up in the dilemmas and contradictions of Relativism – consider Donald Davidson's (1974) [18] famous attack on 'Conceptual Relativism'.[19] According to Davidson, there is a great difficulty for Kuhn, consequent upon what appears to be his 'pluralised' Kantianism.[20] Kuhn wants us to imagine radically different – 'incommensurable – conceptual schemes *in science*;[21] but he is *also* aware that we can only ourselves ever be in – and then we must be *in,* up to our necks, with reams of non-grounded assumptions and ways of acting – one particular conceptual scheme.

Kuhn does not explicitly tell us *how to deal with* the devastating self-contradiction he appears to face; he simply presents us with his (very fine) historical case studies. For inspiration on how to deal with the problem identified here, we might try looking to anthropology and (in particular) in the philosophy

---

17 One hopes that, in this process, Kuhn himself is not 'deforming' the ordinary use of words (e.g. '(different) world', 'talking at cross-purposes', 'incomprehensible') already made by scientists and laypeople. Though, perhaps, if we look closely at how Kuhn actually uses these words, we will in due course see more clearly the possibility of a less extreme incommensurabilism than that which he can appear to be proposing: see 1.3, below (and 1.1, above).

18 Davidson's paper carries further forward the (broadly-speaking) Pragmatistic spirit and project of Quine's *Two Dogmas of Empiricism* (albeit, like Quine, very much in an Analytic idiom).

19 And on the last 'dogma' of (Kantian) Empiricism -- the dogma that there are 'conceptual schemes' in terms of which we understand reality. Insofar as Davidson is attacking Kuhn, for 'conceptual scheme' try to read 'paradigm'.

20 For an in-depth and fairly-convincing exposition of Kuhn as a Kantian, but one who believes there can be more than one phenomenal world (i.e. at least that one phenomenal world can succeed another, due to scientific revolution), see Hoyningen-Huene (1993), especially Chapter 3.

21 Here, he has been influenced also by the Sapir-Whorf hypothesis of Linguistic Relativity, of anthropologically determinable different conceptual schemes lying in languages.

of the social sciences. Kuhn himself compares his studies to enquiries requiring understanding alien ways of life. Perhaps Peter Winch's (1964) sympathetic but uncompromising critique of Evans-Pritchard shows a way forward. Winch takes seriously the extent to which one has to participate in an alien way of life in order to have a chance of fully understanding it – and emphasises that presupposing that one's own way of life is more soundly based in some absolute sense hinders, not helps, understanding an alien form of life. One has to know what it would be to take them seriously, to live them. And one cannot impose upon the 'aliens' one's *own* canons of even logical contradiction or coherence.

There are two potential problems in extrapolating Winch's method from here to Kuhn's case. Firstly, with regard to Kuhn, *if* we are to take some of the words and phrases he uses at apparent face-value – and ignore his qualifiers (for which, see again 1.1, above) – then we seemingly must think that the concept of incommensurability is stronger/deeper than anything in Winch.[22] Secondly, there may be special difficulties attendant upon the fact that it is *science* we are dealing with here. There may be limits[23] to the extent to which one can understand alien *scientific* pictures. For one thing, one surely cannot attempt to live them seriously again; one's understanding of contemporary science, beyond which there is no further court of appeal, forbids that (this is what Kuhn calls 'irreversibility'). One just can't attempt to do experiments to produce phlogiston, for example. At least in the case of the sociology of religion or of the anthropology of alien cultures, one has live people to interact with, and possibly one can more easily imagine adopting their way of life and beliefs, providing one doesn't presuppose that their way of life is incompatible with (e.g.) scientific beliefs that one cannot oneself give up.[24]

I will return to the Winch-Kuhn analogy later and repeatedly, developing it in some detail in the early portion of Part 2. The problem for now, in essence, is this: *There appear to be significant limits to the extent to which paradigms from history can be alien and yet be scientific pictures at all.* This chimes in with the slightly peculiar fact that if one is impressed by Kuhn's clearest examples of scientific revolutions (if one doesn't conclude that there is no serious incommensurability or unintelligibility) one is very often left wondering whether the pre-revolutionary science actually was science in our modern sense *at all.* This difference is perhaps most marked in the case of the chemical revolution, but is intriguingly present also in Ptolemy-Copernicus case and even perhaps in the Newton-Einstein case. Compare, for instance, Arthur Koestler's analyses of the extent to which in the

22 See 1.3 for the position I in fact take on this.

23 In the sense of Wittgenstein's *Tractatus.*

24 And even in the case of history, where there are no live people to interact with, there is still the difference between one's scientific world-view, which one cannot much if at all actually give up, and other kinds of beliefs or practices, which are perhaps never so definitively 'falsified' or superseded.

historiography of science we have systematically left out of account the huge non-scientific elements of the thought of Tycho, Kepler, and Newton.[25]

Kuhn emphasises that the role of 'external' history is greater in the formation or transformation of early 'pre-paradigmatic' science than in later (not just paradigm-based but professionalised) science, where the development of the professional apparatus, the intensification of the training and the demanding technical level of the practice all 'insulate' the scientists against the external environment… To say it again: Kuhn, seemingly wants us to imagine that we can understand radically different conceptual schemes; but he is also quite aware that we can only ourselves ever be in and understand for ourselves one particular conceptual scheme. One really is *inside* one's conceptual scheme. Thus, if Kuhn had really ushered in a revolution in the philosophy of science, and if what I have said above is correct, then it seems that one could not understand the philosophy of science that Kuhn has superseded![26] However, that at least *seems* false: it seems that, insofar as there ever was anything to understand in the Positivists *etc.*, it is still accessible to us.

Let us not be too quick to maljudge Kuhn here. Because he might quite reasonably claim that in fact we cannot understand Logical Positivism *as a live option,* and *completely,* any more. If that were so, he would still be faced with the Davidsonian dilemma initially presented above: Is the claim that we can't genuinely understand (e.g.) Positivism anymore? Or is it that we *can* understand it, if we make a special kind of hermeneutic effort, as Kuhn implies of Aristotle, of Carnot and of Copernicus, in those moments when he is getting us into their mind-set; and as he sometimes seems to imply similarly of those philosophical views he disagrees with? In which case, doesn't the alleged radical incommensurability (incommensurability of meaning) fail? Surely the claim is not that Kuhn has a godlike capacity to see what no scientist (or philosopher of science) can? Can we so much as understand a paradigm that we cannot / do not accept? This is the question which Kuhn can now seem ill-equipped to answer.

This Section has thus far represented my best effort to set out the difficulties Kuhn faces if he is some kind of strong meaning-incommensurabilist, and my best effort (1.1 notwithstanding) to set out why such an interpretation of Kuhn has seemed attractive. Let us explore these questions that I have raised thus far in this Section, by means of an investigation of the concept of 'incommensurability' via the philosophy of language.

---

25 See Koestler's *The Sleepwalkers.* This point also takes us close to Hacking's notion of 'topic-incommensurability', the thought that what people are working on (what area their science is (in) is different before and after a revolution.

26 And now we are back to what, at the start of this Section, I termed a 'cheap' and 'unrewarding' way of labelling Kuhn a relativist. It now seems, according to the interpretation that we have attempted to develop in the present Section, that we are in fact driven down this way. This is a clue to the failure of the interpretation under consideration in this Section.

And let us bear this in mind, as we proceed: To be in a position to settle these questions, we need not only to bear in mind the provisos expressed earlier about important respects in which *philosophy* ought not to be expected to conform to an analysis of *science,* but *also* to explore more fully what 'incommensurability' can or could actually mean.

Incommensurability: What Can 'Incommensurability' Actually Mean?[27]

The idea of incommensurability, meaning the lack of a common system of measure, originally comes from mathematics, from the shock which the Pythagoreans long ago got from realising that there must be numbers which are not expressible as fractions, which are not 'rational'. That the hypotenuse of a right-angled isosceles triangle is not a rational number in length if the other two sides are directly implies that the hypotenuse is in the final analysis incommensurable in its length to the other side(s). You cannot use the same ruler to measure them both with full accuracy.[28]

But this will yield us only a very tiny 'amount' of incommensurability. For, to *any* arbitrary less than 'complete' *standard* of accuracy, the 'incommensurable' sides of the triangle in question *can* be compared in length. How can we understand Kuhn then as saying something 'more' than what a Pythagorean would say here, as he clearly seems to want to. Once more, a comparison with Winch may prove helpful. Winch, like Kuhn, is interested in understanding – is interested in the very 'intelligibility' of – the 'alien' culture, not in the question of the truth of its claims. There is no sense, insofar as one's purpose is to understand the Azande way of life, in saying that their concept of 'witch' is incoherent, or even in saying 'Witches don't exist': that's just another way of saying 'We don't believe in Witches'.[29] What one *can* say, and this is *clearly* true, is '*We* just *don't use* terms/concepts like 'witch'. We absolutely/simply can't take *seriously* the idea that there are witches.' Much as the Einsteinian could say, without question-begging '*We* just don't use the term 'mass' in the way that Newtonians did; we have *good reason* to believe

27 These pages very occasionally draw on an earlier version of this material, in 'Incommensurability 1' in Sharrock and Read (2002).

28 For full explication, see Sharrock and Read (2002: 141f).

29 This is not quite right -- to be precise, what Winch says is that the social student or philosopher has no business imposing a standard of coherence upon the Azande such that the Azande concepts are incoherent, except insofar as there may be incoherence within those concepts which the Azande themselves are ready to acknowledge. Winch's specific point, of course, is that it looks on careful inspection like they make no such acknowledgement -- and so we must revise our understanding of the game they are playing, and learn to see it as not founded upon something incoherent. (Of course, my approach at this point is presupposing rather than arguing for the adequacy and pointfulness of Winch's approach and conclusions -- some readers may be unwilling to concede this. See 2.1, below.)

that the theories one gets out of using it that way will be less good, so far as we're concerned, than those we can get out of talking *a la* Einstein instead.' To say that would be far better, historico-philosophically speaking, than to say, 'You *can't* use that term in that way!' Such language-policing is pointless. It's not that one can't (though surely most busy and efficiency-minded practising scientists can't any more), it's that we just *don't*, and that we don't even begin to *take it seriously*, except if we are interested in understanding the *history* of the discipline. And again, what natural scientist *qua* natural scientist should have to be much interested in doing that?

***

Ian Hacking describes how a possibility like that which we have been discussing can be generalised:

> If the meaning of the theoretical terms or 'concepts' depends on the laws and theories, what happens when we revise or abandon the law or a theory? The radical conclusion.is that meanings change. Moreover if the same term occurs in two different theories, we seem driven to the conclusion that the term differs in meaning in the two contexts. Theoretical progress cannot occur by deductive subsumption of one theory under another, for that requires sentences meaning the same in both stronger and weaker theories, and that now seems ruled out. Even the crucial experiment founders. Here is a vivid statement of that possibility, by Paul Feyerabend: '[A] crucial experiment is now impossible. It is impossible not because the experimental device would be too complex or expensive, but because there is no universally accepted statement capable of expressing whatever emerges from observation.' .[R]ival or successive theories in the same domain are incomparable or incommensurable. (Hacking, 1975: 123–4)

Here, (radical) incommensurability means/is explained by means of the impossibility of understanding across (in Kuhn's case) paradigms. Now clearly, this must mean more than that one cannot do more than one thing at once. One cannot *simultaneously,* at one moment, in one laboratory, be a phlogistonic chemist and a Lavosierian chemist–this is an important point not to miss, but it is essentially trivial and obvious, once one thinks about it for even a moment; and neither can one alternate between phlogistic and post-Chemical-Revolution chemistry, as one would be able to if all that a scientific revolution involved was the opening up of the possibility of a gestalt-switch.

But is this a fair account of what *Kuhn* thinks? Well, before concluding that it isn't, we should at least bear in mind again how Kuhn thinks of the suggestion that Newtonian mechanics be taken as a special case of Einsteinian relativity, true as an approximation in circumstances involving only 'very low' velocities: he is completely untaken by the suggestion. To say it again: while being keen on the gestalt-switch metaphor as an account of the experience of the historian of science,

he points out that we ought not to *overplay* the analogy between standard gestalt-switch (e.g. with the duck-rabbit figure, and how it can switch back and forth before one's eyes) and gestalt-switch in science–because in science there is no going back.

Things get more complex, when we recall Kuhn's insistence that:

> [T]he derivation [of Newton as a special case from Einstein] is spurious. Newtonian mass is conserved, Einsteinian is convertible with energy. Only at low relative velocity may the two be measured in the same way, and *even then they must not be conceived to be the same*. Newton's laws [are not] a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed (*SSR:* 101–2; italics added).

Kuhn holds that Einstein overrode Newton, and that the two are logically incompatible. One can only be right if the other is wrong (*SSR*: 98). This striking statement from our alleged 'relativist' philosopher of science suggests how Kuhn's idea here is not only ranged against the thought of continual progress in science, but also against the thought (sometimes attributed to Kuhn) of old paradigms being true 'from a different point of view'. We can't seriously speak Newtonese any more – it is, according to Kuhn, far more dead than it is according to a 'point-of-view' relativist, or, on a cumulativist position about scientific-knowledge, e.g. Positivist. *Newton is not, for Kuhn, true from some different point of view, and nor is he even, strictly-speaking, approximately true. He is thoroughgoingly displaced and overridden.*

So here we appear to have a radical new line of thinking in the philosophy of science: but now *not* one, it seems, that can be understood coherently at all, according to the semantic-relativist 'paradigm'. For: the old paradigm is *not* 'true from an alien point of view'. *However*, one may still be alarmed by the *means* through which Kuhn argues against the 'approximately true' version of Newton. Does Kuhn's idea that (e.g.) the Newtonian concept of mass means something different from and irreconcilable with the Einsteinian rest on a reified, deviant conception of meaning, a conception of meaning as radically changing over time; and, if so, are we any nearer to avoiding a vision of incommensurability which will lead us into disastrous semantic doctrines, (and/or) into a self-refutation?

Let us once more quote at length from Hacking to attempt to shed light on this:

> There are three philosophical fantasies that we could label 'too much', 'too little', and 'just right'. The just-righters.claim there is just one right system of translation between any pair of languages. Philosophical debates currently [1975; and still to quite a large extent in 2010] focus on the claim that there is *too much* free play between languages to determine any uniquely best system

> of translation. The most famous exponent of this is Quine, who calls it the indeterminacy of translation.
>
> Quine urges that there is *too much* possibility for translation. The opposed doctrine maintains that there is *too little.* Two human languages could be so disparate that no system of translation is possible. This is in the spirit of Feyerabend's doctrine of incommensurability. He wrote about competing or successive scientific theories. To say that there is no system of translation between languages *E* and *F* is not to say that *F* is impenetrable to speakers of *E*. But the *E* people can learn *F* only in the childish way of learning mostly from scratch. After they have done so they realize that a preponderance of sentences of *F* (if indeed *F* has 'sentences' at all) have no expression in *E.* It is not just that individual words fail us, but that sentences, paragraphs, wondering, fears, questions, and jokes expressed in *E* cannot be represented in *F*. Languages, like incommensurable scientific theories, can on this view be learned by a man of good will but they do not necessarily lend themselves to translation. Some of the greatest anthropological pioneers write as if they *experienced* this incommensurability. (Hacking, 1975: 151–3)

A key point from the above is: the semantic incommensurabilist argues that there is not even one adequate translation between radically different schemes. Hacking is (in the above) deliberately charitable to the 'incommensurabilist' approach, and perhaps is trying to draw some of the sting from it before he even begins to evaluate it. However, even if we take this (charitable) interpretation there is a problem: Kuhn seemingly wants to be Quinian *and* Feyerabendian! He wants to talk of incommensurabilities as deep as or deeper than Feyerabend (recall that for Kuhn a paradigm is normally *entirely dominant* at any one time, [30] there cannot even be the contestation from other (albeit incommensurable) approaches that Feyerabend envisages; one is always, except possibly in the actual throes of crisis, 'in the grip of' one paradigm),[31] but *yet* he wants to talk of translation between different languages, *via* different schemes, as Quine does.[32]

Kuhn seems to wants to agree with Quine on the philosophy of language; and thinks he can do so by means of thinking of paradigms as *like* languages – languages which however may be non-inter-translatable. This is an uncomfortable attempt, it would seem, to be a 'too-mucher' *and* a 'too littler' at the same time.

---

30 We ought to note here that Kuhn later became slightly more sympathetic to the possibility of multiple paradigms existing within a genuinely scientific discipline. See e.g. (*SSR*: 178).

31 This picture is complicated by the 1970 Post-Script to *SSR*, but not in a way that changes the central point here – the overwhelming difference between 'pre-paradigmatic' and 'paradigmatic' disciplines remains in place, in Kuhn of 1970 vintage and thereafter.

32 See, for example Kuhn (*SSR*: 202). Here we see how Kuhn's efforts to 'play the game' of Analytic philosophy may have undermined his coherence and vitality.

Kuhn may think that this is possible on the grounds of Quine's disagreement with Davidson on the question of 'conceptual schemes'. Quine thinks that there remains room in philosophy for the conception of a conceptual scheme – Davidson does not. Davidson is thus a 'just righter', as Hacking says[33] – but not on the grounds that there is one really metaphysically true way of inter-translating two languages. The point is that for Davidson, the translation must take place and that there is therefore just no meaning to the purported idea of genuine multiple possible translation schemata. Davidson's is thus a 'transcendental' argument.

We will settle on a translation scheme, and that scheme, provided it does not issue in mistakes, will simply be the 'just right' scheme – insofar as it is coherent at all to talk of 'just right' here. Davidson's argument, whether he is aware of this or not, does not prove that there is only one conceptual scheme, which we all share; it simply proves that there is *no sense to speaking of conceptual schemes* in this context. Davidson, at his best, *repudiates* the entire 'too much *vs.* too little' dichotomy, and thus is not well-understood as a 'just-righter' at all. (The danger is that Davidson appears to leave no room for the very phenomena that motivated Kuhn's inquiry: the features of previous science that are peculiarly hard to understand, except as error or stupidity (which would take us back to Whiggery again).[34]

Kuhn doesn't appear to recognise that his would-be 'too-mucher' Quinianism is incompatible with the 'too-littler' Feyerabendian (or rather, what we are taking to be the distinctively semantic-incommensurabilist) elements of his own picture. These latter elements are those that suggest that there can be no way of 'capturing' in language alien *schemes* of thought. For Quine, alien schemes *have to be* the basis of possible translation manuals, and indeed, they must cash out behaviourially.[35] Kuhn wants to hold roughly the Quinian line – but at the same time to raise the possibility of utterly discrepant schemata, *incommensurable* schemata that cannot be adequately translated. There is no place for this that is compatible with the Quinian picture (let alone with the Davidsonian one) – if what Quine (or Davidson, for quite different reasons) were saying were correct, there would certainly be no way that one could be a serious semantic relativist.[36] And this is just another way of putting the dilemma that we have expressed several times now in this Section.

Thus, there still seem to be mutually incompatible elements in Kuhn's thought, and it is not clear how one might conceivably put all of his thought together. Because

33 On p.151, and cf. pp.153–4 for detail.

34 See Sharrock and Read (2002: 148f) for more on this front.

35 For Davidson, there is no such thing as an alien scheme, and so of course there is trivially no fact of the matter as to whether we have got an alien scheme right or wrong -- for it's just not meaningful to talk that way.

36 Contrast Chris Swoyer's Relativist incomprehension of Quine's (admittedly subtle and itself highly-problematic) position, on p.101 of 'True for', in Relativism: Cognitive and Moral (eds. Meiland and Krausz, Univ. of Notre Dame Press, Notre Dame and London, 1982.

it is not clear what we are to make of a 'conceptual scheme' that is described to us – and then we are told that we cannot really understand the description. Yet that is what (a meaning-incommensurabilist) Kuhn gives us – or *appears* to give us.[37] Kuhn has apparently *by his own lights* said the unsayable.

The irreconcilability of distinct elements in the position Kuhn seemingly wishes to put forward philosophically is such that we have to conclude that, insofar as it is meaningful ever to say that someone refutes themselves, Kuhn-the-relativist has done so. *If* this were Kuhn, then: he could not give us the history of science on a plate,[38] and then refuse us the meal. He could not both have his conceptual scheme cake, not even *a la* Quine, and acknowledge that the cake (as it were) *eats itself*.

***

Though Kuhn has received the attention of some of the most impressive modern philosophers, they have perhaps traded criticism with the legendary, more than the real, figure. The figure I have set out above, though not without prompting in Kuhn's text, is not, I believe, the real figure. Kuhn-the-relativist is *not* Kuhn – but how so? This Section has been an experiment, a way of testing out a powerful and popular (both with major mainstream American philosophers, and with many of Kuhn's 'followers') version of Kuhn's thoughts on what happens in paradigm-shift. In the following Section, I will try to lay out – on pain otherwise of having to convict Kuhn of a deep flaw in his philosophy – a reading of Kuhn which will find those moments in his work (moments marked by terms such as 'partial communication', 'incommensurable', 'different worlds', etc.) which I dwelled upon in this Section *to be reinterpretable in other ways than those I have here offered/explored*.

Thus I am about to engage in a different kind of experiment – not so much the application of what have often been taken to be Kuhn's views on incommensurability to Kuhn himself (for that experiment ought to founder on the

37 As it were, on this interpretation he gives us Aristotle or Ptolemy or Copernicus or Carnot on a plate, invites us to enjoy our meal – and then says that it is in principle indigestible.

38 Important here is Kuhn's antipathy to Whig history (of science). I am of course in hearty agreement with Kuhn here – provided one recognises that there are significant limits to the extent to which the 'avoidance' of Whiggism is in principle possible. There is no such thing as someone writing a history of science with NO preconceptions as to actual the nature of scientific phenomena, as to what some of the laws of science are, etc. – that, again, (a different part of) Kuhn himself taught us. If there is any meaning to talk of conceptual schemes – or paradigms -- then there is no writing the history of science without some recourse to them on one's own part; i.e., to one's own paradigm. One cannot see x neither as a planet nor as a satellite (if Kuhn is right – see SSR: 115. One may adopt a stance of methodological indifference; but one must always recognise the built-in 'limits' of such a stance, limits which have in part to do with the very identifying of some activity as in the field of 'science' at all.

difference, not always appreciated by Kuhn himself in *SSR*, between philosophical and methodological reflections on the one hand and science itself on the other), but (rather) the attempt to develop a reading which definitively *finds a sense* in Kuhn's own views on incommensurability. A sense, rather than the nonsense which Kuhn's views would amount to, as we have seen, if he were a 'strong incommensurabilist' about meaning. In order to escape the dilemma, the sense of Kuhn hovering between two incompatible desires that we have explored in recent pages, we need to find a way of decisively overcoming the Kuhn-as-semantic-relativist reading of Kuhn.

1.3 Wittgenstein and Kuhn on Incommensurability – The View From Inside

> [I]f anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize – then let him imagine certain very general facts of nature to be different from what we are used to, and the formation of concepts different from the usual ones will become intelligible to him.
>
> Ludwig Wittgenstein, *PI*, Part II section xii.

> 'So you are saying that human agreement decides what is true and what is false?' – It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life.
>
> Ludwig Wittgenstein, *PI,* 241.

In this Section, I take up the challenge explored in the previous Section from inside the perspective of Kuhn's critics, such as Davidson. Building on suggestions already made about how what Kuhn in fact offers us is a kind of view from 'inside' the history of science, an opportunity genuinely to understand what Aristotle or whoever was up to, and about how Winch offers a helpful model of this too, I endeavour to set out a picture, compatible with that already offered in Section 1.1, of what Kuhn on incommensurability actually offers. Thus broadly Wittgensteinian thinking here helps, I hope once and for all, to lay to rest the kind of dubious/relativistic 'incommensurability thesis' that threatens so to deform our understanding both of natural science *and* (by extension) of 'human science'.

What happens when paradigms shift? Successive paradigms overlap, in the sense that the successor carries on with much of the work of the predecessor. Much material is re-used from the earlier paradigm (Kuhn talks of it being 'cannibalised'), but, rather than being retained in its inherited form, it is recast in the 'wholistic' context of the new paradigm. For example, the vocabulary of physics ostensibly changed quite little in the transition from Newton to Einstein – but we have seen how Kuhn insists that the Newtonian concepts were in effect surreptitiously *thoroughly* cannibalised and remoulded. In this case particularly, the apparently *common* elements *dividing* the two paradigms – like the well-known two peoples / nations (the USA and the UK), well said by popular lore to be 'divided by a common language', *misleadingly* look like they are the same.

Kuhn offers, then, a way to avoid being taken in by 'false friends', when doing the history of science.

Kuhn wants one to be able to understand the Newtonian 'thought-style' or 'sensibility'; for this task, he believes that finding 'translations' (the scare-quotes around that word are important here, and below) of 'Newtonese' into 'Einsteinese' is not enough. One needs to be taught – as Kuhn hopes to help teach – how to pick up *nuances* and connections lost in any such Davidsonian or Quinian translation. From my Wittgensteinian perspective, it helps to point out the considerable extent to which what Kuhn is really talking about is, roughly, ways in which changes in paradigms engage in and can even be said to constitute the reconfiguration of grammar.[1] Language is not something that floats free, but something which is thoroughgoingly – utterly – interwoven with activities. 'Conceptual change' is integral to change in ways of organising activities: What words can mean depends upon their connection to, and part in, our activities. When Wittgenstein says that to describe a language is to describe a way of life, he does *not*, as Sapir-Whorf would have it, mean that the ordinary meaningful use of some particular language is in the end incomprehensible to someone thinking or speaking in the 'different ontology' of another language. That way, clearly, lies self-refutation.

What *does* Wittgenstein mean, then? Take the word 'king'. If we want to explain that *word*, we might start by describing the game of Chess. 'King' there doesn't mean the same as it does when it is the title of a British ruler – though there is some connection between the two.[2] It is perhaps a common mistake to follow the idea that a word gets its meaning from its involvement with an activity to the

---

1 For details on how I here understand 'grammar', see Guetti and Read (1999) or the early chapters of Read (2007a). For more on what I mean by this claim as a whole, compare the following fascinating remark from Wittgenstein's Nachlass (Thanks to Oskari Kuusela for pointing this remark out to me, and to Philip Wilson for the translation:) 'The difficulty only consists in understanding how determining a rule can help us. Why it calms us after we were so deeply disturbed. What calms is evidently that we see a system that systematically excludes those aspects that have always disturbed us that we did not know how to manage but that we thought we had to respect. Is determining a grammatical rule in this connection not like the discovery of an explanation in physics? E.g. of the Copernican system? A similarity is available.' (MS 112, p.119; emphasis added).

2 I exploit this kind of connection shortly, in suggesting the respects in which really grasping an ontology – a conceptual scheme – is very like grasping the full panoply of connections between words, or even between different senses of the same word. In other words, for example, perhaps the natural (to us) connection between 'king' in Chess and 'king' in the Buckingham Palace sense is necessary to a full appreciation of what Chess is; and likewise, one will be missing something central to Newton if one connects his concept of (e.g.) 'mass' with what doesn't naturally connect with it, and misses the connection between it and what naturally goes along with it in the Newtonian 'world-view'. The word 'mass' in Newton is perhaps connected with a world-picture in the kind of way in which the word 'king' in Chess is connected with literally a picture of how things are in the real, human (in this case, political) world. (The considerations Feyerabend used to argue for incommensurability between Newton and Einstein were not dissimilar – see Preston (1997: 106f) for a useful account.)

conclusion that only those who engage in the activity can properly understand its meaning(s). The word 'king', in the English language, is the name of a Chess piece and the title of a monarch – and it is clear that republicans can use and understand the word 'king' in the latter sense, in a perfectly ordinary everyday way, just as well as monarchists.

Kuhn tries to get us to understand periods / forms of science which we do not participate in – indeed, could not possibly participate in. He does so, in part, by getting us to understand the 'game' that was played with the words used by those scientists, and the ways in which those words were inter-related with one another, and with actual practice. The disappearance of an activity does not deprive us completely of the use of the language it is interwoven with – Latin teachers would have been living some big lies, otherwise. It is not as if, either, the word 'king' loses its meaning the minute we establish a republic. There is a sense in which a change in grammar alters what it is possible to say, and another sense in which it doesn't.

Compare Kuhn with what Wittgenstein says in 'On Certainty', a passage which encapsulates much of the message of the central chapters of *SSR*, and whose various elements can in almost every case be tied, if one cares to put in the work, to elements of Kuhn's own presentation:

> I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: It is the inherited background against which I distinguish between true and false.
>
> The propositions describing this world-picture might be part of a kind of mythology. And their role is like that of rules of a game; and the game can be learned purely practically, without learning any explicit rules.
>
> It might be imagined that some propositions, of the form of empirical propositions, were hardened and functioned as channels for such empirical propositions as were not hardened but fluid; and that this relation altered with time, in that fluid propositions hardened, and hard ones became fluid.
>
> The mythology may change back into a state of flux, the river-bed of thoughts may shift. But I distinguish between the movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other..the same proposition may get treated at one time as something to test by experience, at another as a rule of testing.' Wittgenstein (1969: sections 94–98)

This kind of change in grammar is, I think, absolutely central to what Kuhn means by 'incommensurability'. There cannot be any such thing as surveying from some external standpoint such changes in grammar. And they are not in the main well-understood as strictly semantic changes, on the standard

understanding of what 'semantics' is: for that would require that there were such a thing as a mode of comparing, and an 'Archimedean' position from which to compare, the meanings of the words before and after a shift in the state of the 'river', whereas all we can actually do is note the changes in patterns of use, and try to draw out how those changes *may be usefully said to yield a change in our way(s) of thinking*, or some such.

When Kuhn attempts to get one to understand conceptual schemes of days gone by, this is really, I believe, what he is about. Were any of us in the sort of circumstances in which Newton operated, we would have been Newtonians, and would not have found the difficulty we now do in finding Newton both persuasive and completely natural. This is Kuhn's harmless 'relativism' about rationality, which does not imply any endorsement of the mode of thinking in question; far from it. Compare once more Winch on 'witchcraft': Winch's arguments absolutely do *not* require us to endorse witchcraft (and indeed, we could not possibly do so), only to see that practicing it isn't some kind of dumb mistake or misguided pursuit of scientific objectives. What I am trying to do is to facilitate the reader's adopting an attitude toward 'incommensurability' that finds it to be more modest and reasonable, and less about (what 'mainstream' philosophy tells us is) meaning *per se,* than most of the philosophy of science literature would lead one to expect.

I think the key to the sense in which Kuhn can be some kind of 'incommensurabilist' without refuting himself is to see how hereabouts one needn't exactly be an incommensurabilist about *meaning*, nor even really about meaning considered as use. There is a strong sense in which a tenable version of 'incommensurabilism' is available which is not incompatible with Wittgenstein, nor with the best of Davidson (with what is tenable in Davidson's thought), if one focuses not on matters strictly semantic, but on other aspects of language and its practice, other aspects which as a first approximation might be put under the heading of 'those elements of language which one loses in a translation'.[3] (Like a translation from one form of mathematical representation to another: from reals to rationals, as with Pythagoras, for instance.) Those effects and *aspects* of a 'language-game' which are not simply transferable from one natural language to another, or which are missed if one simply focuses on what is being deliberately done or accomplished or communicated through the use of words.

3 These elements are termed 'grammatical effects' by Guetti (1993a) – see Guetti and Read (1999), a paper chiefly concerned with the delineation from meaning (from the 'meaningful consequences' of language use) of such effects; see especially pp.304–7. Guetti's discussions, which are relatively unfamiliar in philosophy of language, let alone philosophy of science, are I think superior in their philosophic 'productiveness' to (say) Gricean ideas which also and more familiarly complexify one's understanding of meaning and effects of words. I think that the Guettian contribution toward 'pragmatics' is substantial – and is just what is needed, to help understand what Kuhn can be and – we think – is intelligibly up to. A first approximation might be: not semantic incommensurability, but incommensurability of 'grammatical effects'.

What happens *in the seeing of aspects* is not strictly a matter of what mainstream philosophy calls meaning, of 'semantics' – but it is of real importance.[4] If, rather, we look carefully at the way in which something – although not exactly 'meaning' – is lost, gained or changed in these transitions, we may want to say that what is lost is the distinctive *aspect* – the physiognomy – of the language of one paradigm as opposed to the other.

This is why Kuhn comes over time to stress more the importance of the incompatibility of the languages of different paradigms in the same field. It is not exactly that one can't translate from one to another – one can, and nothing that might be termed 'strictly semantic' *need be lost* in the transition, the translation. But still there is a change of aspect – and it is this which means that successful translations of (say) phlogistic into Lavoisierian chemistry are necessarily only partially successful. *This* is what Kuhn can mean by 'partial communication'. Translation is not to be identified with understanding. We can have understanding, even though there is 'something missing' from the translation, because there is something more than successful *translation* that constitutes such understanding.

Here again, it becomes 'clear' why Kuhn found it so difficult to say what he wanted to say. For it is *difficult,* it takes hard work, to make the point that *I* am trying to make here. One is always tempted either toward some kind of gesture at ineffable mysteries beyond language, or toward some kind of straightforward theorisation (of the kind which philosophers normally give) of the domain in question, or both.[5] One is, that is, tempted toward an ineffabilism which cannot be satisfactory, and/or toward a theorisation of the semantics of the situation

---

4 One might start with Wittgenstein's discussion of 'meaning-blindness' and 'aspect-blindness' in Part II of *P*I. Wittgenstein does not deny that the 'meaning-blind' person could use language tolerably well in a basic way; what s/he would miss is all the subtler 'effects' which we are going to go into shortly. For further detail here, to see just the kind of thing we have in mind, the interested reader is advised to consult the best of those Wittgensteinian writings which focus on aspect-seeing. I have in mind particularly Guetti (1993b), Koethe (1996) and Mulhall (1990). Cf. also Stanley Cavell's work. (It is worth noting once more in passing that Kuhn had a long intellectual relationship with Cavell and his students, his particular Wittgensteinian 'school', at Harvard and elsewhere. (One of this 'school', James Conant, was appointed by Kuhn his literary executor.) This relationship may have something to do with the obvious prominence of aspect-seeing in Kuhn's account of revolution and paradigm-shift. It may also explain the close connection between the 'Cavell school's reading of Wittgenstein on 'logically alien thought' (which several essays in Crary and Read (2000) argue Wittgenstein rightly found to be a quite unintelligible idea) and the reading of Kuhn which in this Section I am arguing is the correct one: that Kuhn did not put forward the (unintelligible) thesis that different paradigms are semantically unintelligible to one another, such that one appears to the other not even to be (logical) thought at all, but rather the modest (non-)thesis that it requires work, and not a simple or plain comparison, to understand science very different from one's own.)

5 For a detailed account of such temptations and oscillation, see Part I of Read (2007a).

which will either yield paradox and self-refutation (as in the case of Kuhn applied to Kuhn as discussed in 1.2 above); alternatively, one simply fails to grasp the phenomenon, fails to see how there is a problem here which Kuhn is trying to respond to in the first place.

This is where most philosophers end up: thinking (*a la* Section 1.2, above) that Kuhn refutes himself, and that the only alternative is to deny that there is really any problem understanding past science, understanding across scientific revolutions, etc. . As should now be plain, I think this response is quite inadequate. There is a reason why Kuhn found most contemporary history and philosophy of science to be inadequate (and why much of it is still so). So we should follow Kuhn in trying to tread the difficult path between a kind of mysticism of paradigm-shift and an 'account' of paradigm-shift which suggests that there is no obstacle to standard formalist or realist accounts of the historical episodes in question, i.e. a denial that there is really anything worth calling paradigm-shift at all.

As Kuhn makes clear in his *Reflections on my Critics* there *is* in fact a minimal sense in which he is willing hereabouts to be regarded as a 'relativist' . That sense is conveyed in the following remarks:

> [There *are*] contexts in which I am wary about applying the label 'truth'.. . Members of a given scientific community will generally agree which consequences of a shared theory sustain the test of experiment and are therefore true, which are false as theory is currently applied, and which are as yet untested. Dealing with the comparison of theories designed to cover the same range of natural phenomena, I am more cautious. If they are historical theories...I can join with Sir Karl [Popper] in saying that each was believed in its time to be true but was later abandoned as false. In addition, I can say that the later theory was the better of the two as a tool for the practice of normal science.. Being able to go that far, I do not myself feel that I am a relativist. Nevertheless, there is another step, or kind of step, which many philosophers of science wish to take and which I refuse. They wish, that is, to compare theories as representations of nature, as statements about 'what is really out there'. Granting that neither theory of a historical pair is true, they nonetheless seek a sense in which the latter is a better approximation to the truth. I believe nothing of that sort can be found. (Kuhn, 2000: 264).

Thinking of incommensurabilism as being about 'meaning' but as compatible with something like an 'indeterminacy of translation' idea is the result of an unfortunate combination of Quinianism with a wish to hold onto a semantical attitude incompatible with it, and I am now in a position to suggest that this unfortunate hybrid was *not* Kuhn's. Rather, 'partial communication' is about speaking at cross-purposes, from bases which are not fully reconcilable with one another (and across

which there is no point and no available means of making serious 'verisimilist' calculations). *That* is what Kuhn is saying.[6]

If Kuhn is not usefully thought of as a relativist about truth in the strong sense in which that idea is normally meant, nor is he an incommensurabilist about meaning. But he does want to say that there is something worth calling incommensurability between paradigms; and he thinks that correspondence theories of truth are trivial or empty. Kuhn, unlike Popper, thinks that 'verisimilitude' is an empty and unnecessary shuffle. 'Correspondencism' works only when it is entirely unsurprising – when there is no live issue.[7] *When one has an uncontroversial ontology, one can compare what one says–one's sentences–with reality. But ontologically controversial claims, i.e. all claims made at times of crisis, thus at precisely the times which Kuhn speaks of – allow of no such adjudication.*

So, as we have already seen hints of, Kuhn thinks that Popper's 'verisimilitist' claims are of extremely dubious philosophic standing:

> To say, for example, of a field theory that it 'approach[es] more closely to the truth' than an older matter-and-force theory should mean, unless words are being oddly used, that the ultimate constituents of nature are more like fields than like matter and force. But in this ontological context it is far from clear how the phrase 'more like' is to be applied. Comparison of historical theories gives no sense that their ontologies are approaching a limit: in some fundamental ways Einstein's general relativity resembles Aristotle's physics more than Newton's. In any case, the evidence from which conclusions about an ontological limit are to be drawn is not of whole theories but of their empirical consequences. (Kuhn, 2000: 265)[8]

You can call this 'relativism about truth', if you want. If it is, then it is in a very particular and modest sense only. It doesn't do the harm that any strong relativism about truth (or meaning) generally does.

Scientific theories can be thought of as making claims about nature, of course. But Kuhn thinks there are two mistakes that must be avoided: firstly, the mistake

---

6 See Kuhn (2000: 233)

7 The point here is the very same point as Kuhn makes about crucial experiments – that crucial experiments, decisive choosers between paradigms, only exist in the mind of a historian (and a poor historian, at that – the kind of historian who isn't interested really in how things were at the time!). Crucial experiments, again, are only veridically identifiable in retrospect – once there is no live issue any more. At the time, they will always be open to conflicting judgement dependent upon paradigm-choice. They are only decisive when there is no longer anything to decide – when they are unsurprising and uncontroversial (i.e. not decisive; uncrucial!).

8 One might wish to object as follows: Can't Newton's propositions (as opposed to his ontology) be truer than Aristotle's (etc.)? The answer, once again, is: Sure; but only given a paradigm.

(which follows directly from a failure to understand the point of Kuhn's concept of 'paradigm') of thinking that there can be a direct comparison between theory/theories on the one hand and nature bare in tooth and claw on the other. Secondly, and still more fundamentally, one must avoid the mistake of thinking that there is any such thing as comparing the *elements* of a theory – its ontological fundamentals – with nature at all. One cannot compare a concept with nature; there is no such thing as doing so. To think there is, is to be the victim of an illusion of sense.

A powerful way of putting Kuhn's point here is to say that it is only *sentences* (and indeed sentences within an actual context of use) which can be intelligibly and unmisleadingly described as having meanings, uses.[9] Not words, nor concepts, and not integrated sets of words or concepts. *Words require a context* – absolutely minimally, a sentence. What words refer to is only cashed out *via* sentences with uses. It makes no sense whatsoever to speak of individual words as fitting or not fitting reality. A word can't be true or false! It is only claims – sentences in contexts – which 'fit reality' or otherwise. Thus point-by-point translation in a way never works or happens *at all* – its would-be units of meaning are too small.

Insofar as word for word, point by point translation can be said to happen (at all), it can do so only when the languages in question are relatively congruent, and when we have no trouble in uncontroversially actualising the concepts in question – for example, it is unlikely to be misleading if one translates *neige* as 'snow', *est* as 'is' etc. However, Kuhn rightly holds[10] that some languages (or aspects/parts thereof) are not like this. And, analogically, that sometimes there are shifts in the history of our thought such that even 'the same sentence' has a markedly different aspect, a different place in a system, a different set of connections with other sentences and concepts, at different times. (Think once more of Kuhn's account in *SSR* of the relation between Newton and Einstein.) 'The Context Principle,' the Fregean/Wittgensteinian principle that, philosophically-speaking, we can make no sense of isolated words or names, when put together as it naturally can be with the 'modest' version of Kuhn's thought concerning paradigm-shifts which I am putting forth here, has as a clear consequence the complete overcoming of Realist 'correspondencism'. However, correspondencism is *not* thereby replaced by a substantive Relativism.

Everyday language-users, and especially scientists, 'live' in one paradigm at a time (within any one given field): a contemporary physicist for example cannot seriously make *assertions* in Newtonese. This is what Kuhn calls 'irreversibility' in the development of science.[11] Kuhn is *not* saying that from a certain point of view, the old paradigm is still true. As we have already seen, he expressly holds,

9 See the work of Kuhn's literary executor, James Conant, on the Context Principle in Wittgenstein (and Frege). See also Read (2000a).

10 See in particular the close of this Section, below, and Kuhn (2000: 264–6).

11 See Kuhn (2000: 264). It is worth repeating this point, because it is fundamental: if Relativism were tenable in the form it is often taken to be – and taken indeed to be Kuhnian – then one could say that there is a point of view from which Newton (or Ptolemy)

for example, that Newton can be rendered a special case of Einstein (and thus remain true) only at the cost of *misunderstanding* Newton's theory,[12] where 'misunderstanding' is taken in the somewhat specific and peculiar sense which I am here endeavouring to construe. There is a real *clash* between paradigms for Kuhn, in a way which Relativist readings of him typically ignore or finesse, at the cost of completely missing Kuhn's point.

We might try as an 'object of comparison' thinking of paradigm-breaking science as akin to genuinely new poetry – which can (depending upon the reader) seem to be good poetry, bad poetry, or not poetry at all. Or, perhaps better, we sometimes encounter people saying things like: 'I can see what you're saying in a way, I hear the words, I know what you could mean if you meant them in an ordinary sense, but yet …'. This is often the case *in philosophy.*

When Kuhn is talking about incommensurability, the best analogy of all may be *to the situation of two persons or schools locked in ongoing philosophical misunderstanding.* (And again, this should hardly surprise us: for Kuhn is quite explicit that at times of crisis science sometimes starts to look like philosophy.) Such philosophical puzzlement, where what one side says must seem either pointless or nonsensical to the other, is virtually never resolvable through semantic clarification, through a simple mutual demonstration of meanings.[13] (The philosopher must practice a 'hermeneutic' effort to understand another philosopher–and just this is what is called for in the philosophy and history of science, too).

Relativism, as already suggested, tends to be deeply internally unstable (self-refuting, in fact), always leaning towards saying incompatible things at the same time. Relativism either claims that Newton and Einstein are saying the same thing in different ways (but that is not really any different from Realism, which states

---

is right. But Kuhn expressly (and repeatedly) denies this: 'Einstein's theory can be accepted only with the recognition that Newton's was wrong' (*SSR*: 98).

12 See the second half of 1.2, above. One should add to this that global judgements of truth or falsity are of relatively little use hereabouts, anyway. Ptolemy's system included many 'small' accurate observations; and all theories, even 'true' ones, are born refuted. All theories work – to some extent.

13 For typically, as Wittgenstein puts it, ' The one party attack the normal form of expression as if they were attacking a statement; the others defend it, as if they were stating facts recognized by every reasonable human being' (*PI* 402.) This, according to Wittgenstein, is what disputes between 'relativists' etc. on the one hand and 'realists' on the other are like. And even if one gives up such 'isms', the basic situation remains the same. Efforts at mutual understanding (in philosophy) are, for Wittgensteinians, beset continually by disconcerting little and large incommensurabilities. It is these which one spends one's philosophical time and effort trying to resolve: 'We mind about the kind of expressions we use concerning [everyday things – e.g. physics]; we do not understand them, however, but misinterpret them. When we do philosophy we are like savages, primitive people, who hear the expressions of civilized men, put a false interpretation on them, and then draw the queerest conclusions'(*PI* 194).

that Newton is a special case of Einstein) or it claims there is no understanding *whatsoever* possible of one from the point of view of the other – in which case it is a kind of extreme Scepticism, or 'pluralised' Solipsism, and will at the least result in the kind of self-refutation discussed earlier (especially in 1.2), if one nevertheless goes on to try to give any kind of account of the nature of past science.

Kuhn, by contrast, looks to carefully establish the sense in which paradigms clash, and to differentiate that from the sense in which claims *within* a paradigm clash (e.g. different claims as to the value of a particular constant). He is, in the final analysis, *by no means* the Relativist he has been painted as being.

Part 1 has been thus far an experiment in reading Kuhn, an attempt to see how far one can push out the boat of looking at Kuhn as a genuinely Wittgensteinian writer (1.1) or as a non-Wittgensteinian *theorist* (1.2), writing about an area about which Wittgenstein himself said little. My primary interest in 1.1 and 1.3 has been to explore some of these affinities by means of trying to think Kuhn's philosophical methods and indeed *style* at some difficult and crucial points in his work as alike to Wittgenstein's own.

The reader will have to judge how far the experiment has been a success. But can I suggest that that experiment is best pursued further, if one is so minded, by continuing to look at other moments in Kuhn's work, to see if it is successful there too. Insofar as it is, insofar as one finds Kuhn writing in a philosophically sophisticated and self-aware fashion, aware of the dangerousness, importance and near-inevitability of writing nonsense when one thinks the philosophical history of science in a way which is not tedious, timid or Whiggish, then I think one must tend to concur with my own judgement: that Kuhn was the greatest philosopher of science we have yet known, and one who was, if anyone has yet been, something like a Wittgenstein of the sciences, as maintained in 1.1.

I began the previous Section, Section 1.2, by saying that Kuhn is not a relativist about truth. However, as suggested above (in relation to the Aristotle-and-Einstein case, and again with reference to Popper) there *is* a limited sense in which he certainly *can* be justly called a relativist – and not castigated for being so. Kuhn believes that correspondence theory is either trivial or empty. It is trivial in cases where it is unsurprising: No-one could *disagree* with the truism that 'Snow is white' is true if and only if snow is white. But it is an empty shuffle – and a dangerously misleading one (because it may seem non-empty) – when applied to interesting cases:

> To apply [the semantic conception of truth] in the comparison of two theories, one must…suppose that their proponents agree about technical equivalents of such matters as whether snow is white. If that supposition were exclusively about objective observation of nature, it would present no insuperable problems,[14]

14 Presumably, Kuhn uses the words 'no insuperable.' here because he believes the problems presented are still severe, even in the case of 'objective observation'. Not only the straight scientific difficulties, but more importantly the difficulties posed by the way in

> but it involves as well the assumption that the objective observers in question understand ' snow is white' in the same way, a matter which may not be obvious if the sentence reads 'elements combine in constant proportion by weight'. Sir Karl [Popper] takes it for granted that the proponents of competing theories do share a neutral language adequate to the comparison of such observation reports. I do not. (Kuhn, 2000: 265–6) [15]

None of this need be seen as producing 'idealism' or 'relativism'– except perhaps in the minimal senses which I have allowed. The nature of what Kuhn means by 'incommensurability' has been missed by nearly all of Kuhn's friends and foes, because, by means of attributing to Kuhn a (self-refuting) semantic *doctrine,* they have wrongly assumed that Kuhn must be committed to some strong relativist doctrine.

Above (see also Sharrock and Read (2002) in the latter portions of 'Incommensurability 1') I have suggested that what gets 'lost in translation' between paradigms is more or less what McGilchrist would describe as essentially 'right-brained' material: the *how*, rather than the *what*, of how the world is. 1.2 above sought again and again to understand Kuhn in terms of a what: *what* it was/is that is allegedly lost (or gained), when one translates knowledge from one scientific paradigm to another. But what if this is essentially the wrong approach? What if what was lost / gained just cannot be adequately presented within such standard quasi-scientific categories? What if it is instead closer to (for instance) 'what' gets lost in the translation between two languages of some piece of literature, or between an artwork and its interpretation?

Then it could not be simply: some*thing* that was meant and cannot any longer be, in the new paradigm: which idea leads one into semantic relativism/ the incommensurability 'thesis'. The risk of self-refutation starts to look far less once one understands that it is unhelpful to regard Kuhn as pushing a 'thesis' or a

---

which Kuhn holds that scientists 'see' according to a paradigm (e.g. the different ways in which Galileo and Aristotle saw the pendulum).

15 Kuhn goes on to add that 'If I am right, then 'truth' may, like 'proof', be a term with only intra-theoretic applications.' This may sound alarmingly like a strong Relativism about truth. But one should note how Kuhn immediately continues, and explicates his 'position': 'Until this problem of a neutral observation language is resolved.', and Kuhn believes of course that it will not be resolved (see the note immediately above, and see also Kuhn, 2000: 234–5), where Kuhn writes 'confusion will only be perpetuated by those who point out that the term is regularly used as though the transition from intra- to inter- theoretic contexts made no difference.' In other words, Kuhn's point is not to make a dogmatic denial of the inter-theoretic validity of uses of the term 'truth', but rather to draw one's attention to differences. Viz., in this case, the difference between an intra-theoretic application of the term 'truth', and an inter-theoretic application of same. He is concerned that most philosophers, including Popperians such as Watkins, fail to see that there is any difference that makes any difference. The difference made, I have suggested, is a very important difference of aspect.

'theory' at all. Similarly, the whole Davidson-based critique of Kuhn et al suddenly looks much less relevant: Because it had *assumed* that incommensurability had to be a matter of semantic *content*. A *what*. It omitted a possibility: that what was lost was more akin to *style*, or to a whole *way* of seeing/doing. A way of *taking* the world. Something more processual, more like a style: something more how-like than what-like.

Sapir-Whorf tried to express this, but ultimately blundered: they ended up converting the differences between radically different 'world views' into something that itself could allegedly be *seen*, or indeed relatively-straightforwardly *said*. No: if you turn the how of such radical difference back into a what, back into a *content*, then indeed you end up mired in a theoretical stance, semantic relativism, and are justly eliminated by Davidson et al. But this wasn't where *Kuhn* ends up.

Kuhn has a 'view' with far less in the way of dogmatic theoretical commitments than most of his opponents have presumed. Thus those opponents of his have almost all failed actually to come to grips with the subtle and modest view (or non-view – it certainly isn't well described as a *position,* or a *doctrine,* or a *theory,* at any rate) which Kuhn actually has. They have spoken past him, and failed to contradict him. If Kuhn is read as I suggest he should be, as someone reaching for the kinds of felicitous formulations which were perhaps more often found in Wittgenstein, and not in abstraction from the lessons concerning the history of science that, *via* certain 'exemplars', he wishes to teach, then he has a lot to offer. He neither self-refutes nor is refuted by his opponents in the philosophy of science. For, after all, he cannot refute himself nor be refuted *if he doesn't have a* 'position'.

This is the real challenge: not to insist that Kuhn be read as having a *position* (be it 'relativist' or otherwise) *at all.*[16] I believe that a full understanding of Kuhn (of who Kuhn is, at his best, as someone with 'something' new to say) leaves one in something like the non-theoretical non-position of Wittgenstein. This non-position seems to some a wilful and culpable 'quietism',[17] or a continuing ambivalence which must be settled: 'Are you a relativist or a realist, or somewhere in between? *Decide!*' Thus Wittgenstein is sometimes accused of being ambivalent between, on the one hand, the kind of 'conceptual relativism' allegedly present in any talk of 'the formation of concepts different from the usual ones'[18] and, on the other hand, an quasi-Davidsonian 'anti-relativism.' This latter 'position' is allegedly present in

---

16 It is peculiarly difficult to succeed in being read in this way, as not committed to a 'position', for reasons that Wittgenstein's 'therapeutic' approach and McGilchrist's 'right-brained' approach make clear: According to the left-brain, according to the thesis-driven, more-or-less 'analytic' philosopher, according to the would-be normal-scientist, it is impossible for there to be any such non-position. It just makes no sense; there is no *room* for it and no calling for it. Thus again and again, Kuhn is read *through the left-brain*; and again and again then he fails to be understood.

17 For rebuttal of the charge, see Cerbone's essay in Crary and Read (2000).

18 As quoted earlier, from page 230 of [Part II of] PI: ' .if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean

Wittgenstein's famous remark that 'If language is to be a means of communication there must be agreement not only in definitions but also … in judgements' (*PI* 242). It is sometimes thought that the kind of agreement 'transcendentally' required by the latter rules out the kind of deep conceptual differentiation seemingly allowed for in the former.

I see no such contradiction. *PI* 242 consists of grammatical remarks, which is to say, reminders for *particular* purposes. If one is talking with someone who thinks that our concepts could or must be 'absolutely the correct ones' and thinks that it makes sense to compare our concepts with reality, then it may be wise to cite Wittgenstein on the formation of (different) concepts. If one is talking with someone who thinks that there can be complete, pervasive, total breakdowns of communication, of the kind suggested by some Sceptics, Solipsists or Relativists, someone who thinks that there can be no comparison of one person's or culture's concepts with another, then it may be wise to cite Wittgenstein on agreements in judgement (though see also 2.2, below, for a possible exception to test this rule with). Such citations would be the starting-points in discussions, attempts to mutually comprehend.

Kuhn mostly found himself writing in an intellectual *milieu* in which the dominant 'positions' were cumulativist or correspondencist or both. So he spent most of his time doubting that our concepts were or even could be absolutely the correct ones, as it were. But that strategic decision, which perhaps he regretted later in life, no more makes him a Relativist than it does Wittgenstein (or Winch).

For, take the wonderful remark of Wittgenstein's from *Culture and Value* that what a Copernicus or a Darwin really offered us is best-seen as not a true scientific theory but a fertile new point of view. A new *way* of seeing, a new set (as Lakoff and Johnson might put it) of live metaphors. A new… *world*. [19] That is what Kuhn was talking about in his self-awarely-paradoxical invocations of 'new worlds'; and that is what is of the essence of what I offered here in 1.3.

And so we see that the structure of Sections 1.2 and 1.3 has been thus: Firstly, in (1.2), I went with the philosophical flow, and took a 'strong' construal of Kuhn's 'incommensurabilism' on board. On this construal, taking for example the phrase 'partial communication' in a semantically 'straightforward' fashion, I established that Kuhn's 'relativism' would self-refute. Now, in 1.3, I have offered a 'deflationary' way of taking Kuhn that disposes of the common misconceptions of him by offering a better alternative, a genuinely charitable and useful way of reading him on incommensurability. On this construal, taking 'partial communication' as connoting not an irrevocable semantic barrier, but rather important 'non-semantic' aspects of language-use, I established what we might actually take the *point* of Kuhn's talk of

---

not realizing something that we realize – then let him imagine certain very general facts of nature to be different from what we are used to.'

19 See 1.1 above; and Chapter 4 of McGilchrist (2009), on how only the right-brain can give us a new world, and a different world from that of the left hemisphere.

'incommensurability' to be.[20] Beyond obsessions with semantic content, and with 'left-brain' scientistic reductions of Kuhn's methodological reorientation into mere 'semantic relativism'.

One might choose to express this point as follows: Kuhn has a very modest 'relativism' about truth (i.e. he finds no positive use in the philosophy of science for talk of 'correspondence' or 'verisimilitude'); Kuhn has a very modest 'relativism' about meaning (i.e. while no relativist about 'meaning as such', he could be said to be one about 'connotations' or 'grammatical effects,' those aspects of words which amount to the 'atmosphere' or 'feel' of a thought-style); Kuhn has a very modest 'relativism' about rationality (i.e. while he thinks the *concept* of rationality is clearly directly positively applicable to virtually all scientists, he also thinks that different *conceptions* of rationality operate in different paradigms).[21] One might choose to express my point thus; or one might not. One might simply say, instead, that Kuhn instantiates a non-theoretical mode of philosophising which sits ill with any attempt to pigeon-hole it within tired philosophical categories such as 'realist' and 'relativist'. That a proper understanding of the point of talk of 'incommensurability' goes beyond what friends or foes of 'relativism' have comprehended.

Great philosophers invite us *to reconsider our categories*. Scientific revolutions, Kuhn argues, invite or require scientists to do the same. This is perhaps the deepest sense in which Kuhn's thought on science actually *can* productively and unmisleadingly be applied to his own work. Kuhn is inviting us to reinvent the philosophy of science, largely by means of re-imagining the history of science. Only those who would resist his revolution by any means necessary should really want to slot him into categories with which we were familiar before his time (e.g. categories such as 'relativist'). When Kuhn wrote *SSR*, not 'all the relevant conceptual categories were prepared in advance' (*SSR*: 55). If the Kuhnian revolution in philosophy of science is to be realised, we all need to stop trying to pretend that we know into which pre-prepared category (*if any*) Kuhn can be said to fit.

Section 1.2 was entitled 'Kuhn and incommensurability: an interpretation.' It now becomes starkly evident that the interpretation I offered there *is not an interpretation I endorse*. It is a widespread interpretation, an attractive one. I inhabited it, *in an endeavour to exhaust that attractiveness*.

The current Section is called 'Wittgenstein and Kuhn on incommensurability: the view from inside.' Here, I have offered a Wittgensteinian take, parallel to and extending that offered in Sharrock and Read (2002), of how Kuhn's concept of 'incommensurability' functions as a way of enabling the reader to understand

20 Furthermore, I take this to have been almost entirely unrecognised in the Kuhn 'literature' to date.

21 One might add to these, as suggested much earlier: a very modest 'meta-relativism', i.e. a modest 'relativism' as a hermeneutic strategy, as a research strategy. This basically amounts to the overcoming of 'whiggism'.

'from the inside' conceptually-surprising phenomena such as scientific revolutions. (From inside the change in, as we might put it, *form of life* that such phenomena necessarily involve… – see the epigraph that opens this section).

My effort in 1.2–1.3 has involved*, in toto*, a (parallel) form of understanding 'from the inside': I have endeavoured, as I did not in my *Kuhn*, to offer the reader an understanding *from the inside* of the attractions of reading Kuhn as a semantic relativist, and (thus) a practicable way of genuinely overcoming those attractions. Thus the design of this Part has itself (thus far) been thoroughly therapeutic in conception: having set out a radically Wittgensteinian vision of Kuhn in 1.1, I have endeavoured radically-therapeutically to defuse the primary obstacle to accepting it: the ongoing attractions of the popular 'irrationalist' / 'semantic relativist' reading of Kuhn. By the time one reaches this point, the end of 1.3, one is perhaps – I hope – ready to *accept* a Wittgensteinian way with Kuhn, and Kuhn as a kind of proxy for Wittgenstein *vis-à-vis* the sciences.

This gives one a platform from which to start to look at the non-natural sciences. And one reason that this is important is that, beyond Empiricistic etc. fantasies of 'common measure', what 'incommensurability' has to offer us is: an *understanding* of the kinds of cases in which understanding is genuinely hard, and not a matter of mere theory-building, nor of acquiring 'more data', etc. (I explore in detail some such cases in 2.1–2.3, below.)

For, understanding Kuhn aright, finding him a kind of 'Wittgenstein' among the (natural) sciences, and seeing him as offering us our best picture of the nature of science, we place ourselves ideally to understand the 'sciences', if such they are, that concern human understanding: namely, the 'human sciences'. We are well-placed to approach them in their individual specificity and in their possible difference from the cases with which Kuhn himself was focally so brilliantly concerned. Thus armed, it is to them that I will shortly turn after two more shorter sections, which will aid that turn: Beginning, in 1.4, by looking at Kuhn once more on this central concept of his, incommunsurability – but this time, the apparently different and comparatively neglected 'incommensurability of [scientific] values'.

1.4 Values: Another Kind of Incommensurability?: On Incommensurability of *Values* in Science[1]

> I am suggesting … that the criteria of choice … function not as rules, which determine choice, but as values, which influence. Two men deeply committed to the same values may nevertheless, in particular situations, make different choices as, in fact, they do. But that difference in outcome ought not to suggest that the values scientists share are less than critically important either to their decisions or to the development of the enterprise in which they participate. Values like accuracy, consistency, and scope may prove ambiguous in application, both individually and collectively; they may, that is, be an insufficient basis for a shared algorithm of choice.
>
> Thomas Kuhn, *The Essential Tension*, 'Objectivity, value-judgement and theory-choice', page 331.

I have so far considered in full detail the philosophically-'radical' and potentially-problematic form of 'incommensurability' for which Kuhn is most famous, incommensurability to do with meaning*, etc.* But (perhaps especially in light of the very serious doubts raised in Sharrock and Read (2002) and in 1.1 and 1.3 above, about whether Kuhn, except possibly in certain incautious formulations (which are transcended as one comes to understand better what he is really about) really *is* a semantic incommensurabilist after all), it might be asked whether there is not another, rather different, kind of incommensurability that actually is to be found in Kuhn's work. This is the kind of incommensurability which is quite widely discussed in Moral and Political Philosophy (and in Economics) – incommensurability of *values.*

In other words, say, not the question of how / whether two people can *understand* each other, but the question of whether what two people *believe in* can be intelligibly given a common measure, can be 'reconciled'. The importance of considering this for the general approach of the book is once again to make clear how Kuhn is no Relativist theorist, and thus to head off pre-emptively a potential profound misunderstanding of Part 2 of the book (as a set of exercises in Relativism).

Now, the reason for thinking that this question is of importance in relation to Kuhn's work is that Kuhn himself, in at least one point in his work, explicitly

---

1 Many thanks to Fred D'Agostino, whose work and words provided the inspiration for this Section. Thanks also to the late Nadine Cipa and to Wes Sharrock.

and in detail discusses the operation of different values in science, in scientists' determinations of what to believe, assert, and do. In his major essay 'Objectivity, Value Judgement and Theory Choice',[2] Kuhn tries out a list of the values which scientists use and perhaps require in making choices between theories. His preliminary list is of five such values:

1. Accuracy,
2. Consistency (both internal and more general),
3. Simplicity
4. Scope
5. Fruitfulness.

Kuhn is here, as he makes quite explicit, not differing from the best of the traditional historians or philosophers of science to any very great degree. Indeed, in this paper, he is trying fairly self-consciously to appear 'modest' and 'moderate', unlike his 'Kuhnian' followers. This is perhaps partly why the paper appears, from my point of view, slightly methodologically 'conservative'. It does not entirely stick to the negative philosophical hard-line that I would argue characterises most of *SSR*, but sometimes makes forays into slightly more traditional questions in the 'applied' sociology and psychology of science – e.g. the quasi-Functionalist question of what kind of looseness of criteria for evidence or theory choice is most conducive to the structuring of an effective scientific community.

Having said this, it is very important not to be overly affected by Kuhn's apparent 'conservatism' in this paper. One way of avoiding such an error is by noting clear-headedly the way that Kuhn ends the paper – with several pages of discussion of how, really, his discussion should be radicalised by reading into it various points he makes elsewhere:

> I have throughout this paper utilized some traditional concepts and locutions about the viability of which I have elsewhere expressed serious doubts. For those who know the work in which I have done so [chiefly, in *SSR*], I close by indicating three aspects of what I have said *which would better represent my views if cast in other terms*, simultaneously indicating the main directions in which such recastings should proceed. The areas I have in mind are: value invariance, subjectivity, and partial communication. (*ET*: 334, italics added)

Kuhn's criterion of 'fruitfulness' is the only novelty on the above list, if there is one. Kuhn goes on to remark as follows (and it is here that I find his real novelty compared to the tradition):

2 This essay can most conveniently be found in Kuhn's *The Essential Tension* (Chicago: U. of Chicago, 1977), henceforth ET.

> [T]wo sorts of difficulties are regularly encountered by the men who must use these criteria in choosing, say, between Ptolemy's astronomical theory and Copernicus's, between the oxygen and phlogiston theories of combustion, or between Newtonian mechanics and the quantum theory. Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another. (*ET*: 332)

The second point is of course very important, as Kuhn goes on to make clear. But in the first instance it is the first point that most interests me, in the present context. That the criteria are ambiguous and open-ended, and, moreover, that this may not even become known or evident, because, of course, scientists do not spend their time settling with each other definitions and applications of terms such as 'simplicity', 'fruitfulness', etc. .

Now, there is something here which seems to satisfy the desiderata with which I opened my discussion. We have here something which is more than mere weak pluralism, more than mere apparent multi-dimensionality, and which resists the move toward a dictatorial value, or a meta-value. In particular, in the first point – that 'the criteria are imprecise' – we have something that looks like involving an incommensurability of values, in that even one value 'itself', let alone the various values in combination, may irreconcilably differ from person to person, from scientist to scientist.

So, have we found here an instance, a (new) type – perhaps less vulnerable to philosophical critique than that central to most of the discussion in previous sections on *semantic* incommensurability – of incommensurability, *of value*? But perhaps the reader has already anticipated what my reply to this will be. No, there is no real difference in type here. Because this 'incommensurability of value' here is not radically different to what Kuhn *actually* means (as we have discovered, in 1.3) by 'incommensurability of meaning'; surely, in fact, this is pretty obvious.

For: Not being able to give a common measure to two values – how could this idea of (non-) comparison really *not* be intimately related to problems of incommensurability of meaning and understanding, once these are understood aright? If we turn again to Kuhn, we find that precisely one of the main instances of 'incommensurability of meaning' to be found in Kuhn is in the *different understandings* of words – e.g. words like 'mass' or 'velocity' – which 'members' of different paradigms have, and indeed which 'members' even of the *same* paradigm have, as they sometimes find to their surprise and cost if and when a crisis erupts! So, if what is meant by 'incommensurability of value' is that words like 'simplicity' and 'consistency'[3] 'mean' very different things to different people at different times – against different backgrounds of world-view and paradigmatic-theory, given different temperaments and interests, *etc.* – then, and this should not

3 Against 'value invariance', Kuhn thinks that these two are the most historically variable – i.e. that what these two terms *mean* has changed very greatly over time.

I think much surprise one, it turns out that 'incommensurability of value' and 'incommensurability of meaning' are not really much different at all, after all.

The incommensurability of value consequent upon the clash of different and unsubsumable values involves scientists' values coming together in more or less indeterminate ways (as rules for action, values alone or in combination are, Kuhn says, 'intrinsically incomplete' (*ET:* 333)) and yet issuing in action. In other words, scientists do somehow decide what to do – but why exactly they are doing it is potentially just as much up for grabs as what exactly it is they are doing. And one can at least say this: that an 'absolutist' account, guaranteed in its validity from the standpoint of one's contemporary correct knowledge of the way the Universe works, of what they (scientists) are doing *or* of why they are doing it, is surely in principle unavailable to one. There is no such thing as *the* correctly-balanced set of scientific values, nor even of an absolutely true, 'perspective-less', understanding of past science. There are only ways of approaching these that help overcome actual difficulties that we are having in understanding them.

In other words: For the same reason that Kuhn (in his most famous work on incommensurability) urges us not too quickly to think that we know *what* scientists from a defunct paradigm are doing (how it ought to be described), we ought not too quickly (drawing now on his thinking about incommensurability of values) to think that we know *why* they are doing it. These are really two sides of the same coin. Incommensurability in values in science is just another way of looking at the way that the world of the scientist (in roughly the kind of way that we also speak of 'the world of sci-fi fans' or 'the world of celebrities') changes when paradigms change, and at how when extraordinary science is underway scientists cannot guarantee any more that they share a methodology. What a scientific value *means* becomes up for grabs, in a situation of paradigm shift. And with that remark, it is clear how difficult it is to get a cigarette paper between incommensurability of meaning and incommensurability of value.

These thoughts of mine are buttressed, again, by Kuhn's thoughts toward the close of his paper about the variance over time of values, of their weighting and of their *meaning;* and by his thoughts on the sense in which 'discussions surrounding theory choice' are problematically affected by 'partial communication'. He disputes that 'the facts appealed to in such discussions are independent of a theory, and that the discussions' outcome is appropriately called a choice.' (*ET*: 338). Kuhn's ideas seem to stand or fall together: his suggestion of a kind of incommensurability of values is not independent of his suggesting an incommensurability somewhere in the vicinity of meaning, of paradigms. What Kuhn is about here is simply helping us to get clearer, in cases where we may have difficulties, about methodological etc. choices that scientists make.

One needs in all this to beware an over-intellectualist version of scientific change and discourse. Kuhn reminds us of the important respects in which science (e.g.) does not involve 'choice' between theories as traditionally-

construed.[4] There is no algorithm for scientific change. While those very rare moments when the option of 'theory choice' is a live and forced option may be just as often decided by the nitty-gritty of what one has set out to do, what one has a hunch about, what is practical to do with one's equipment, as it is by recourse to the 'values' one is purportedly trying to instantiate in one's scientific work. Explicit 'values' are in the main even more remote from the lived reality of scientific practice than the formalist's 'theories' between which s/he imagines the scientist testing and choosing. (Though of course we should be fully alive to values that are implicit, too: Cf. the epigraph to this Section.)

So, perhaps it is not so surprising after all that these 'two' forms of 'incommensurability', traditionally found in very different philosophical 'literatures', are in fact best understood as in fact not disjoint at all. We have seen that the 'incommensurability of values' is a misleading expression, *unless* that 'incommensurability' has significant aspects which *are* in fact broadly-speaking 'semantic', or at least are understood as directly analogous in important respects to (what Kuhn actually means, as made clear in 1.3 above, by) 'semantic' etc. incommensurability. And in the end this would only surprise someone who failed to understand the following elementary philosophical 'truths', 'truths' which the work of Wittgenstein, Winch (See Part 2, below) *and* Kuhn reminds us of:

- 'Knowledge' and 'intelligible use of language' are *normative* (and thus in a sense value-laden) concepts;
- Values and beliefs alike are best not identified in the abstract but *in action* (including but by no means restricted to linguistic action);
- Human values are of course only identifiable through processes which among other things involve *understanding the meanings* of others' words and sentences, and understanding the full panoply of associations which make words mean what they do to individuals.

In short, only a serious failure to comprehend the sense in which values and meanings thoroughly interpenetrate could lead one to assume that it would be intelligible to separate off anything worth calling 'incommensurability of meaning' from anything worth calling 'incommensurability of value'.

The examples Kuhn attempts to give of the former are examples which we could equally describe (if at all) as examples of people trying to attain different goals (e.g. Ptolemy and Kepler can be well-described as having signally different goals and values, the same with Newton and Einstein, and even with Planck and Einstein); and the examples common in the 'literature'[5] of ethical dispute of people having values which deeply and perhaps incommensurably differ are examples

4 See also my criticisms of the widespread 'voluntarism' found in the philosophy of science – see the Conclusion (especially) of Sharrock and Read (2002).

5 Including in the 'literature' critical of Rational Choice Theory – see for example the work of Michael Taylor.

which may as well be described as examples of people *meaning* irreconcilable things by, e.g. the phrase 'a good life'.[6]

To assume that 'semantic' and 'value' incommensurability – the question of someone's / a community's theories and the question of radically different sets of values – can be thoroughly separated is, in fact, already *to beg the question against* 'world-views' which would not wish radically to separate out theories and values (such as the 'Pragmatist-Wittgensteinian' worldview). To assume that what is meant by 'incommensurability of value' can be something quite other than 'incommensurability of meaning' would thus be to beg all the really interesting questions – concerning communities and their differences – that Winch and Kuhn devote themselves to. In the name of clear thinking, logic and rationality, it would *obscure* from view a large *tranche* of human phenomena; including much of the practice of the sciences.

In conclusion then: Once we have understood the nature of the approach I argued for in 1.1 and 1.3 above aright, we come to see incommensurability of (scientific) values as *naturally* akin to a new *way* of seeing (and *this*, a new *way* of seeing and of doing – a new 'how', as McGilchrist might put it, rather perhaps than a new 'what'[7]– is what I have argued Kuhnian incommensurability above all aims to draw one's attention to). *Not* a new *view*, *not* 'semantic relativism' as a theory, but what *matters* in one *way* of seeing as opposed to in another. *This* is what incommensurability of value is all about. This is how Kuhn's discussion of it allies it directly and integrates it into meaning-incommensurability as discussed in 1.3 (and 1.1) above – though therefore *not* as such to standard interpretations of 'the [alleged] incommensurability thesis'.

***

Before turning to Winch *et al*, before turning explicitly to those human phenomena and their understanding and explanation, to ways of seeing and doing, let us take one more final and brief look at Kuhn. In the light of our reflections in Sections 1.1–1.4 on paradigm-shifts, what should we say about Kuhn's 'model' of science, Kuhn's idea of a paradigm? How should we understand the 'model' that some have extrapolated to suggest an allegedly Kuhnian way of going about social/human science?

6 For a powerful discussion of perhaps irreconcilable versions of this phrase, see Colin Lyas's (1999: 91f) account of Peter Winch's views on the great difficulty of understanding others – their theories and their values.

7 See Chapters 4 and 5 of McGilchrist (2009).

1.5 Does Kuhn Have a 'Model' of Science? [1]

> The aspects of things that are most important for us are hidden because of their simplicity and familiarity. (One is unable to notice something – because it is always before one's eyes.) … // Our clear and simple language-games are not preparatory studies for a future regularization of language – as it were first approximations, ignoring friction and air-resistance. The language-games are rather set up as *objects of comparison* which are meant to throw light on the facts of our language by way not only of similarities, but also of dissimilarities. // For we can avoid ineptness or emptiness in our assertions only be presenting the model as what it is, as an object of comparison – as, so to speak, a measuring-rod; not as a preconceived idea to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.)
>
> Ludwig Wittgenstein, *PI* 129–131.

The question of the present Section is whether it is helpful to see Kuhn as having a 'model' of science (at least, in anything like the sense of the word 'model' employed *in* science – in this regard, the present Section builds on the argument of 1.2, above, as well as on Wittgenstein's thinking, as for instance in the epigraph above), and of the relation of this to questions of science policy. I make herein the suggestion that allegedly Kuhnian relativism or allegedly anti-Kuhnian 'social epistemology' (which are actually remarkably similar to each other…) offer an equal poverty of policy advice, and that a better move is toward the thought that Kuhn facilitates for us a good sense of the considerable extent to which science (but not technology) thrives on and is entitled to a freedom from broader societal dictates.

As we have seen, especially in Sections 1.2–3, there is no 'foolproof' method of presenting a set of ideas, especially in an environment where readers are often likely to be impatient, casual and unsympathetic. One often cannot blame an author for the fact that many readers have been misled by their reading, especially insofar as they have been misled by preconceptions that are projected onto, rather than derived from, the author in question. Such has tended to be Kuhn's fate. (For, as we saw in 1.1 especially, sympathetic reading can overcome such projections, even when (as we saw in 1.2) they are prompted by understandable temptations with regard to how to take the text.)

Let us take as an example Steve Fuller's (e.g. 2000) lengthy, repeated and not uninfluential accounts of Kuhn. Fuller, though he may have attempted a more 'rounded' survey of Kuhn's background and character than is usual, has been no

---

1 An earlier version of the present section was co-authored with Wes Sharrock. Does Thomas Kuhn Have a 'Model of Science? *Social Epistemology* 17(2), 293–296.

less impatient, casual and unsympathetic in his reading of Kuhn than have those who, in his view, have – as a result of Kuhn's allegedly malign influence – taken a wrong turning in their understanding of the political situation of science.[2]

In relation to Fuller's account of Kuhn, as in relation to some others, much turns upon Kuhn's supposed model of science, especially in the way that it (allegedly) reifies the features of one (relatively brief) period of science into a general model of science. The vast changes which recently, through and after the warfare state of the Cold war, have transformed the context within which science works, do not appear in Kuhn's account. At best, then, Kuhn's model of science is seemingly outmoded.

But simply: does Kuhn *have* 'a model of science'? Not really. I will not of course deny that there is the simple schematic composed of the terminology Kuhn sets out in *SSR*. Readers of his book seem to think they have adequately understood his ideas if they have (a) read that book and (b) got some handle on this vocabulary. The words 'paradigm' (later decomposed into 'disciplinary matrix' and 'exemplar'), 'normal science', 'revolution' and 'incommensurability' are the key words, and the ones on to which most readers settle their attachment, quite unlike Kuhn himself.

But do these terms comprise a 'model of science'? For Kuhn himself they are more a 'heuristic'. They are, we might venture, closer to being a model in the sense of a Wittgensteinian 'object of comparison' than to comprising a pseudo-scientific 'model'. They offer a perspective, a way to see which can bring certain things into prominence, which were heretofore hard to notice. As we saw in 1.2 and 1.3, there are serious dangers in thinking of Kuhn's vision of paradigms and normal science as *itself* constituting a scientifical account. *If one takes Kuhn to have a model of science, one is covertly taking Kuhn himself to be some species of scientist*. We saw, especially in 1.2 (when we tried to apply Kuhn to himself, as a 'scientist-philosopher' of science, rather than just a philosopher of science), that this results, ultimately, in disaster. Fuller, like many others, covertly treats Kuhn in this way – and the results are unhelpful and misleading.

Kuhn's positing of paradigms as an 'object of comparison' for science, which can help to deliver a truer understanding thereof, serves the purposes of (a) deflating the late-empiricist conception of scientific growth and (b) guiding the writing of the history of *singular episodes in the development of science*. Kuhn was sufficiently relaxed about his distinctive terminology to make no use of it in his only major full-length post-*SSR* scholarly study of an historical episode in the development of science, his account of Max Planck's wrestling with the problem of black body radiation and the 'quantum'. Soon after its publication Kuhn had to write a supplement to the volume explaining that though the *words* that he had made famous in *SSR* were not to be found in the black-body book, the ideas that he had attempted to express *were*, broadly, present in it. However, the important thing was to avoid converting these into a dogma about the history of science.

2 In fact, he has been *more* so – see Read (2005).

Is Kuhn's a quasi-sociological account of science? Kuhn certainly emphasises, as even Popper had done before him, that science is a 'social institution', and that scientific thought can be affected by the nature of the involvement of that institution with the other institutions and the culture of the wider society, as was manifestly the case with respect to the Copernican revolution. Again, though, this point was not to be converted (*pace* the 'Sociology of Scientific Knowledge') into a dogma, for the extent to which the development of scientific thought was directly and strongly shaped through the influence of extra-scientific social forces *was quite variable*, depending upon the configuration of social, cultural and scientific situations. There was a tendency for the degree of influence from 'the social' (in the sense of the wider sociocultural context) *onto the content of paradigms* to diminish in proportion to the (associated) increase in the technical character of the problems and in the professionalisation of the field around a paradigm. The difference between the two cases is manifest in the contrast between Kuhn's own *The Copernican Revolution* and his work on Planck, for while the former makes much play with broader cultural influences on astronomy, the latter encompasses little of the 'sociocultural' kind, save the difference that being expressed by an established figure can make to the acceptance of an idea. Kuhn also stressed – but did not especially follow up – the point that science involves more than just theories and ideas, that the development *of instrumentation* for empirical work is an important element in the development of science.

Whatever avenues might have been thereby opened up for 'sociological' treatment, Kuhn retained a focus on his own project, which was – consistently throughout his career – what at one point he identified as understanding 'change in scientific belief'. Understanding the nature of conceptual change (in science). But what kind of problem is this? Is it an empirical-cum-sociological one, or is it a philosophical one? It was *always* a philosophical/methodological one, though it was only latterly that Kuhn came to regret that he had spent more time than he now thought was strictly necessary on his historical concerns. This goes against the idea that Kuhn's 'model' was an invitation for historical / sociological elaboration, but leaves this question: what is the relation between the empirical / historical and the philosophical here?

Sociologists and historians might understand Kuhn as asking an empirical / causal question: What are the *causal conditions* for a change in scientific belief - what conditions will bring an area of science into a state of crisis, and what conditions will determine the way in which the crisis is resolved? What, e.g. will determine which party will be victorious in the revolutionary struggle? Given, of course, that one can find instances of crisis, revolution and so on.

I would not want to insist that Kuhn does not make (occasional, sometimes casual) causal comments of this kind, nor need I do so, to make my point, which is that Kuhn's problem is fundamentally not empirically causal in this sense, but is philosophical/methodological in nature. His inquiry into 'change of scientific belief' centrally asks *this* question: what *kind of change* is a change in scientific belief. It is, therefore, historical examples of instances of change that

do not provide *evidence* for the testing of Kuhn's (causal) claims, but *material for reflection* upon *what we can intelligibly say* about the kind of changes that ensue from the appearance of a piece of 'revolutionary science'. The mainspring of Kuhn's career is then his insistent denial that the succession of scientific ideas involves a logically continuous replacement of earlier by later, thus giving pride of place to the theme of incommensurability (as explicated in 1.3–4, above), and the attempt to specify the kind(s) of discontinuity involved.

Put another way: if a new scientific idea is not logically compelling, then what kinds of attractions can give it appeal? Whilst recognising that an idea may appeal to a diversity of preferences – religious, ideological, aesthetic and so on (see also 1.4, above, on this) – and stressing that these might be decisive to the adoption of the idea in a particular case, Kuhn nonetheless centres attention on the preferences that pertain to an idea's scientific status. The question is, again: *what kind* of change is involved, what standards are used in science to assess progress there? He maintains that the requirement for fundamental change is not primary (hence the concept of normal science) and determinedly outlines the 'normal' achievements of science, such as determining fundamental constants, increasing precision and the like.

I am not arguing about the specifics of Kuhn's case, here, but about its *character*. It points to factors which have determined the course of (a) science or the outcome of certain revolutionary struggles, but it makes no particular attempt to give a systematic account of the forces – 'external' or 'internal' – that direct the course of science. It is not that kind of inquiry, but one that centres on the specification of what is involved (in the sciences) in identifying one piece of scientific work as an advance over another, identifying the main bases on which such a judgement is made. The appreciation that the science is predominantly a matter of problem solving (under conditions of normal science, of the sub-type 'puzzle solving') and that an important element in the appeal of a new idea is its capacity to 'raise the game' and present more demanding problem-solving challenges means that there will be a tendency – across areas of science – for the level of work to become more sophisticated and technically difficult.

Further, the long term development of the natural sciences has resulted in proliferation, and in accumulation of a very substantial – very detailed, and very technical – knowledge of nature, in significant part as an outcome of the puzzle-solving, paradigm-shifting nature of the exercise. Such long-term development has seen change in the institutional setting of science, changes in the balance between science and other cultural systems within the societies of the west, in the organisational settings of scientific workers, in the social location and status of those who become scientists, in the connections between science and other organisations in the society – universities, companies, the state – but – if Kuhn's is

a reasonable description – then the pattern of paradigm-shift is one that is entirely compatible with substantial changes in the institutional form of scientific activity.[3]

It would not be in the spirit of Kuhn's treatment of his scheme as a heuristic to convert it into a dogma that would insist that scientific development *must* continue to follow the pattern of paradigm-shift (after all, Kuhn himself points out that some areas of science do cease to change in this way, and 'become engineering'). However, while it is entirely possible that there may be changes in the nature of *natural* science that bring to an end the pattern that Kuhn describes, nevertheless, were that to happen it would not invalidate Kuhn's account. If scientific paradigm-shifts ceased to take place or ceased to involve the kinds of preferences that Kuhn identifies, then change would no longer conform to Kuhn's so-called 'model', but that would not of course mean that it *never* did so.

There have been massive changes in the institutional structure of science throughout the twentieth century and on to the present day. The fundamental changes in physics at the beginning of the twentieth century are perhaps the most fundamental changes in the nature of physical sciences during that time (which, rather than any nefarious political evasiveness, as wildly and conspiratorially alleged by Fuller, might explain Kuhn-the-historian's interest in this period), marking a fundamental change in the very nature of physical science, and – eventually – giving great impetus to the institutional shifts that have taken place, especially the involvement of science with military and state secrecy, not to mention the massive growth of the universities, and the increasing incorporation of science into commercial laboratories. However, there is no clear or substantial reason to suppose that these changes have made a difference to the issues which were the central focus of Kuhn's concerns, the characterisation of the grounds upon which an area *of science* will fundamentally change its loyalties. The fact that military or commercial preferences might sometimes be important drivers – along with, though perhaps in the contemporary world, instead of, religious and ideological ones – of scientific direction may be a valid observation, but it is already accommodated in Kuhn's observation on the role of extra-scientific influences upon the balance of preference within a scientific grouping. It is not easy – and perhaps impossible – to uniquely extrapolate from Kuhn's 'model' any conclusions about the significance of such institutional changes for natural science as problem solving, nor can one extrapolate from it Kuhn's personal views on these matters.

3 Note that, while this is largely so for science, it does not follow that it is so for technology. The trajectory of development in genuine sciences probably remains very largely Kuhnian, even under corporate near-domination; but technologies and forms of engineering (e.g. Genetically Modified food) may be much more open to ideological direction in very roughly the manner that Fuller depicts. One interesting question about the development of some human sciences, e.g. economics, is whether they tend in fact more to the character of technologies than of sciences, in this respect.

One final observation: Kuhn emphasises that the natural sciences tend to become very detailed, and very technical. He (rather regretfully) notes that there may be a price to pay for the knowledge of nature that has been acquired, in terms of the exclusion of laypeople from a proper understanding of what goes on in any reasonably advanced area of science. The increasing professionalism of an area of (natural) scientific work is presumably as much a consequence of the access-restricting feature of front-line scientific work, that it typically – if not quite invariably – requires intense familiarity with the work-and-results-so-far, which work and results are packaged in highly technical forms, as it is that which produces the exclusiveness. There is nothing to stop anyone accessing the major journals for any significant field of scientific work. Whether they could then read them is another question. The exclusion works both ways, of course, for the obvious reciprocal of Kuhn's point is that the scientists themselves are, outside the area of their special and specific competence, only just more members of the society.

So, 'scientists' in general can claim no monopoly of competence on how to politically control science. One does not have to choose between them ('the community of scientists') on the one hand and the new rhetoricians of the 'construction' of science, namely the so-called 'social epistemologists', on the other. Rather, science policy must always be a matter of the reconciliation of a very specific scientific specialism on the one hand and the social polity as a whole on the other. Kuhn's adaptation of Conant has offered one of the very best ways ever devised for some at least of the polity to be educated as to the actual nature of scientific change.[4]

***

Our 'account' of Kuhn as a Wittgensteinian among the (natural) sciences is now complete (or at least: as complete as it is going to get). We can turn our attention explicitly now to what has already clearly crept into this Part as an emergent topic of interest: the question of how a Wittgensteinian sensibility should be manifested among the 'human sciences'. Especially, among those disciplines keenest to portray themselves as inheriting / wearing a true scientific mantle. My approach in this Section already suggests one way in which the present work will undercut

4 Fuller's attempt to poison Kuhn's reputation, through bizarre and unsubstantiated claims as to Kuhn's alleged hidden political agenda, was presumably designed to undermine the possibility of people learning from Kuhn about the history and philosophy of science. That is one reason among many why I agree with Thomas Uebel, in his masterly paper demolishing the pretensions of the totality of Fuller's recent writings of science, 'The poverty of 'constructivist' history (and policy advice)', (In M. Heidelberger and F. Stadler (eds), *History of Philosophy and Science* 2002, pp. 379–389) that *it would be a terrifying thought that someone like Steve Fuller might nowadays be giving a lead in matters of science policy.*

that ambition: by urging that it is not in the final analysis helpful to cast philosophy itself as science. If, as Peter Winch explicitly argues, 'social science' itself is to some considerable extent philosophical in character (in his famous phrase: 'misbegotten epistemology'), then we already see here one powerful reason for positing a totally different character to social etc. science than to natural science, and for worrying about whether the use of the appellation science then in that context is more misleading than helpful.

Let us then turn explicitly to examining Winch and how to be a Wittgensteinian among the human sciences. To facilitate the transition, there follows a short 'Inter-Section', standing back momentarily from the concerns of either Part of the book considered by itself.

Inter-Section: An Outline Wittgensteinian Elicitation of Criteria

This little interlude, aiming explicitly to bridge from Part 1 into Part 2, consists of a schematic Wittgensteinian elicitation of criteria *vis-à-vis* a number of words / concepts that have already pre-occupied us above, and/or that will do so in what follows. Consider then the following terms, almost but obviously not-quite randomly-selected: *language, game, science, money, chair, bird, seeing, seeing-as.*

- *Language:* This, as was mentioned much earlier, is Wittgenstein's key example in (the early parts especially of) *Philosophical Investigations*. He invites one to consider a sequence of ever-complexifying possible for-instances of language (beginning with the seemingly ultra-simple / 'basic' case of the 'builders', in *PI* 2). The question he invites one to address, again and again, is the following therapeutic reader-oriented question: Is *this* enough to lead you on reflection to want unqualifiedly to use the term 'language' as a name /a descriptor? Or *this*? Or *that*? And in fact this process proceeds right through the book: very notably, in relation to so-called 'private language'. As Wittgenstein's subtle complexification proceeds, one comes to notice features of putative languages that seem important to their *being* usefully adjudged languages: viz., a certain *size* (a certain 'bigness'), some element of *reflexivity* and *normativity*, etc. .
- *Game:* This case of course is very explicitly addressed by Wittgenstein, in section 65f. of *PI*. As part of his concomitant investigation of what criteria one will actually all things considered assign to 'language', Wittgenstein points up how 'game', and by analogy 'language', is a much-less unified concept than we are inclined to think (though his analysis is *not* a nominalist one: The crucial final sentences of *PI* 65 are often neglected: 'Instead of producing something common to all that we call language, I am saying that these phenomena have no one thing in common which makes us use the same word for all, – but that they are related to one another in many different ways. And *it is because of this relationship, or these relationships, that we call them all* '*language.*' (Emphasis added.))
- *Science:* Somewhat similarly: There are very different types of things that surely are sciences. Think for instance of botany. More, probably, than 'language' or 'game', 'science' is also a *value*-term, an honorific. Perhaps like with 'language', a certain 'size' seems a pre-requisite for a happy application of the term 'science', too. In the light of Part 1, I hope it is clear how Kuhn's highlighting of paradigms and exemplars can help one to reach

a state of some peace with regard to thinking about how and when to apply the word 'science'; and how further examples/comparators may (in Part 2) help us gain further clarity, about some of the more contested cases, as emerge for instance in some of the following:

- *Money:* 'Money' is an ultra-functional term: Money is as money does. It is as we might put it saturated or constituted by its social role. It is thus also thoroughly reflexive. (As I suggested in the Lecture-Transcripts above, and shall follow up in 2.4 below, this makes money a poor candidate for scientisation.)
- *Chair:* 'Chair' is an artifactual kind (to use Nelson Goodman's useful term). It is a fairly function-based term, and chairs are prototypically designed.
- *Bird:* 'Bird' is an example of a term that can perhaps be harmlessly and usefully scientised. Then (but not before) it has sharp criteria demarcating it from other 'natural kinds'. *Once* it has such sharp criteria, a Roschian schema for it is a complete irrelevance, a point unfortunately rarely noted. Considered as an evolutionary natural kind,[1] things either are or not birds. It is only when considered as an ordinary language kind or as a 'psychological' kind that it could conceivably make any sense at all to ask 'Is this a typical bird?' And even then, such questions are always in danger of toppling into the kind of absurdity that such decontextualised questions continually court, at least if the answers to them are supposed to tell us anything whatsoever about the criteria for the correct application of the concept of 'bird'.[2]
- *Seeing:* Like with many concepts, it is important to observe a fairly sharp distinction between metaphorical and literal uses of this term. 'Seeing' is a very widespread term of our ordinary language.
- *Seeing-as:* This is a much less ordinary term, though still one capturing or aiming to bring into prominence an ordinary and familiar experience. It too will be used as the basis of a theoretical construction only at some peril. For its 'home' is in a very specific set of applications; it was *designed* to bring into prominence a non-ubiquitous phenomenon.

One should, I suggest, see quite a lot of what has preceded this Inter-Section and of what follows it as filling out, testing, greatly expanding and using some such elicitation as that gestured at here. But to remind you of what I tried to stress in the Preface, above: *it is ultimately* you *who has to elicitate,* to fill out and test. This book is about Wittgenstein and me *and you* among and between the sciences (and the 'sciences').

1 Of course, a wrinkle here is that there is a difficulty with the very idea of 'evolutionary natural kinds' because species don't have hard edges, especially not hard temporal edges.

2 For detailed argumentation to this effect, challenging the Roschian 'paradigm' on Wittgensteinian grounds, see Read (2005).

The reader needs to find themselves in the investigations undertaken here; anywhere I try to force you to go I *ipso facto* have no authority in …

Onwards, then, to Part 2, for the elicitation and clarification etc. that may occur there, among the human sciences.

PART 2
Wittgenstein, Winch and 'Human Science'

Putting a Wittgensteinian Methodology to Work among Psychology, Psychiatry, Economics, etc.

> The left hemisphere's lack of concern for context leads to two important consequences, each of which makes its version of reality more dangerous and simultaneously more difficult to resist. The appropriateness or otherwise of applying scientism to one field of human experience rather than another – Aristotle's perception – is disregarded, since to understand that would require a sense of context, and of what is reasonable, both of which, from the left hemisphere's point of view, are unnecessary intrusions by the right hemisphere on its absolute, non-contingent nature, the source of its absolute power. At the same time, science preached that it was exempt from … historicisation of contextualisation …, a way of enabling science to criticise all other accounts of the world and of human experience while rendering itself immune to criticism.
>
> Iain McGilchrist, *The Master and his Emissary*, page 385.

2.1 The Ghost of Winch's Ghost

> Philosophy may in no way interfere with the actual use of language; it can in the end only describe it.
>
> Wittgenstein, *PI* 124.

> Social scientists do not have a theory … for how the people they study determines what counts as an action, because the determination derives from situationally defined (context-dependent) skills, which the objects of study are proficient and experts in exercising, and because theory – by definition – presupposes context-independence.
>
> Bent Flyvberg, Making social science matter, p.42.

This Section, 2.1, serves among other things as a kind of *introduction* to Part 2. It (also) takes up very roughly where Hutchinson, Read and Sharrock's (2008) *There is no such thing as a social science* left off. It provides an overview of / an introduction to Winch as a truly Wittgensteinian thinker among the social and human sciences, as a preliminary to the more focussed 'case studies' in the human sciences which occupy the remainder of the book.

We have seen in Part 1 how Kuhn's 'historicisation' of (natural) science can and should be read as modest, not at all revisionistic in its consequences for science (It leaves science as it is, as it were …), not at all debasing of science. But this leaves open the question of whether what Kuhn showed is true of other sciences/'sciences'. This Section argues that Peter Winch can and should be read as offering a genuinely *therapeutic* Wittgensteinian take on the methodology of what I would rather call the social studies, and offers an indication of some of the reasons why we should be at least suspicious of the claim to scientificity of these disciplines. And furthermore of why such claims frequently obscure the *independent worth* and different functional nature of the social studies. 'Science' is not the measure of all things.

Winch pursues the project of illuminating the centrality of certain ideas of Wittgenstein's for the foundations and methodology of 'social science'. The ideas of rule-following and of a form of life are perhaps the central ones he urges upon us: by means of these notions we can indicate a framework for the characterisation of human action as action that constitutes meaning, or (rather) for avoiding its mischaracterisation. It is because this is possible and necessary that Winch talks about the ultimate convergence of 'social science' and philosophy. Insofar as philosophy seeks to understand experience (or: to seek efficacious ways of avoiding *mis*understanding it), it is concerned with developing ideas that, by

illustrating structures of meaning and life, provide a foundation for what is called social science.[1]

Central to Winch's aims in his Wittgensteinian take on 'social science' was the importance of getting the right *description* of whatever one is seeking to understand or explain (Cf. *Philosophical Investigations* 124, as quoted above). As I shall argue in 2.2 and 2.3 (and in the 'Concluding Summary' to the present work), there is a kind of understanding that philosophy seeks that science doesn't deliver. (And one reason for this is that 'We feel that even if *all possible* scientific questions be answered, the problems of life have still not been touched at all' (*Tractatus* 6.52. Cf. also 4.111). The problems that are central to politics, to psychiatry, and so on, are *problems of life.)*

This is Winch's promise. But: *The Idea of a Social Science and its Relation to Philosophy,*[2] the greatest or at least most famous of Wittgensteinian takes on the social sciences, is a polemical work. Peter Winch wrote it when he was less than 30 years old. It is the text of an angry young man – of someone angry at the potentially dangerous perspective(s) on society dominant in his time, and still, I would suggest, dominant today, albeit mostly in different guises.

A sober assessment of Winch's challenge and achievement in the philosophy of the social sciences, after his death and at a distance of over 50 years from his original text, must take seriously into account both what it was that he wished polemically to question and to challenge, and what it was (if anything) that he himself actually wanted to say. On the latter front especially, a serious effort to understand Winch will take into account how he wished to be heard and read, in the light of the 'evidence' which the totality of his later work provides, and in particular, that provided by the new 1990 Preface to his epochal book. I have offered such an assessment, in concert with Phil Hutchinson and Wes Sharrock, in our (2008). The present Section aims to take that debate on a little further, and, in reflecting upon it, offers my own perspective only, unalloyed. I shall not marshall extensive textual evidence here from Winch, having already done this reasonably extensively elsewhere. I aim rather, primarily, to offer a kind of '*ubersicht'* of how one can and (I hold) ought to take Winch, in this domain, and of how Wittgenstein can help one to see this.

I think that, if one can, one *must* re-read *ISS* in the light of the Preface which Winch added in 1990. *If* one can, because an author suggesting how their own work should be understood cannot be guaranteed to get it right, especially when writing at a much later time. Authorship is not a trump card; but it is, I think, a card to which attention should always be closely paid. If one wants to understand a text, rather than reductively misread it or fail to read it altogether, then one will want *if possible* to understand it as its author understands it. Where the text is simply confused, or where the author is evidently revising the text rather than exegetically

---

1 Thanks to Davide Rizza for great thoughts on these points.

2 Henceforth *ISS.*

commenting on it, then one will of course have to give up such a strategy.[3] But I think that the strategy can take us an awful long way, in the present context, and that it is regrettable how rarely it has been pursued by Winch's would-be 'critics'.

Few indeed of the many critiques of Winch take his post-1964[4] work at all seriously. I want to pay some attention here to what remains one of the best 'collective' efforts to understand Winch in the last decade or two. I am referring to the important special issue of *History of the Human Sciences* devoted to his work, published a decade ago now.[5] That special issue, devoted to Winch's work, is no exception to the 'rule' given above – it does not take seriously the later Winch. The special issue features ten papers, by some major names in the field. It features several authors who would characterise themselves as 'Wittgensteinians' of one stripe or another. It contains several papers which purport to involve a serious effort at understanding Winch. Some of the papers in it are very interesting, and there is some good philosophical and intellectual work in some of them. But my own opinion is that, regrettably, at most only two of them actually succeed in taking up a Wittgensteinian stance upon the human and social sciences,[6] and that at most, these same two succeed in understanding Winch in a manner that Winch himself would find minimally acceptable. In sum, hardly any of the contributors to *HHS* takes up the kind of anti-theoreticist stance that I believe Winch took, following Wittgenstein. *They do not seek to follow Winch, in seeking to understand Winch*.

Most of the papers in the *HHS* special issue, of course, mean to criticise Winch in one or another fashion. But I believe that *they mostly fail even to do that,* because they do not evince a sufficient level of understanding of the object of their criticisms, which are largely directed not against Winch's ghost, but against a mere *ghost of Winch's ghost*. They are *not*, in my view, in actuality directed against his work. Their alleged target is simply missed.

'But what,' asks an interlocutor, 'do you mean by theory? You say that we argue only against a ghostly unreal Winch; but *what would it be* to take up a

---

3 As will become evident, I believe that almost all of the 1990 Preface can be read as explicating the 1958 text, rather than revising it. Winch frequently refers to the unfortunate way he put things in the first edition of *ISS*; he rarely suggests that it needs to be substantively revised. On the single occasion where he does, I think, suggest substantive revision (see below), I personally would be inclined to accept the revision (while conceding, of course, that it *is* a revision).

4 The publication date of 'Understanding a Primitive Society.' Most critics do give this paper some attention – even though few of them seem to understand it, and fewer still seem to use Winch's later work to help themselves do so. UPS is often taken to manifest a substantive relativism. One way of seeing that it need not do so is to take seriously the 'modest' interpretation of incommensurability argued for in 1.1 and 1.3, above. One way of seeing that it surely *does* not do so is to read and reflect upon Winch's later essay, 'Incommensurability'.

5 *History of the Human Sciences,* 13(1), February 2000. Henceforth *HHS*.

6 The two exceptions to the general rule, in my opinion, are Lynch and (perhaps) Lassman.

non-theoretical stance *vis-à-vis* the social sciences? Isn't the avoidance of theory itself merely an out-dated empiricist fantasy? And isn't it only Winch's empiricist fantasy of natural science[7] that stands against what he calls 'social study' so as to provide a supposedly clear counter-class?'

For reasons that will be familiar by now to readers of this book, I am deeply disinclined to answer the demand for a definition of 'theory' directly. I do want to allow for the possibility – which this kind of questioner typically wants dogmatically to pre-emptively shut off – that there can be such a thing as not having a theory. It's not at all true however that I think that theory outside the natural science is always bad. It is true that I don't offer in Part 2 of this book many examples of harmless or even good theories or theoretical concepts. In a longer work, I could do so: on the basis that not all theories have to be in the form of 'scientific' or 'philosophical' (e.g. metaphysical) theory, and that some 'theory', in the sense of well-motivated and -positioned critical reflection and questioning (one might or might not want to call that 'theory'), can be OK. For: what makes a speech act good or bad theory is not the form of words in which it is couched (though those that are mostly in practice self-referential, such as most 'social theory', can't be much use outside of their own game) but what is done with it, how it is *used*, what it is used to do; i.e. I am suggesting here something along the lines of: 'Don't look for the meaning; Look for the use'; good philosophy is about activity not product.

It is very important to understanding the present work to be clear that *there is nothing wrong* with the term 'theory' *in itself*. To say that there were, or that it should never be used in regard to human phenomena, would be a dogmatic and futile attempt at word-policing. There is no harm in using the word 'theory' even in relation to one's analysis or what-have-you of human affairs – *provided* one is clear about how one is using the word, and alert to the ways it may incline one, as part of an extant literal or metaphorical 'system' of thought or of associations, to move in certain directions rather than others, perhaps problematically. This book is, I hope, helping to develop in the reader (and in the author) a sense of judgment about the difference between different uses of the word 'theory' (and of the words 'science', 'explain', and so on.). The hope is that one will no longer be *taken in* by *resemblances* between what one is doing (as a student of society, say) and natural science.

So: it is *true* that Winch's 1958 presentation was marred by an empiricist rendering of natural science.[8] But this does not vitiate his questioning of the *scientism* of 'social science'. The presumption, for instance, that the task in hand ought to be the production of something on balance (i.e., thinking of natural science as in some sense our model) worth calling a *theory* of the phenomena. The possibility that something other than a theory might be what we need is rarely considered; it is hard for most commentators even to bring it into view clearly.[9]

---

7 For detail, see for instance Bhaskar (1979).

8 Winch admits this on pp.xi–xii of the 1990 Preface.

9 The main reasons for this are given in McGilchrist's wonderful (2009). See also Lakoff and Johnson's important work; and see the Interview that rounds out the present work, below.

It is a possibility central to the possibility that Wittgenstein and Winch may have something novel and radical to offer one, as one tries to find one's way about the so-called human sciences.

The question as to what it would be to take up a non-theoretical stance in reflection (e.g.) upon the subject-matter of 'the social sciences' (i.e. upon social phenomena) is a good question, a question integral to Part 2 of the present work. But those asking it often seem to think it a rhetorical question, a question that answers itself in the following way: it is impossible for there to be any such stance. The claim that something worth calling a non-theoretical stance in philosophy is impossible is a claim that I question. First off, *what* is it that is being said to be impossible here? Until *that* question is answered, my interlocutor's own claim is moot – *what* is being claimed?[10] It often seems that such a 'claim' is a product of what Richard Rorty[11] has called 'post-modern knowingness'. According to such a 'knowing' stance, 'everyone' knows in advance that anyone claiming to be able to do without a theory is merely relying tacitly and dishonestly on a (hidden) theory. Often, such 'knowingness' is in the service not of a genuine anti-positivism, but of an elitist theoreticism.[12]

The point of much of the best work on and after Wittgenstein in the last generation has been to dispute such assumptions. More specifically, there is a 'school' of Wittgensteinians (loosely associated with the heritage of Winch, more closely with that of the later Baker, Cavell and Diamond),[13] my school, who believe that Wittgenstein *can* be understood, after his own word, as no purveyor of theories, nor of any philosophical assertions or theses barring those which elicit no disagreement.[14] The scholars in this 'school' dispute that the 'return to the everyday' need be politically conservative;[15] they dispute that it need be naive philosophically;[16] and they ask (again) whether the very various philosophical

---

10 For further explication and exemplification of the 'logical method' I am employing here, see Stone's (2000).

11 Unfortunately, Rorty sometimes fails into a trap somewhat analogous to that which he has delineated in the work of Jameson and others. I.e. He writes as if it must be obvious to anyone with a modicum of intelligence that such and such a position (e.g. advocacy of 'liberal democracy') is now uncontestable, or that such and such a theory (e.g. Darwinism) has inevitable philosophical or ethical consequences.

12 For a brilliant critique of all such 'knowingness', see the opening of Chapter 3 of McGilchrist's (2009); and cf. the 'Concluding Summary' of the present work, below.

13 The 'Winchians' tend to be sceptical of the 'Diamondians' claim that Wittgenstein's *early* work can be resolutely interpreted in this fashion (see for instance Reid's 1998). There is more agreement between them on the later work, and thus on Wittgenstein's enduring legacy.

14 This 'school's' work was first brought together in Crary and Read (2000).

15 See for instance the close of Crary's 2000. Cf. also Nigel Pleasants's recent work, and Gavin Kitching's.

16 See especially Cavell (1990) and Stone (2000).

and social theoretic opponents of the everyday have in fact succeeded in saying anything at all, when they say that such a return is 'impossible'.

Martin Stone's (2000) essay 'Wittgenstein and Deconstruction' exemplifies the last of these points particularly well. Stone notes the deep similarities of influential currents in recent English-speaking philosophy – for example Saul Kripke's work on 'rule-scepticism' – with the influential current in Continental philosophy explicitly known as 'Deconstruction'. Deconstructive writing typically suggests that there is no escape from the 'trap' of language, that one should be anti-metaphysical but yet that there is no escape from metaphysics, and that everything is political – such is the spirit of 'Post-Modern knowingness'. Stone then tries to understand what Derrida can be trying to say in his 'Deconstructive' writings.

Stone notes carefully Derrida's own painstaking efforts to avoid the cliches of Post-Modern knowingness. But he reluctantly concludes, even so, that he cannot find a successful way of understanding central features of what Derrida is trying to say, and in particular that Derrida has not shown that there does not exist a way of dealing with philosophical perplexities which refrains from giving a philosophical account or theory or making philosophical claims (Ibid: 106). In short, Stone argues that the central aim of Wittgenstein's discussion of rules is to suggest that if a certain metaphysical idea of meaning, and the *deconstruction* of that idea, seem to exhaust the philosophical options, that is owing to our failure to see another possibility – namely, a return to the ordinary or everyday:

> What we do [*i.e. in contrast to other philosophers* – Stone's note] is to bring words back from their metaphysical to their everyday use. (*PI* 116)

> Wittgenstein identifies philosophy's metaphysical voice as his critical target. But this alone would hardly distinguish him from any number of other philosophers within the huge Kantian wake of philosophy's self-criticism. So, it would be a mistake to infer from such a common metaphysical target, that the contrast Wittgenstein wishes to draw (between himself and others) should not embrace – or even refer most especially to – those philosophers who [like Derrida] set their face *against* metaphysics. 'We bring words back,' Wittgenstein is to be read as saying – 'in contrast to the way other philosophers criticize metaphysics; in *their* form of criticism, words remain metaphysically astray'. (Stone, 2000: 84)

It is (we 'New Wittgensteinians' and 'therapeutic Wittgensteinians' believe) possible to interpret Wittgenstein as austerely non-theoretical, and as resolutely therapeutic in his philosophical aims and methods. Such an interpretation takes seriously moments in Wittgenstein such as the following:

> We may not advance any kind of theory. There must not be anything hypothetical in our considerations. We must do away with all explanation, and description alone must take its place. (*PI* 109)[17]

These moments are clearly not, I believe, to be regarded as naive retreats into 'common-sense philosophy' or empiricism, but as reminders of a possibility undreamt of in Analytic or empiricist or Post-Modernist philosophies. I also believe that the main currents in Winch's thought are consonant with this interpretation of Wittgenstein. Thus, this 'detour' into Wittgenstein's thought was necessary, and was in fact no detour at all.[18] After Winch, after Wittgenstein, I am against anything probably worth calling 'theory' in the social studies in most cases for the following reasons:

1. 'Theory' in the social studies tends to occlude the very phenomena it deals with, substituting for them simplified 'dummy' versions (and, in supporting this claim, one might cite the bulk of the work of ethnomethodologists, over the past 50 years);
2. Part of this process is an abstraction from participants' own understandings which, when such abstraction is deep or complete,[19] fails to deal with those participants as humans or social actors *at all*;
3. Explanatory theories distract us from the real task of assembling therapeutically-motivated descriptions of certain social phenomena which *do* puzzle us;
4. There is a grave danger that a pre-occupation with theory will lead one to presume that actual human actors' primary relation to the world is itself theoretical: a disaster made visible for example in the 'Theory of Mind' theory of Autism, which attributes to ordinary human actors the very kind of theory that autists need to resort to precisely *because* they lack a natural, atheoretical engagement with the world.[20]

---

17 Wittgenstein goes on: 'And this description gets its light, that is to say its purpose, from the philosophical problems. These are, of course, not empirical problems; they are solved, rather, by looking into the workings of our language, and that in such a way as to make us recognize those workings: in despite of an urge to misunderstand them. The problems are solved, not by giving new information, but by arranging what we have always known. Philosophy is a battle against the bewitchment of our intelligence by means of language.' For more on Wittgenstein's understanding of theory, and his antipathy to theory in philosophy see especially his (1980: 28, 32, and 44). For more on the sense in which Winch does (and does not) oppose the giving of explanatory theories, see Pleasants (2000: 82).

18 See also the opening chapters of Lyas (1999).

19 See Harold Garfinkel, especially his early and his recent criticisms of what the ideology of social science systematically tends to conceal.

20 For detail, see e.g. Sharrock and Coulter (2004).

The third of the above was especially Winch's concern. But this concern should not be confused with dogma. It is perhaps helpful to emphasise the proviso that it is 'in *most* cases' that theory-talk is to be avoided in (the philosophy of) social study. The only thing I dogmatically insist upon is that my position hereabouts is not dogmatic. Once more: I of course do *not* say that there cannot be uses of the word 'theory' in relation to social study which can be intelligible and helpful. I say rather that the word 'theory' has a bad history in this context, and is probably usually best avoided. I do *not* say that whether or not one calls what one is doing the production of a theory or not is the key issue. On the contrary, it is at most a marker of more fundamental issues concerning the actual character of one's intellectual work. I say that most intellectual work which pretends to produce or to realise something like what has been traditionally called a social theory (whether an 'old' social theory, e.g. Scientific Socialist class-analysis, or a 'critical' social theory, e.g. Giddens' or Bhaskar's) is highly likely to be vulnerable to the criticisms that I have mentioned. Winch was the first to make these criticisms in detail.

Pleasants's (1999) *Wittgenstein and the Idea of a Critical Social Theory* buttresses the last of my claims here. The allusion to Winch in the title is to the point, and Pleasants argues that would-be 'post-positivist' leading lights of contemporary social theory, especially Bhaskar, Giddens, and Habermas, are actually still falling into the same traps of scientism and misbegotten epistemology as their positivist predecessors.[21] Moreover, Pleasants lays out in effective detail what I have had space here only to sketch the barest outlines of: namely, Wittgenstein's antipathy to 'theory' in inappropriate places, and how this can be spelt out in relation to 'social science'.[22] Unfortunately, I would suggest, even Pleasants fails (in this book of his) to be sufficiently sympathetic to Winch's own perspective on these matters, in characterising Winch[23] as a (more or less unwitting) fomentor of and participant in 'critical social theory'.

Winch, I am urging, can and should be read as rejecting a place for 'theory' in accounts of social reality, if by 'theory' we mean anything much like the various things that the likes of (for instance) Durkheim, Friedman, or Bhaskar, appear to mean by it. 'But,' an interlocutor may interrupt, 'how can this be? For, whether or not you are right about Wittgenstein, clearly *Winch* at least does have a theory, centred around the notion of a *rule*.'

---

21 Moreover, that their politicised 'knowingness' is more or less necessarily coupled with actual political impotence, and is thus a counter-productive use of time and energy, for anyone serious about changing their society. Pleasants believes that on this score, Winch – often derided as a 'conservative' Swansea Wittgensteinian – has a more genuine 'critical' edge to his philosophising than these self-consciously leftist contemporary academics.

22 See the first two chapters of his (1999). The reader is strongly advised to consult these, if dissatisfied with my own far-too-summary critique of theory in philosophy and related domains.

23 See Chapter 3 of his (1999).

Does Winch have a rule-based social theory? Many have thought so, based on their reading(s) of *ISS*. A major contemporary author who has critiqued Winch repeatedly on these grounds is Ted Schatzki (1893, 1999, 2000). A striking but not unusual fact about Schatzki's interpretation of Winch is that it pays no attention whatsoever to the 1990 Preface. I shall not labour my criticisms of Schatzki here (having already taken him on in detail in Chapter 1 of *There is no such thing as a social science*), but shall just get straight to the point:

Winch, unlike Schatzki, is (on my 'charitable' reading of him) undogmatic. Schatzki insists that the term 'rule' must be eschewed. But, as I acknowledged above, in connection with the term 'theory', no term is through and through flawed or misleading. To think otherwise is to fall back into the grip of an Augustinian misapprehension of the functioning of language. It is words *in use* which mean something or other, not single words in isolation.[24] Strings of words in isolation from any significant context of interest outside of the mere playing about with the words in theory-games: this, once more, is the problem with so much social theory. (See in this connection the quotation from Bent Flyvberg that opens this section.)

So, the way in which Winch uses the term 'rule' may be at times ill-advised or risky – as he himself admitted in 1990 – but it is a similar mistake to that of verificationism/positivism to claim that we can rule out *in advance*[25] the possibility of Winch's rule-talk being helpful in our philosophical inquiries.

But Winch never insisted on the term nor, for that matter, or any other term. The maxim which he seems to be following is more like 'I'll settle for whatever way works to convey a certain contrast (e.g., between the social and the natural) which is therapeutically/practically useful' rather than looking for one particular term – it could for instance turn out to be 'rule', or 'norm', or 'tradition'- which is theoretically 'correct' and may solve all our problems.

The latter alternative, the search for the magic (quasi-scientific) word, is roughly Schatzki's approach, and this is perhaps not surprising, when placed in the broader context of the fact that Schatzki has his own ('practice-theoretic') account of social reality. He wants to follow Wittgenstein – but ends up doing so, it seems, much less than Winch.

I have picked on Schatzki *because* there are moments in his work when he, like some others in the *HHS* group, such as Pleasants, is tantalisingly close to Wittgenstein's vision.[26] I stress that Schatzki and Pleasants are, as it were, notionally right: if Winch did hold the kind of thesis about the absolute centrality to social life of rules attributed to him by them, then he would indeed be guilty

24 A full discussion of this point would require a detailed rendition of the New Wittgensteinian take on the importance of Frege's 'Context Principle' in Wittgenstein's thought. A brief primer on this can be found in Crary and Read (2000).

25 As, for instance, Carnap famously rules out that Heidegger's 'The nothing itself nothings' can be possibly mean anything.

26 Cf. especially Schatzki (1983: 137, 2000: 104, 1991).

of a 'metaphysical error'. But I have argued that, especially when we understand Winch as he asks us to understand him, we find him making no such error, or at worst using formulations at times that are uncomfortably similar to what someone would say who was making such an error.

Illustrative of the difference in temperament between Winch and some of his critics is his distinctive reaction to the challenge presented by Kripke's 'rule-scepticism'. Many 'Wittgensteinians' have sought to refute Kripke, to answer him in one way or another. Most notable here perhaps are Baker and Hacker (1984), who rely on the concept of 'internal relations' to bind together rule and application. They do come awfully close to having a rule-centred (though individualist) 'social' philosophic theory.[27] A minority of 'Wittgensteinians' have instead embraced Kripke's rule-scepticism as helping us to found a true social theory. Most notable here is David Bloor (1983), in his *Wittgenstein: A Social Theory of Knowledge*.[28] But others have refused altogether to take part in the debate on these terms. They have not tried to refute Kripke, but rather, to understand what is important and tenable in the human and linguistic roots of the challenge he tries to issue, and to deflate that purported challenge by means of issuing an invitation to the philosopher caught up in a Kripkean problematic to revisit the everyday employments of the terms (e.g. 'rule' or 'interpretation') which are at the heart of the matter. It is striking that Winch (1987) proceeds more or less explicitly in this manner, as (shortly after him) did Cavell (1990) and Diamond (1989).[29]

Schatzki and Pleasants do not go as far as Bloor, in that they appear to disagree with his Kripkean belief in 'undermining' or criticising the concept of 'rule'. For the likes of Bloor, 'rules' are a flawed element of a would-be social theory. For the likes of Baker and Hacker, 'rules' offer the tools to successfully rebut Kripkean skepticism. It is only Winch (*et al)* who bypasses this debate, out of the conviction that 'rule' is at base just another word of the English language, no more exceptional than 'game' or 'chair' or 'microscope'.[30] Winch's discussions of 'rules' and 'internal relations' in *ISS* and elsewhere are then best read quite differently from Baker and Hacker's reading in their critique of Kripke.

Winch is building on Wittgenstein's *Tractatus* understanding of 'internal relation', which arguably persists into Wittgenstein's later work [31] as something

---

27 My own arguments detailing why any follower of Diamond will question and reject their position are to be found Guetti and Read (1996) and Read (1997).

28 In conversation, Bloor has evinced surprise that Winch too has/had not embraced Kripke. This surprise stems, I believe, from Bloor's total lack of comprehension of the idea of a non-theoreticist philosophy, of a philosophy wherein words are truly brought back from metaphysical holiday to their everyday employments. Cf. also p.378 of Bloor's (1996).

29 Diamond's paper is in a collection co-edited by Winch. Other 'New Wittgensteinian' critiques of Kripke include Stone (2000), and Read (1995, 2000a).

30 Cf. *PI* para.s 116–121.

31 See again Guetti and Read (1996) and Read (1997). Winch of course emphasised the continuities in Wittgenstein's philosophy much more than is usually appreciated – he

strictly unsayable, a notion of transitional value at best. *Contra* Baker-and-Hacker, the term 'internal relation' cannot in the final analysis be used faithfully to Wittgenstein in a way which provides a generalistic account of 'metaphysical glue' between rule and application – this idea *does* deserve the kind of criticism which Schatzki (inappropriately) levels against *Winch* on rules.[32] 'Internal relations', for Winch as for Wittgenstein, are not *genuinely* relations. Only 'external' relations are actually relations, between separate things. And there have to be separate things, if there are to be relations (between things). It follows that when Winch speaks of different 'parts' of social life, and similarly of social relations as being 'internal relations', what he is really saying is most usefully put as follows: that *they are not relations at all,* and that characterizing them as relations can, riskily, lead to society being thought of in nonsensical atomistic ways. 'Internal relations' are simply alleged parts of *wholes/unities*. (Whereas 'applications' of rules, as we might put it, sometimes *develop* rules.)

When we read Winch with a sensitivity to the non-assertoric, post-metaphysical nature of his philosophy (at his best – see Chapter 1 of *ISS* for detail on this), and when we attend to his attendance to the continuities in Wittgenstein's philosophy (rather than, as is usual, reading in him at best only an alleged version of Wittgenstein's alleged 'later philosophy'), then profitable ways of understanding a multitude of his remarks in *ISS* open up, and unprofitable ways are shut down. For example, when we pick up the notion of 'internal relation' for a while, we see that, though it cannot be 'ultimately' satisfactory (because it is a *transitional* therapeutic notion), and though it can risk leading us to say things which sound awfully like (nonsensical) metaphysical 'theses' about the social world, it at least usefully closes down the unprofitable avenue of thinking of different practices as being (metaphysically) hermetically sealed off from one another (Cf. the closing pages of Winch's 1990 Preface to *ISS*); and furthermore it suggests instead an alternative 'picture' which may help to point up the *absurdity* (not falsity) of the atomism and ontological individualism or 'atomism' which have dominated much social theory.

In sum, careful attention to Winch's text and to its Wittgensteinian backdrop starts to show quite clearly how he resists being boxed into any 'position' in the conventional spectrum of social theories/philosophies – whether 'individualistic' (e.g. Baker and Hacker) *or* conventionally 'holistic' (e.g. Bloor).

'You might have a point', my interlocutor may say, 'in arguing that Winch does not hold the rule-ubiquity theses attributed to him by Pleasants and Schatzki.[33]

---

arguably 'pioneered' the 'New Wittgenstein' interpretation of the continuity of the *Tractatus* with the *Investigations*, in his (1969). See on this also Denis McManus's (2006) masterful reading of 'internal relations'.

32 See Guetti and Read (1996) for detail as to where Baker and Hacker go wrong.

33 Compare also Stephen Turner's (2010:404–5) remarks about Winch's alleged dependence on 'the act of 'grasping' … as a fundamental mental act.' This mentalist or at best idealist interpretation of Winch is a charming counterpart to the quasi-behaviourist

Perhaps these 'theses' are at most truisms for Winch, not controversial and fruitful social-theoretic claims. But then, in his crucial remarks in Section VI of Chapter 3 of *ISS* on 'Understanding social institutions' and on what makes possible judgements of identity (Winch says that these judgements require a certain quasi-empathic identification with the 'subjects' one is aiming to understand),[34] what do 'different' and 'same' *mean*, for Winch? How, for example, is the notion of 'same community' or 'different community' operationalised in the first place?'

To answer the second question first: thankfully, it is not. To answer the first question: These terms mean nothing at all for Winch, considered as parts of a *theory*. Of course, they can mean everything in the rich and sometimes conflicting lives of real people from (or not from) real particular communities. Why think that we need philosophers and/or social theorists to individuate communities or otherwise, when people are so busy already doing it for themselves? *What could such a theoretical individuation be, other than an attempted imposition of a simplified dummy reality on our complex world?*

'But this is ridiculous! Every time you are pressed to clarify what you mean by a term, or what one of Winch's notions amounts to, you say that it doesn't mean anything!'

Right! It doesn't mean anything – as part of (anything worth calling) a *theory*.[35] Roughly: only insofar as a theory were being put forward would one's claims be truth-evaluable, and would what one says turn out to be true or false. But there is no good reason to think that this is what Winch is doing. Our work in philosophy is/should be thoroughgoingly 'therapeutic'. *It returns us to ourselves.* That is 'all'.

'But isn't it *true* that the Azande *are* (were) a relatively homogenous culture relative to ours, and that their culture *is* (was) relatively isolated from others, from other communities?'

Now we have rapidly moved from an exclusively philosophic terrain to a question which is at least partly a simple matter of fact, or at least of empirical social description. The answer to this question may be 'Yes' – at least, for all I know. The real question is: What philosophic or social-theoretic weight can/could be put on this 'Yes'? What interesting consequences follow from this historical/

---

interpretation of him that has sometimes attracted others. When one get such 'balancing out' interpretations, it is (as shown in Chapter 1 of *ISS*) usually a reliable sign that there is a complete failure to understand the radicalism of the text in question – its not fitting into prior theoreticist categories. Compare here the closing passages of the Introduction to Kuhn's *SSR,* and Sharrock and Read (2002: 27). Winch's thinking is, we might say, in a new dimension, which the likes of Schatzki and Turner seem incapable of perceiving.

34 Cf. also the epigraph to this Section from Flyvberg's impressive book. (The only big point on which I disagree with Flyvberg is his continued obsession with the idea of social *science*, after he so effectively deconstructs everything it means in the early chapters of his book.)

35 For a beautiful, relevant discussion of the etymology of 'theory', cf. p. 266 of McGilchrist's (2009).

commonsensical point for philosophy of mind or language, or for the validity or otherwise of (e.g.) Giddens's picture of the human agent? Surely none at all! *The problem of other communities' is simply a disguised contemporary version of the old problem of other minds*.[36] It can be just as phenomenologically real, but to someone in its grip, either notionally or really,[37] theory is an irrelevance. 'Therapy' is called for instead.

And this goes even for any terms which I myself might appear to rely on. Turner (2010: 414, 421–4) thinks for instance that my take on Winch and Wittgenstein is dependent on a theory of practices. (He thinks that *that* is my 'magic word'.) He thinks that 'practices' are posited as the ultimate facts/the defining theoretical entity, in my book *There is no such thing as a social science*. But this is a misunderstanding. When my colleagues and I mention or use the word 'practices', we mean only *to redirect attention back to what people actually do* in our society and in others. It is not a theoretical appeal, not a conversation-stopper. It is a way of turning attention back from *any* theory at all. It is a way of our not making any questionable appeal to (our) expertise, or to (quasi-scientific) authority.

Talk of practices is simply a way of returning to what has actual authority in the social world: understandings embedded in accounts people give as they do what they do. This is (among other things) to draw attention to what we do and thus to the body, to embodiedness,[38] to 'phenomenology' … yes, because these are part of the doing of anything: but attention is drawn to these only in the context [39] of what Wittgenstein calls agreement in judgement and in form of life. That is practice. *All this talk returns us 'only' to us.* It is non-theoretical. *Pace* Turner, there is no 'theory of practices' in my work or in Winch's.

So Winch, like Kuhn, has been almost endlessly and more or less tragically misunderstood. Admittedly, the way he wrote, and some of his polemical and thought-provocative formulations, like Kuhn's (see the early Sections of Part 1, above), gave away hostages to fortune. Thereafter the hostages were murdered over and over again, and therefore their ghosts still, regrettably, walk among us. If Winch's ghost is to have peace (in the subtle and complex sense in which Wittgenstein wished to give philosophy peace, in *PI* 133), then the ghost of his ghost, the phantasm of those hostages, must be laid to rest. This will require

---

36 This point is expanded on beautifully by Richard Hamilton in his (unpublished).

37 I have in mind Louis Sass's portrait of some sufferers from schizophrenia as would-be real-life solipsists – see his 1994, and 2.2 below.

38 I am thinking here of Lakoff and Johnson's description of their 'embodied realism'.

39 And 'context' too is not a word that I/we theorise. See Iain McGilchrist (2009) on 'context' as properly a right-brain, not a left-brain, concept (Cf. the epigraph to this Part). The failure to understand this consumes even someone like Derrida, for reasons similar to those given in the discussion of Derrida and Stone, above: Derrida, in his famous attempt to criticise Austin, can't help falling into the 'left-brain' hyper-analytic presumption that 'context' must be a theorisable notion that Austin must be trying to theorise.

philosophers and theorists of the social sciences to find a way no longer to hear Winch himself as a fantasist with incoherent mumblings of a Relativistic or Idealistic nature forever behind his lips. Winch never meant to be a social theorist or a philosopher with a metaphysical message.[40] I have tried to suggest here how he can be read otherwise than as that. But it requires an effort of will to follow my suggestions. I hope I have offered enough *materiel* to help one willing to make such an effort, in their journeyings.

Wittgenstein once suggested that philosophers confronted with our own – their own – ordinary lives and words are like primitive peoples confronted with the artifacts of modern civilisations. Philosophers (of the social sciences, for example) typically lose their concepts[41] and lose their footing in their everyday practices, when they reflect on (theorise) those practices.[42] The same applies to 'social scientists,' and so both tend to invoke crude two-dimensional substitutes for those practices, with all their vast complexity and subtlety.

Winch once sketched a way of avoiding doing this in connection with an influential example, that of Evans-Pritchard's *Azande*. How ironic it is that, so frequently, the way in which Winch has been treated in the literature involves *just* the kind of primitive misunderstanding (both of his topic and of his text) that he himself warned against.[43] The misunderstandings of Winch which have predominated among philosophers and others can be alleviated – but not by any theory, and not by any reader unwilling to do the work for themselves. (In the end, each of us has to do the real philosophical work for ourselves, using others' words, such as mine, at best as directions which one can turn to one's own case.)

Readers *willing* to do that work will find that they no longer have to wrestle with Winch's ghost in the form of only a ghastly misrepresentation of him. Rather, they will find, as I/we found in writing *There is no such thing as a social science*, that Winch is alive and well and living in his texts, if not in most of his extant 'influence'.

---

40 This is quite clear, for instance, in his 'Persuasion' (1992).

41 In Cavell's sense – for explication, see Cora Diamond's (1988).

42 And this can of course much too easily slip 'by extension' into looking down not only on ordinary people as primitive but on 'primitive' peoples as thus doubly primitive … as if the 'disenchantment' of modernity is obviously a good thing. As if it could make sense to think of a fully human life lived without any rituals at all. As if the category of 'the sacred' were self-evidently a primitive or undesirable one – as opposed to perhaps *essential* for a sane society. As if the sense of 'attunement' frequently manifest in 'primitive' societies for example is self-evidently a primitive and undesirable thing – as opposed to perhaps highly-desirable, *essential* even for human survival, maybe …

43 For some chapter and verse in the case of the 'critical social theorists', see Pleasants (2000: 83)

2.2 The Hard Case of (Severe Cases of) Schizophrenia[1]

> Man's greatest happiness is love. Suppose you say of the schizophrenic: he does not love, he cannot love, he refuses to love – what is the difference?!
>
> Ludwig Wittgenstein, *Culture and Value*, p.77

This Section, and 2.3 following, focus primarily on a discipline, psychiatry, that frequently trumpets its own allegedly scientific status, but that must (I argue) come genuinely to be open to and to understand (to the extent that such is possible) the phenomenology of the phenomena that are its subject-matter: the lived reality and (sometimes) self-perpetuating course of psychopathologies. Self-perpetuating, in that the *phenomenology* thereof is a crucial part of why they are difficult to escape or to emerge from. My argument here (and in 2.3) is that Wittgensteinian thinking has something substantive to offer in this regard: Wittgenstein's therapeutic diagnosis of solipsism and of other philosophical maladies, as Louis Sass suggests, can offer one vital clues to the phenomenology and thus to the aetiology and course of some psychopathologies. The terrible danger of the quest to make psychiatry 'scientific', whether via biological reductionism or via cognitive scientific modelling of 'abnormal cognition', is that it obscures this kind of possibility, and obscures the relevance then of humanistic understanding to the essence of the subject. (Thus the force of the question: Ought we to figure psychiatry as a science?)

***

In Part 1 of this work, we spent much of our time clarifying Kuhn's conception of 'incommensurability'. In 1.2, we inhabited a broadly 'interpretive' version of 'incommensurability': we supposed that there was a 'what', perhaps 'closed' to us, that was of the essence of the beliefs of defunct scientists. We saw that (as with Sapir-Whorf) this issued in a vicious relativism. So, as suggested in 1.1 and 1.3, we instead opted for a more 'modest', 'descriptive' version of incommensurability: one that urges that scientists who are 'distant' from us are genuinely hard to understand but perfectly comprehensible once we have made enough of a hermeneutic shift, and placed them at a greater distance from

1 Some of the material in the first half of the present Section is a much reworked version of a piece originally published as 'On approaching schizophrenia through Wittgenstein' in *Philosophical Psychology*, 14: 4 (2001), 449–475.

ourselves than we were initially inclined to. That such understanding is more akin to the understanding of (say) a philosopher with whom one is in dispute that it is to the understanding of a doctrine that one needs to decode. I am going to suggest in the present Section that, in the end, Sass's influential vision of the project of understanding schizophrenia is closer to method we used in 1.2 than in 1.3/1.1.

In the previous Section, 'The ghost of Winch's ghost', I addressed some of the persistent misunderstandings of Winch which have regrettably dominated the literature to date. I sought to present Winch as a successful thinker, employing Wittgensteinian methods among the 'human sciences' in order to gain some clarity therein. I now wish to take a hard 'case' with which to test our presentation of Winch (And with which concomitantly to test the method and the stance of 1.3 and 1.1)[2] an example drawn from the work of the philosophically-inclined clinical psychologist Louis Sass, from his bold recent 'Wittgensteinian' attempt to provide an interpretation of something very alien, namely (hard cases of) schizophrenia. I take this example '*for the sake of argument';* there is no space *here* exegetically to describe in detail or to argue conclusively over whether Sass's line on schizophrenia is basically correct or not.[3]

There is an additional reason for the consideration of this case, and similarly for the argument of 2.3 that follows. It is this: That (as Winch suggested/predicted, in his famous remark about 'misbegotten epistemology'), in order to do psychiatry and psychology fully adequately, one *has* to do some philosophy. Philosophy has a role to play *in* psychiatry and philosophy. That is in part what I seek to show in 2.2 and 2.3. Philosophy doesn't just deliver vital methodological cautions; it can play a substantive 'hermeneutic' role. (In 2.5, we consider a potential objection that could arise from the approach I take here in 2.2 and 2.3: am I placing too much weight on 'consciousness', as an object of study that 'defies' natural science, and using *that*, dangerously, as the basis for my argument in the book that it is inappropriate to model studies of the human world on the natural sciences?)

***

Louis Sass argues that, far from being a 'disease' of cognitive deficit, much of schizophrenia is a 'disease' of hyper-reflexivity, even hyper-rationality and alienation. Putting the point too strongly, but to give one an idea of the direction-of-travel here: He suggests that schizophrenia is almost a philosophical malady. His fascinating critique of traditional scientistic and psychoanalytic accounts of the nature of schizophrenia, which I invite and urge the reader to consult for herself should she wish for more detail,[4] is, arguably, highly-effective. Sass argues that we can understand schizophrenia – if we follow Wittgenstein's diagnosis of

2 To anticipate: I shall argue that, unfortunately, the method of 1.1 and 1.3 and 2.1 is *inapplicable* to hard cases of schizophrenia.

3 See Part 2 of Read (2007) for detailed presentation of and discussion on this.

4 The best place to start is perhaps with Sass (1992).

solipsism as a disease of the intellect – as a disease of *hyper-thinking* and hyper-reflexivity in which the consistent solipsist moves seamlessly from thinking of herself as the centre of everything, to thinking of herself as nothing at all, to the pragmatic absurdity of thinking of herself as requiring the existence of another mind, and so on, interminably back around this merry-go-round of philosophical 'positions'.[5] We can interpret the severe schizophrenic, e.g. Daniel P. Schreber, as strictly analogous to such a solipsist, Sass claims. And thus we can understand schizophrenia, understand why it is the way it is, and what it is like.

I want to ask the following question of what Sass represents for us. Firstly, is his account as Wittgensteinian an account as he would wish it to be? If not, could (the spirit of) Winch suggest an alternative 'account' of (how not to misunderstand) the phenomena under investigation here, which would be closer to the spirit of Wittgenstein?

Here is an example of Sass's 'hermeneutic' account of schizophrenia. He writes, of Schreber:

> Schreber's *dream* is that spirits should be drawn down toward him and perish therein. For example, he sometimes hears the spirit of his wife saying (*more precisely, he hears it 'represented as saying'*) 'Let me' – words which he knew to mean let me 'dissolve in my husband's body'. Since the spirits of Schreber's cosmos *represent* potentially rivalrous conscious centres, their perishing in him *can be interpreted as* a conceding of defeat in the competition of consciousness. (Sass: 1994: 120 Emphasis added)

It *can* indeed be interpreted that way, a way consistent with Sass's hermeneutic for schizophrenia, on a rigorously-pursued analogy with consistent solipsism, and the inevitable confusions concerning the nature of consciousness which solipsism (according to Wittgenstein) involves. Alternative and at least somewhat-plausible interpretations are available; for example that the wish of the spirit of Schreber's wife to dissolve in him is actually an expression of his wish to have her (womanly) 'voluptuousness' become an actual, 'literal' part of him.

But: why ought we to be *interpreting* here at all? Oughtn't we to start with Schreber's experience *simpliciter* in its, and his, own terms? *Within* those terms, nothing represents anything unless Schreber takes it to do so, as he *does* evidently take certain words – a little bizarrely – to symbolise certain sentences, as in the example under discussion. *In* Schreber's experience, spirits *are* drawn down

5 For detail, consult chapters 1 and 2 Sass (1994a) wherein Sass, drawing particularly on Schreber's *Memoirs* of his nervous illness, presents a compelling reading of Wittgenstein on solipsism *etc.* as less a univocal and stable 'egocentric' doctrine than a continually self-deconstructing impulse, with a logic that defeats any effort to stabilize itself at 'egocentrism'. For sympathetic exposition and critique, see Read (2001a), from which some of the next few pages are loosely adapted.

toward him, and no matter that it is hard to see how we can begin to take this remotely seriously. We have nevertheless to try to see what, if anything, it *means*.

One might argue then that Sass's turn of phrase here betrays that he is palpably offering an interpretation of Schreber's experience, rather than trying as hard as he (Sass) might to 'let that experience speak for itself'. Schreber's experience, it might be argued, gets a chance to so speak (so to speak) not only in his *Memoirs,* but also in his other, and crucially his *own* (rather than Sass's), writings.

One might again take as a partial and comparatively useful analogy here an anthropologist trying to let (the experiences of) a very different people speak for themselves. We might compare and contrast Wittgenstein's attitude, in his *Remarks on Frazer's Golden Bough*, or similarly Peter Winch's approach to the Azande's quasi 'double-bookkeeping,' in his *Understanding a Primitve Society*. Apparently, deeply-bizarre and/or contradictory beliefs can proceed perfectly happily in tandem with a practical workaday attitude toward growing crops *etc.*

Wittgenstein and Winch provide us with a salient contrast to Sass's approach. Winch, after Wittgenstein, does not aim to say, for example, what the Azande are *really* doing 'behind' the appearances – when they use their poison-oracles and so on. Not at *all,* except of course in the (vital) sense that Winch aims firstly at sketching parallels which actually might help us to understand the Azande (e.g. he poses the suggestion of thinking of their practices as like prayer rather than like science), and secondly at offering in outline a description which can would reasonably well satisfy the *Azande*, were they to be interested in *being* described or in describing *themselves*. Winch holds that wherever social study is carried out, it must aim to give a version, an account, that could in principle satisfy the criterion of being accepted by those being 'accounted for'.

A very important difference of nuance here from the case of schizophrenics now suggests itself. Without, I hope, giving offence, one can say that, often tragically, there is a serious question as to whether in some serious cases of schizophrenia there can be any question of taking seriously any affirmation which a schizophrenic might make of one's interpretation of their condition, or how they feel, and so on. Because their schizophrenia (launching them as it does, according to Sass himself, on a 'hyper-rational'/hyper-reflexive/hyper-alienated journey which issues in nothing consistent) *deprives* them of the position of being able to be taken seriously in any such affirmation or denial, unlike (surely) the Azande. This raises a question about what the *criteria* of correctness of interpretation could possibly be, in the case of anyone attempting to hermeneuticise schizophrenia, and ought to make us worry about whether an 'interpretation' could possibly be what we ought to look for.

If one is 'hovering', or flip-flopping, between alternative ways of expressing oneself, or between different ways of being understood which one is being invited either to assent to or to deny, there will normally be a resolution to such hovering available. Typically, one will settle on some form or forms of words. There is a qualitative difference between 'hovering' when at least in principle a *resolution* of the hovering is available in one of these forms, and when it *isn't.* In the latter

kind of case – as, arguably, in much severe schizophrenia – we lack a basis for rendering the humans in question genuinely intelligible.[6] This is an absolutely critical point.

Very roughly: the Azande are playing a language-game, just a radically unfamiliar one; the schizophrenic is not.

The *strangeness* to us of the Azande way of life needs emphasising just as Winch himself emphasises it, if it is to come to make any sense to us.[7] Winch is careful to point out that he would have no hesitation in calling their practices irrational if he had a standard on which they could be judged as such, but that he does not because their ways are culturally independent of ours, and in this respect, are different from witchcraft in our own history, for in the latter case, there are cultural standards according to which judgement can be made, for example Christian standards.

Witchcraft-beliefs in our society are in many cases arguably 'parasitic' on Christianity, and it is in those terms that they can be called irrational. It is not, of course, the impersonal, general scientific standards of universal rationality that are in play, but those of a specific culture. The deep mistake would presumably be to mistake our cultural tradition of science as a non-imperialistic standard for judging others rather than itself a product of our culture; the Azande are *not* unintelligible to us in that we cannot grasp their witchcraft system, but are irrational, in *Evans-Pritchards's* (questionable) conception of them, because they allegedly hold contradictory beliefs. Evans-Pritchard emphasises how much at home he became with the daily practice of the Azande's magic, and it is only when he tries to bring it under comparative review that he gets into the kinds of problems that Winch tries to disentangle, resulting in his (Winch's) showing that this way of life nevertheless can in principle be understood. The *severe schizophrenic's*, however, cannot.

Let me explain in more detail. While the form of the Azande practice can be comprehended provided one takes on board Wittgensteinian lessons about

---

6 Arguably, much more than 'consistency' and order in logic, belief and visible action are required in order for a person to be intelligible. The full panoply of human expression and action, including perception, desire and affect, is needed. Where one of these is wholly lacking, as arguably in some Autism and Schizophrenia, we think we just don't know what to say about the experience of the persons concerned – we don't have grounds for saying one thing rather than another. What we can unmisleadingly be said to understand is I think *(ordinary)* human psychology, *just as Wittgenstein says,* on p.77 of *Culture and Value*, also the epigraph to this Section: 'Man's greatest happiness is love. Suppose you say of the schizophrenic: he does not love, he cannot love, he refuses to love – what is the difference?!' One can understand – or fail to understand – a friend. But one's failure to understand Schreber is, sadly, fated to be more 'absolute'.

7 This strangeness is, Winch thinks, underplayed even by Evans-Pritchard, as he characterises the Azande as irrational – for Winch thinks that the true strangeness of the Azande is that they may be doing and saying just the very odd things that they apparently do and say and yet not be accurately and without interpretive violence fitted into boxes marked by us with the labels 'irrational' or 'mistaken'.

context-relativity and about the games (of language, of life) being collectively played, the form of the severe schizophrenic case goes 'beyond' what can strictly be comprehended, and into what can only (again, after Wittgenstein) at best be diagnosed. While there is, perhaps, something one can call a Zande 'belief'-system'[8] in action, albeit by our lights a pretty peculiar one, there is often in schizophrenics only the illusion of a system[9] – almost invariably with no like-minded community to sustain it –[10] and a 'system' furthermore within which one can find no real resting-place.[11] And while the *Azande* do not press their thinking

---

8 The scare quotes are to indicate that one courts a serious misunderstanding if one even uses the term 'belief' to refer to the Azande's 'magical' notions and practices. In particular, Sharrock and Anderson invite us to consider the suggestion (made originally by J. Cook, in his 'Magic, Witchcraft and Science', *Philosophical Investigations* 6:1 (Jan. '83), pp.2–36) that because magical 'systems' 'do not involve beliefs they do not involve beliefs which can be mistaken.' And I think they are right when they go on to say that 'Wittgenstein [and Winch are] not suggesting that magical practices do not have anything to do with beliefs but that they are not *founded* in them.' ('Magic, Witchcraft and the Materialist Mentality' (unpublished), p.7)

9 Cf. Laing, (1965), on pp.33–34: '[T]he expositor of a text has a right to presume that, despite the passage of time, and the wide divergence of world view between him and the ancient author, he stands in a not entirely different context of living experience from the original writer. He exists, in the world, like the other, as a permanent object in time and place with others like himself. It is just this presupposition that one cannot make with the psychotic. *In this respect, there may be a greater difficulty in understanding the psychotic. [than] in understanding the writer of a hieroglyphic dead for thousands of years.'* (Emphasis added.) The reader is invited then to compare *and contrast* the case of (say) incommensurable 'distant' science, like that of Aristotle, with that of the schizophrenic.

10 Should we regret the lack of such a community to sustain a schizophrenic's world-view? I feel the force of this ethical/emotional question, but it may be moot, because in serious cases we have good grounds for thinking that *there could not be* a community to sustain it. Thus, using the term 'form of life' somewhat metaphorically, we might risk saying that severe schizophrenics *don't have* a form of life. Cf. Jaspers (1929/1963: 282–3) 'Schizophrenics.are not surrounded by a single schizophrenic world, but by a number of such worlds. If there were a single, uniform world-formation schizophrenics would understand each other and form their own community. But.[t]hey hardly ever understand each other. A healthy person understands them better. A community of schizophrenics is almost certainly an impossibility, since in every case it has to grow artificially and is not there naturally. In acute psychoses lack of awareness excludes any communal life anyway.' (Cf. also Wittgenstein (1980b: para. 957). So I can't endorse Blankenburg's (1971) view; for while (like Sass) he argues well that schizophrenia constitutes more a *different* than a *deficient* way of being-in-the-world, he does not adequately consider a third possibility: that 'it' rather 'constitutes' various ways of *not* being-in-the-world *at all*. (I extend this line of thought in 2.3, below.)

11 As argued by Sass (1994), chapters 1 and 2; Sass argues, following Wittgenstein's diagnosis of solipsism, that there is an absolutely unstable dialectic inherent to schizophrenic thinking, as to solipsism.

to what others try to impose on them as its 'logical conclusion', Schreber *does* so press his/their thinking. Whether this is deliberate or non-voluntary, it is hard to say, even perhaps in principle impossible, and this too is an important fact. Thus it is that Schreber continually finds himself in a continually-puzzling, circular, paralyzing paradoxicality,[12] and in the absence of self-understanding, in spite of, and indeed perhaps *because* of, all his efforts at reflection.

All this would have the implication that, in the cases which most puzzle us, there could not be a successful interpretation of schizophrenia, for the simple but deep reason given just above: that there simply aren't any true self-understandings available, in this case, against which to triangulate such an interpretation. That such sufferers from schizophrenia do not at key moments exhibit the understanding-in-practice of their own talk, the kind of everyday 'rule-governedness' in that talk and action, which in everyday human contexts we can and normally must take for granted.[13] We might even say that there cannot, logically, be true self-understandings in severe schizophrenics (to facilitate criteria for accurately understanding their words and actions).[14] And without those, there can be no production of descriptions which could be the basis for interpretation(s).[15]

12 See for instance the example given on pp.60–61 of Sass's (1994).

13 The kind of understanding-in-practice of daily life which the ethnomethodologist D. Francis (1994:106) is referring to in the following discussion of the 'primitive society' of the Dene Tha: '[T]o speak of 'Dene Tha reality' *in the singular* is to obscure the fact that the Dene Tha have what Alfred Schutz. referred to as 'multiple realities' – different orders of experience between which they move. In this respect they are 'like us'. But as Schutz was at pains to point out, the wide-awake reality of daily life is the *paramount reality,* phenomenologically speaking. . The world of daily life is paramount in that it is 'home' to our sense of the *taken-for-granted;* it is the order of experience in which much is simply *there* and can be acted towards unreflectively and counted upon unproblematically'.

The 'paramount reality' of daily life just cannot be counted on in the case of – by – Schreber. (But no more should the 'multiple realities' element of the picture offered here of the Dene Than and of us be ignored. Perhaps, again, some people think that it is 'relativistic' to take this Schutzian/Winchian line on reality; for an efficient argument against this thought, see Lyas *(op.cit.)*, e.g. p.24–25; and also below.)

14 For severe schizophrenia is in practice quite largely *defined* by the absence of same. When we try (and fail) to imagine what the speech of someone experiencing psychosis might mean, it *may* be helpful to think of their words along the lines of broken and muddled musical 'scores' or erratic 'compositions' – that way we can abandon the need to formulate a sensible picture of what s/he is saying, whilst remaining sensitive to the fact her/his imagination is nonetheless being struck. (And here we might wish to remember: music usually has a mood or a feel to it.) We must not give up on *people* suffering psychosis just because they are not making sense.

15 We might then risk the following quasi-Wittgensteinian remark: 'If a severe schizophrenic were to speak, we could not understand them.' (Cf. p.54, p.65 and p.67 of S. Glendinning (1998). On occasions, it may in fact be just as reasonable to say that people with schizophrenia have no world as to say what Jaspers says about their 'worlds'.)

'But isn't there a huge tension here between what follows from a Wittgensteinian/Winchian/Kuhnian approach on the one hand, and Sass's version of schizophrenia and solipsism on the other. The former emphasises the importance of the deed, of practice, while the latter involves a perhaps-excessive interpretivism. The schizophrenic interprets everything, according to Sass – that's hardly Wittgensteinian!'

This objection is to the point. But Sass is describing the exception which tests the rule – it may well be that, *contra* the 'interpretivists', Wittgenstein and Winch are quite right on human beings in general, but that, *contra* traditional understandings of schizophrenia, sufferers from this complaint (like some philosophers?) precisely intellectualise and interpret in the manner which the interpretivists wrongly take to be the norm for humans in general! And we may follow Heidegger in suspecting that the reason philosophers are prone to misunderstand human being is that they are precisely those people prone to over-intellectualisations, to think that what is happening is only mentation, where actually something rather different – typically, involving other forms of *doing* – is going on instead.

So, in thinking through the 'objection', just offered, a question I want to ask is: Just exactly how appropriate is Sass's hermeneutic for the special case (of schizophrenia, analogised to solipsistic *etc.* philosophy) under consideration? Does Sass still perhaps over-interpret? Does he thereby, for instance, blind us to aspects of 'schizophrenic' life and talk – and there are typically many – which are perfectly ordinary and normal? This is an important possibility.

Very differently, and equally importantly, does he risk interpreting into sense something which is in the final analysis such an apotheosised and sublimed transmogrification of the interpretive aspects of our mental life that it can be 'successfully' interpreted into terms which we can comprehend only at the cost of radically and violently falsifying it? We might even, then, pose the following question: In Sass's effort to comprehend schizophrenia, is there not a real danger that he goes too far in the opposite direction to the orthodoxy? The orthodoxy is that schizophrenia, insofar as it is comprehensible at all, is only so as primitivity,[16] (or as regression, or as mental deficit, *etc*). Does Sass, in seeking to overturn this view, go too far in the opposing direction, in making schizophrenia appear to hang together and make rational sense *more* than it in fact does?

One possible promising project then, which Sass pursues to some extent but unfortunately tends to drop when he is endeavouring to draw conclusions or generalise from Schreber's case, would be to find those points where *Schreber* is self-consciously puzzled by his bizarre world and 'dialogically' engage with them. Such points may be of crucial importance in testing critically whether any sense can in the end be made of 'Schreber's 'world'.

An interlocutor might object here: 'Do we have then to take Schreber's 'beliefs' at face value? *Can we not* re-present them and ironise them in the slightest? You will find avoiding doing so difficult to carry off, to say the least!'

---

16 Construed, of course, in something more like a Levy-Breuhlian than a Winchian way.

Undoubtedly true, and I will not pretend thus far or in what follows to have succeeded, or even to have tried to as completely as we might.

'Well then, how can you object to Sass's methods?'

Well, I will make an effort on this count. In particular, I claim that we must take Schreber's beliefs at face-value[17] *if we can.* We must at least present his sentences and paragraphs, make a serious Winch-style effort with regard to them, and then ask ourselves honestly whether we can understand them. We must try to do so, and then perhaps recognise that in this case we simply can't succeed, and that we'd be wasting our time were we to just press on endlessly, regardless. My worry is this: that 'interpretation' of them is a dubious half-way house between the claim to understand and the admittance of incomprehension. It gives the *illusion* of understanding, while tacitly changing the subject.

For if all that an interpretation delivers is a confusion – an illusion of interpretation, an interpretation as thoroughly-confused – then no progress has been made. Confusion is not a mode of understanding. Or, more precisely: a thoroughly confused understanding is not an understanding of thorough confusion …

To summarise: I think that Sass's account is *not* as effectively Wittgensteinian as it might appear at first sight. This becomes evident, when we see how interpretivistic, and thus non-Winchian, his account is. On the other hand it may not follow that even someone who understands Winch will be able to offer anything worth calling a Winchian account of serious cases of schizophrenia. (For, if it can make no difference, to return to the epigraph to this Section, how one describes a schizrophrenic's unavailability to love, as Wittgenstein hypothesises, then there cannot be such an account. For the possibility of giving such an account depends, as with the therapeutic/later-Bakerian version of Wittgenstein in general, upon the potential acceptance of the account by the person in question, or at the very least upon us being able to marshall considerations in favour of one description rather than another. But just this is what cannot be done, when it is all the same whether one says 'he does not love, he cannot love, [or] he refuses to love.'[18])

I shall now explore these two points a little more fully, in order to reach a judgement on exactly what lessons we should take from the example of schizophrenia with regard to our fundamental concern, the reassessment of the achievement of Ludwig Wittgenstein and Peter Winch in the methodology of 'the human sciences'. What exactly should we conclude, from the way in which we cannot, it would seem, *contra* Sass, do for severe cases of schizophrenia what Kuhn did for Aristotle and for phlogiston etc. and what Winch did for the Azande etc.

Like Winch and Wittgenstein, I am keen indeed to not misunderstand other human beings, and I am interested in understanding what they say, in making sense of it insofar as it is possible to do so. Like Winch, and many others, I am convinced that it is often the case that insufficient effort is taken by philosophers,

---

17 Which, incidentally, does not require us to be credulous about them – see my 'taxonomy' of interpretive options, later.

18 See epigraph to this Section for fuller quote.

psychiatrists and historians of science (and so on) to *understand*. And, rather like Sass, I am impressed by the lucidity and (in a certain sense) logicality of the patterns of life and language which can be found sometimes in the midst of the floridity *etc* of people with (for example) schizophrenia. But sometimes, after much trying, one ends up judging that it's *not possible* to do what Winch *et al* would want us to, and what they succeed in doing in some remarkably hard cases; in which case one ends up instead noting the patterns in a discourse but concluding that nevertheless there is an irrevocable incoherence in that discourse.[19] This is an incoherence that, of course, cannot be understood – for there is in the end nothing to understand in incoherence, in nonsense, in nothing.

When there is nothing to understand, then the Winchian project of avoiding misunderstandings that resulted from an over-readiness to theorise is not open to one. I might try putting this point as follows: there's no such thing as succeeding in not misunderstanding nonsense. (The likes of this, as I hope is becoming clear to the reader, is itself 'only' a grammatical and transitional remark, and as such is itself prone to being misunderstanding.)

I am not, for example, as part of a philosophical theory, asserting something such as the following: ''Schizophrenics' sentences, because of how they are put together, are as a matter of fact nonsense.' I am simply suggesting that one attend closely to the features of some of the discourse of some sufferers from severe schizophrenia which, their superficial appearance notwithstanding, are usefully seen as quite deeply different from ordinary purposive sentences in context. This seeing-as, as Koethe (1996) has argued, is normally best strictly distinguished from the seeing of facts.

I have argued (see especially 2.1, above) that Winch's philosophical suggestions, his hints and reminders, are extremely effective. The case I have briefly laid out, for saying that there are cases where Winch's thought as found in his major works will not significantly assist us in avoiding misunderstanding (a small minority of) human actions and words. The limits of Winch's reach only throw *into clearer relief* how very subtle and useful his thought is in the great majority of cases, and in particular in cases of significant difference which are liable to lead to philosophical puzzlement. The exception really does test, and confirm, the rule. I see no grounds whatsoever for thinking that Winch's commitments hereabouts, those commitments that have informed my discussions in this Section so far, involve any relativism. No more do they involve any of the other 'isms' that have been bandied about by Winch's 'interpreters'.

What follows then is an attempt to set out an overview of how a Wittgensteinian can find their way about the human sciences. It is, we might say, *a Winchian taxonomy of options facing the 'interpreter':*

---

19 Here I presuppose a Cavellian rather than Rortian reading of Wittgenstein. That is, a non-absolute distinction for certain 'practical' purposes between speaking 'inside' and 'outside' language-games. For detail, see the papers by Crary, Cavell, and Conant in Crary and Read (2000).

A Taxonomy of 'Interpretive Options'

Firstly, a cautionary note: I use the word 'interpretive' here, keeping it scare-quoted, only for lack of a better and more obvious one – some of the 'options' which follow, I want to say, do *not* actually involve *interpretation* as that word is often used in philosophy. I lay out what have emerged as the 'interpretive options' facing the would-be social student of the recalcitrantly strange; say, the reader of Schreber. The options may be taxonomised roughly as follows:

(0) Non-interpretation: Explanation Simpliciter

At one extreme end of the spectrum of available options, here, we have schizophrenics' utterances construed simply as word-salad, and the suggestion that any apparent order here is utterly illusory, and that only scientific explanation based on there being a deficit, a brain malfunction *etc.,* can help us to give any kind of account of the phenomena in question. I reject such superior knowingness. There is a *respect* in which my own view on these matters, as sketched above, bears *some* relation to (0), while avoiding its obvious and crude reductionism and scientism and its abject refusal to *try* looking at and hearing the actions and words of people with schizophrenia. The novelty of my approach lies in claiming that some schizophrenia is in the final analysis uninterpretable even from a Wittgensteinian perspective.

(1) Literalist 'Interpretation': Taking the Patient at his/her Word.

This approach, which is potentially attractive on the grounds of its endeavours to avoid *over*-interpretation, splits into two further options, which, while apparently very much opposed, are in fact crude mirror-images of one another, *sharing* as they do a certain 'literalistic' approach to 'schizophrenic language':

(1a) Massive Error This is the classic version of the account of schizophrenics as exhibiting 'poor-reality-testing'. Schreber is simply making massive and horrible **mistakes** in his reasoning all over the place. Sass has shown effectively the massive limitations of this option, which usually shades into (0) above.[20] The analogous option in cultural anthropology is that of Frazer and his 'descendants'.

(1b) Credulousness toward the strange (in this case, the patient) Here again, no ironising of Schreber's words is undertaken, but at the drastic cost of a commitment to believing in what Schreber believes in. He doesn't make errors because, incredibly, what he says is / may well be true. Something like this is often attributed to Winch (and occasionally to R.D. Laing). However, Winch no more thinks that 'There simply really are witches, if they say there are' than he

20 See Sass (1994: 3).

thinks 'If they say there are witches, then there simply really are witches, for them'. It is worth noting that 'relativism' hereabouts is normally systematically ambiguous in hovering unstably between these two options. But neither option is in fact any good.

Winch thinks that the general mistake in (1) is that one *assumes* that one knows what they – those who are an enigma to one – are *saying*: but the whole point of social study, for Winch, is *to find out* what we can intelligibly say (about what) they are saying.

(2) The Apparently-Strange But In Fact Not: in this Case, the Patient is not Insane (perhaps the society, by contrast, is).

This Goffmanian dramaturgical/translative interpretation appeals to some who are Anti-Psychiatriacally inclined, and has some affinities with (1b) above. Both can seem humane; and both run severe risks of romanticising and of failing to acknowledge the real depth of the problem, in severe cases. There may be some truth in option (2), as indeed with all of these options, but it is extremely implausible that (2) could be anything like a complete account, in many cases.

(3) Strong Interpretation

This interpretation is manifested prototypically by psychoanalytic approaches, but also by various other psycho-dynamical and psychological readings. Sass usually takes his main opponents to be following option (0), option (1a), or perhaps most ubiquitously the current option, (3). This is the option of 'interpretation proper'.

I suggested above that Sass *himself* does not escape from engaging in just such impositional interpretation, and what I have been trying to suggest involves setting out the worry that, if Sass is roughly *right*, as I suspect he may well be, in taking the word 'delusion' if applied to many cases of schizophrenia to be possessed of a grammar quite unlike that of 'mistake',[21] then the problem is that there is no longer a criterion available to distinguish an adequate interpretation of such language and action from an inadequate one.[22] If we can't know when someone is making a mistake, because they could plausibly be 'delusional' in any given instance, and in their reflexive accounting of it (if any), we simply cannot be meaningfully said to understand them. Thus the project of interpretation ought to be *set aside,* not (as arguably it actually is, by Sass), revised and radicalised.

21 See Sass (2004:24f); Sass argues that delusions are no more simple cognitive errors than 'solipsism' is a straightforward stable doctrine. Schizotypal delusions, he suggests, are not believed univocally, and form a delusional 'system' alternative to that of daily life, *etc.*

22 One starts to realise that it is *too* easy for Sass's interpretation of schizophrenia to 'deal with' just about any objections to it.

(4) Winchian description[23]

In adopting this option, which obviously bears some analogy to (1), above, one attempts to avoid *interpretation*, which might make the patient appear either just like oneself after all, or alternatively perhaps so radically different that imposition upon them will be unavoidable if one is to say anything at all about them. One attempts, that is, to understand a strange system or practice by understanding it *as* a strange and *different* system, unlike (1) or (2), above, but not one so different that it simply cannot be understood, and, in an important sense, after one has done the necessary work of 'distancing' in order to put oneself into a position to understand, *simply* understood. This is where (4) differs from (3) (and of course where it learns from Kuhn's ideas about incommensurability: see Section 1.3, above). One might then attempt, for example, to avoid ironising, *via* talk of solipsism *etc*., Schreber's talk of the supernatural, but *not* through credulousness, as in (1b). Rather one would not assume too quickly that one understands what 'the supernatural' *is,* and would attempt *to find out* through understanding-in-process the rules of the language-game being played.

This is the key possibility made manifest by Wittgenstein (in relation for instance to ordinary language when it appears strange to the philosopher), by Kuhn (in relation to the strange in the history of science, including much of that that to the Whiggish eye doesn't *look* strange), and by Winch (in relation to the Azande, etc.).

I would argue that this last option (4) is genuinely the most attractive option for most apparently strange practices, and that it works very well for the cases which have dominated the philosophical literature, notably, the *Azande*. However, I have tried to suggest reasons why Winchian description and understanding does *not* do for us with regard to some *'schizophrenic* language' what it does for us with regard to much other apparently 'alien' language and life, such as that of the Azande, who at least engage in dialogue, and have a shared world.

(5) Nonsense

I would urge that one is *reluctantly* pushed, having worked through the options listed above, toward (5), this surprising and at first very unattractive view that the upshot of a careful consideration of Sass *et al* is that sometimes we will simply have to conclude that certain central strange and genuinely 'alien' (psychopathological, etc.) phenomena are in the end only in appearance interpretable, and that describing them does not issue in understanding. (5) is an option of last resort, not only for philosophical but also, certainly, for ethical and

23 I use the term 'Winchian description' here, but, as will become clear momentarily, another useful and interesting example in this connection is that of completely outmoded sciences or scientists, who at least had something like a – as Kuhn puts it – 'research tradition'. Take for instance once more Kuhn's account of (his encounter with) Aristotle.

socio-political reasons. However, sometimes, Schreber *et al* exhibit language which, in spite of its having a kind of systematicity, is latently and ultimately *plain*ly nonsense. This is my provisional conclusion, on the issue of whether there can be exceptions to the Winchian rule.[24]

So, I have concluded that approach (5) may on occasion be the right one. *Sometimes,* Wittgenstein thought that others would remain a complete enigma to one; and here (in extreme psychopathology) is a set of real cases where arguably he was *evidently* right to do so. Some philosophy cannot be made sense of: the same is true, perhaps, of some lives. There is no good reason *a priori* to think that every single human/social phenomenon must be comprehensible, or indeed intelligibly interpretable. But, taking approach (4) above whenever it is (necessary and) *possible* to do so seems to me, again, rather plainly to overcome the various interpretive 'isms' one may otherwise be drawn into, and more besides. Normally, *if* some social study is required, it will be able to issue in some useful description and the avoidance of certain misunderstandings. This is Winch's way.

Thus armed, let us for a moment continue the thinking that we were engaging in in 2.1, above. One important reason, which I shall come to in a moment, why Winch has so often been misunderstood, in addition to the ignoring of his later work, by which is meant in this context, unfortunately, all or all bar one of his papers and books after 1958 (including notably the revised Preface of *ISS),* is the ill-advised *assimilation* of him to 'interpretivist' and 'hermeneuticist' approaches. Second, there has been a failure to emphasise that Winch's task is primarily *negative* (i.e. his task is the avoidance of 'philosophically engendered' (thinking of the word 'philosophically' now in a very broad sense) misunderstanding, and nothing more). After all, to have done more would have taken much more space than was present in the short book he wrote! Third, we need to note once more the frequent lack of understanding of the work and of the conception of philosophy of Wittgenstein which usually gives the 'rule' that Winch is following (and in particular a lack of understanding of the very great extent to which Winch tends to follow Wittgenstein's work understood fairly resolutely (and) *as a whole,* and not only a certain problematic rendition of his 'later' philosophy). Fourthly, there has been a failure sometimes to differentiate carefully between cases to which Winch's particular interventions in the philosophy of the social sciences to a greater *or* lesser extent can be analogously applied. For example, there has been a failure to see not just the similarities but also the *differences* between understanding a

24 Again, I trust that the *differences* between my view (5) and a *scientistic* non-interpretivism ((0), or (1i)), are clear. My view is not Kraepelinian nor even Bleulerian, for I think that *sometimes* silence (or even word salad, or Schreberian speech) can, as Laing maintained, be an eloquent means of *communication;* and because it is not just some portion of schizophrenic language that is best judged to be nonsensical, but also most philosophy. My view, my conclusion, is provisional only in the sense that a full investigation of these matters would take more space than I can allow here – for a somewhat fuller investigation, see Read (1998).

local sub-culture, understanding a 'primitive' society and 'understanding' (say) a serious case of schizophrenia.

The additional reason that Winch has, I contend, been misunderstood, is the following one: readers think that, because he writes in more ordinary prose and with more ordinary discursive structures than Wittgenstein, therefore it is less dangerous to 'translate' what Winch says into the lingo of 'analytic' and theory-driven philosophy. In short, even most of those (still, regrettably, the minority) who are leery of telling us what the 'theses' are which Wittgenstein advocates have no such caution or compunction when it comes to Winch.[25] They assume that he can be read as having a theory, as putting forward various theses, and they fail to see what his methods in philosophy are, and what his conception of the subject is.

Such a view is mistaken, because it is, I suggest, truly a Wittgensteinian 'methodology' that Winch adopts, such that it is more helpful to say of Winch that he is not asserting anything at all than to say that he asserts various 'controversial' philosophical theses (e.g. 'pluralism', 'monism', 'relativism', or whatever). Winch, on my reading of him, doesn't say *anything at all,* in the sense that he merely tries to offer a cure to his readers, be they professional philosophers or professional social scientists or whosoever. He offers a 'cure' for intellectual 'disease', and the cure as a set of targeted hints, questions, provocations and so on, is no set of assertions *at all*.

In the first instance, we understand only the 'ordinary' or 'everyday' things that we and others do, which amount to almost everything. Unusual, 'extraordinary' things can, if we try and are fortunate, be understood through instances of social or socio-historical study, especially on the model of (4) in the taxonomy above. There is no task for a general social science here, merely particular tasks of repairing breaches in our ability to 'grasp' or meet others. The utterly bizarre, the irreparably other, the residuum which unfortunately *cannot be understood at all* can, however, be described or 'interpreted' arbitrarily many different ways. This is (5) in the taxonomy above.

---

25 Interestingly, this cause of Winch being misunderstood has much to do with the misunderstanding of religion, 'primitivity' *etc.* in the first place. For religious people, for example, mostly use the same words as non-religious people, even in the course of their religious practices. E.g. words like 'love', 'hope', 'death', *etc.* To the unwary, these can seem an invitation to spring to a conclusion as to what it is that they are saying, doing, and so on; the kind of hastiness that Wittgenstein found in Frazer, and Winch in Evans-Pritchard. (Cf. also *PI* 11, and *Culture and Value* p.15) Winch's writing may look to the casual observer much like Fodor's or Davidson's; but it ain't so. We can succeed in reading Winch as not advancing theses, if we accept the invitation I am proferring here: to re-read Winch's early in the light of his later work, especially, in the late of the later work that he himself intended as re-contextualising his early work: especially, the 1990 Preface and his 'Understanding ourselves' (*op.cit.*), a vital 'sequel' to 'Understanding a primitive society'. (See 2.1 above, for detail on this point).

In fact, when we really *understand* this, we may well find it most useful, least misleading, least confusing, to say: There's *nothing* there to understand. We are faced with nonsense.

Let us consider very briefly then the famous case of the schizophrenic patient Renee, who Sass discusses at some length, to attempt to illustrate this, by reference to McGilchrist's take on the left and right hemispheres of the brain. Renee tells us that everything seems to be alive, but that what it is for everything to be alive is simply for these things to exist. Hard to understand, to say the least. We might see Renee's vision of what it is for things to be alive as an extraordinarily … dead and deadening one. This, from McGilchrist's point of view (and he draws notably, repeatedly, in his book, on John Cutting's understanding of disorders of left-brain hyperactivity or of right-brain deficit as including quintessentially schizophrenia), is to be expected, of a left-brain running wild. When Renee says, ''the thing' sprang up', and then follows this by telling us that the lives of these things consisted uniquely in their existing, and nothing more, this is a take on the world in which real life somehow no longer seems possible – and a take which is not comprehensible without violence being done to it.[26]

But have I just refuted myself? Have I just *given* the relevant interpretation of Renee's lived world?

Yes and no. The 'no', however, is crucial: For, as I say, we can say the kinds of things I just said about Renee: but they only delay the point slightly longer at which interpretation must come to an end, and we must accept that 'this' cannot be worded in a way that we can imagine actually inhabiting. We are faced, it would seem, with lived nonsense. Which is to say: a roundabout form of nothing. A non-world, or an un-world. A world that is entirely 'alive' – and yet whose life consists in nothing that we can recognise as life …

What Sass analogises to schizophrenia is thus crucially different from what Winch analogises (and disanalogises) to the Azande. Solipsism, unlike (say) Christian prayer, or of course science (including defunct science), is sheer nonsense, a restless and relentless failure to mean. So I am not primarily in the business of denying that Sass's analogy works – though I am suspicious of it, as explained above; and I am suspicious of his mode of presentation of it, as a successful interpretation, rather than simply as a comparison (see *PI* section 130), a comparison which *may* lessen our misunderstanding of something deeply strange. What I am primarily in the business of is suggesting that, *even if the Sassian analogy 'works',* even if schizophrenia can be said to be analogous to solipsism,

---

26 Incidentally (and this is a point I shall develop in 2.3, below): One could begin to make a speculative case here that there is a worrying similarity between Renee's '(un-) world-view' and that of 'social science' itself. Doesn't social science itself tend tacitly to figure people as things in something like this way? Doesn't it deaden the world, through a reification of unending scope? If this is right, if *the ideology of social science is itself a left-brain perspective running wild,* out of control, then it shouldn't be so surprising that it so frequently issues in nonsenses.

still the analogy is *no good* to us in a central respect. For it has *no* positive analogical content. Nonsense has no content. So the idea that we understand solipsism as a doctrine, with a distinctive and determinable phenomenology, is an illusion. This breach, unlike the breaches dividing us from people we find odd, such as perhaps Christians, or fanatical football fans, or 'primitive' peoples, is irreparable.

At the heart of Winch's conception, as we have seen, is the idea of looking at the game, and looking for both what is helpful and what is *wrong* with possible analogies for it. Not looking for *the* analogy which 'gets it dead right' – which 'successfully interprets' it. Insofar as someone claims, 'Here's how to understand *x* fully!' where *x* is a society (or a practice, or a person), one ought to be very wary. Especially if involved in the claim is something which can arguably not be rendered effectively as anything other than nonsense!

Where Winch casts light on some object of social study, he does it to help repair a particular breach[27] and that is all. He has no grander quasi-scientific, 'social scientific', epistemic or metaphysical task in the philosophy of the social sciences. If it looks otherwise, that is because of certain ill-advised formulations in the 1958 edition of *ISS*, and because commentators forget that Winch throughout his work *presupposes* Wittgenstein's conception of philosophy, and *enacts* it. Recognition of the Wittgensteinianism latent throughout Winch's corpus is what I have tried to bring about in this – and the preceding – Section.

What we have seen in this Section, more positively-put – and this, I think, is quite exciting (and we will see something similar in 2.3) – is how a properly Wittgensteinian sensibility, put to work among the human sciences, can illuminate more than any scientistic approach. If, rather than attempting to approach 'the mind' purely scientifically, and to pathologise with the categories of correct and incorrect function, error, etc., we look at the inspirational example of and the creative thinking of someone like Sass in relation to psychology and psychiatry, and then take care to stay truer to Wittgenstein in *our* methodology than Sass manages to, we can (then) come to have a saner and less confused and less reificatory relation to 'the mind', in our studies of it, than the usual philosophy and practice of psychology and psychiatry make possible. A broadly Winchian approach to the phenomena that Sass sheds some light upon, open to using the philosophy of nonsense that Wittgenstein makes available – open, that is, to the possibility that the human contains the capacity for irredeemable nonsense, for nothing(s) that masquerade as something(s) – can then actually contribute to the projects of psychology and psychiatry. These become richer, and less prone to scientistic crudification, than they did before we intervened. That is what the example that has been worked through in this Section, I believe, shows, and that is one thing that already the 'taxonomy of 'interpretive options' offered above, that I hope will be of much wider use, helps to establish.

---

27 Similarly, Sharrock and Anderson (1989:10) observe 'In rejecting Frazer's theory, Wittgenstein is not mounting an emotivist or symbolic alternative of his own. Some magic may turn out to be symbolic but equally other forms may not.'

There is no Relativistic sundering of us from severe schizophrenics (as one might supposed if one had bought into a Relativist reading of Kuhn or Winch on incommensurability and then tried to apply it to this case). Nor is there simply a successful novel interpretation available of them into our own schema (as Sass supposes). Rather, they provide a potential exception to test the 'rule' offered by Winch (and somewhat similarly by Kuhn); they show how far that 'rule' rules. They occupy a place, I have suggested, via my taxonomic schema, 'marking out' the edge of the limits of sense.

***

In this Section, I have tended to accentuate the negative, so far as my take on Sass is concerned. I have indicated differences between Kuhn and Winch on incommensurability and understanding, on the one hand, and Sass on interpretation, on the other, and have suggested that these differences do not redound to Sass's benefit. In 2.3, below, I accentuate rather more the positive. In seeking to understand (just as far as it is possible to do so) certain psychopathological/extreme emotions (if that is even the right word for the phenomena to be discussed).

2.3 Extreme Aversive Emotions

> Anyone who listens to a child's crying with understanding will know that psychic forces, terrible forces, sleep within it, different from anything commonly assumed. Profound rage and pain and lust for destruction.
>
> Ludwig Wittgenstein, *Culture and Value* (1998).

> Undifferentiatedness attends the already secured standing reserve of the Un-world (*Unwelt*) of the forgetfulness of being. The earth appears as the Un-world of madness. This is what is being-historical in madness.
>
> Martin Heidegger, *Vorträge und Aufsätze* (Stuttgart: Neske, 1954), p.93.[1]

In this Section, as in 2.2, but still more directly, I aim to put a question-mark before the scientific ambitions of psychiatry. I aim, in particular, to characterise the extreme aversive emotion of psychotic and quasi-psychotic psychopathology that I will call 'dread', but also to call into question standard attempts to make such a characterisation and any over-ambitious rendition even of my (way of) characterising 'it'. This is intended then as a counter-example to a scientifical rendering of psychiatric discourse.

More specifically, my objective is to indicate a 'deep-grammatical' difference between what we might term (i) fear, (ii) anxiety, and (iii) dread, using as a starting point not so much the usual (in philosophy) Heideggerian and Kierkegaardian reference-points, but rather (a) various remarks of Wittgenstein's, such as his wonderful *apercu* about the impenetrably extreme emotions of a young child, and (b) Louis Sass's 'Wittgensteinian' reading of psychotic psychopathology (See 2.2, above, for this).

To flesh out (b) a little further: I will understand such psychopathology as fairly frequently manifesting what I am calling *dread*, and as resulting, very roughly, from an inability to dissolve metaphysical problems which come to obsess one to the point of such dread being very present, where it is almost entirely absent in (say) Descartes' *Meditations*. I will take dread (as in psychotic

---

1 Thanks to Tom Greaves for this quotation and for big help in translating it. This quotation comes from a discussion of the 'principle of efficiency' that Heidegger believes determines our age, and from which comes 'undifferentiatedness' or loss of difference. This provides a link then from the theme of how to capture the phenomenology of madness to the theme of my next book, on the philosophical foundations of 'environmental science' and on the madness of our treatment of the Earth as a 'resource' (a 'standing reserve'): see the closing segment of this Section (including the final footnotes of it) and the final footnote of the 'Concluding Summary' to this book, below.

psychopathology etc.) as a kind of paradigm of what I am interested in here, what I think that mainstream psychology and psychiatry just don't *get* very well: extreme aversive emotions.

Thus, I will urge that a Wittgensteinian investigation of the 'pathological emotion' of dread (iii) – an emotion far stranger than fear or sadness or grief (i), and stranger even than 'merely neurotic' anxiety or depression (ii) –[2] establishes its nature. But that such establish-ment is itself limited in nature, compared to that which we are inclined to desire (especially, if we are captivated by 'the scientific image'). In this way, this Section links directly with the latter parts of the 'interpretive taxonomy' explored in the previous Section (2.2).

Furthermore, and as explicated in the first four paragraphs of 2.2, above: this Section seeks to show how philosophy itself plays a role *in* the best / in some needful psychiatry. This in itself suggests a powerful reason for doubting that a purely scientific approach to the subject can possibly hope to succeed (Unless we count philosophy itself as a science, which of course as a Wittgensteinian I would consider to be a disastrous and self-deceptive manoeuvre.).

Those who seek philosophical illumination or perspicuity as to the nature of 'dread' often look to Kierkegaard or Heidegger. And those are pretty good places to start looking. But these great philosophers do not, in my view, go quite far enough to enable the radically-aversive to be, in the end, understood as best we can understand it.[3] What I will do here is briefly to outline how reading and understanding Wittgenstein can shed a somewhat distinctive light on 'extreme aversive emotions'; *and* can do so in a way that avoids the dubious theoretical commitments common to mainstream approaches in philosophy of the emotions, such as those of most Anglo-American Cognitive Science, and sometimes also those in the work of 'continental philosophers', for example Martin Heidegger.

---

2 As will become clear, I submit that there is a qualitative change at some point in depression/anxiety; that these can become imbued with a more or less psychotic tinge or flavour or character, and that that is one point where it starts to make sense to talk of *dread*. As it were: The fear and trembling in the face of God is become at some point a dread in the face of existence itself, putting what existence *is* into some doubt (See the brief discussion of 'Renee' in 2.2, above). So long as the category 'God' is stable, there is a limit to *angst*; that limit can be breached, when (for instance) the possible *character* or *nature* of God becomes entirely open (e.g. perhaps God is entirely malevolent). I will not be examining Heidegger or Kierkegaard here; but I will indicate very briefly why I take (my development of) Wittgenstein's philosophy to be better able to cope with more-than-neurotic conditions than they. (In a fuller presentation, I would also dwell on what I mostly just pass over here: the deep *differences* between various psychopathological conditions (I am going to say relatively little, for instance, about the category of so-called 'affective disorders'), and the deep doubts, that I share, over whether such diagnostic categories as 'schizophrenia' are even well-defined; one certainly cannot as a philosopher have much methodological faith in the DSM definitions thereof.)

3 A possible exception being the late aphorism of Heidegger's that is the (second) epigraph to this Section.

In short, the key argument I will make in what follows, building on Wittgenstein's thinking, is that dread is best understood as involving not (as 'ordinary' anxiety or depression does) a different *weltanschauung*, but something more extreme: a kind of loss of *welt*.[4] That is, a felt *weltverlust*, or a perceived and (and this is natural, and critically-important) at-times-overwhelmingly-feared *loss of world*. This, of course, if correct, has no parallel within the history of science; it goes beyond anything we can gain an understanding of via a comparison with the phenomena whose understanding we aimed at in Part 1 of the present work; and it has no parallel either even within the life-world of (say) the Azande as offered to us by Winchian (re-)vision.

We are going to have to take another mode of approach; one much-influenced by though non-identical with and in a certain sense more radical than Sass's.

To start with, let us recall a remark from near the close of Wittgenstein's *Tractatus Logico-Philosophicus*:

> If good or bad willing changes the world, it can only change the limits of the world, not the facts; not the things that can be expressed in language. In brief, the world must thereby become quite another. It must so to speak wax or wane as a whole.
>
> The world of the happy is quite another than that of the unhappy. (TL-P 6.43, emphasis added)

Put that alongside the relatively-little-known passage from *Culture and Value* that I chose also as the epigraph for this Section:

> Anyone who listens to a child's crying with understanding will know that psychic forces, terrible forces, sleep within it, *different from anything commonly assumed*. Profound rage & pain & lust for destruction. (Wittgenstein (1998:4), emphasis added)

One might approach this remark of Wittgenstein's in the following way: the world of the young child is quite another than that of the adult. Elsewhere, I have interpreted William Faulkner's *The Sound and the Fury* as an example of 'showing' this profoundly different emotional life, with all its torments and furies, through brilliant literary-conceptual artifice.[5] More crucially for present purposes, I suggest there at some length that this is a useful object of comparison for the task of understanding at least something of something at *least* as profoundly different/

4 This might be connected to Heidegger's late notion of the 'un-world', as instanced in the epigraph to this Section. This goes beyond the usual Heideggerian concern with transformed worlds, poverty of world, the 'unworlding' of the world, etc.

5 See especially Part 2 of Read (2007a).

difficult: the mind of one caught in the profound suffering of serious mental disorder.[6]

My suggestion thus far then is: that the *world* (in roughly the *Tractatus'* all-encompassing sense of that word) of those suffering from more or less psychotic disease truly is quite *profoundly* another that that of those who do not.

This species of 'profundity' is (to say the least) hard to grasp. As laid out in 2.2, above, the hermeneutic task of understanding the cognitive and emotional states (the 'world') of those suffering from 'schiz spectrum' disorders or from the harsher of the affective disorders (i.e. severe/semi-psychotic depression, some semi-psychotic moments in serious anxiety-conditions, etc.) can be significantly harder even than the deeply-challenging task of understanding a 'primitive society' (e.g. as investigated by Winch), or understanding outmoded science (e.g. Thomas Kuhn on Aristotelian physics, on the model of 'partial communication', properly understood – see 1.2-3, above). The worlds of the Azande, or of Aristotelian physicists, deeply different and distant though they are, are far 'closer' to our normal world than is the 'world' of those in the grip of dread. Winch's Evans Pritchard's remarkable Azande, and the long-dead and irrecoverable Aristotelian worldview, are yet almost closer to being live options for us not in the grip of (what I am calling) dread.

For it is not just, as Winch and Kuhn have rightly taught us, that the world 'waxes and wanes as a whole' in ways beyond what is commonly assumed (where what is commonly assumed focuses often upon the hope that we can come to understand the 'primitive' or the outmoded through understanding a different but comprehensible set of beliefs, beliefs we try to see as getting as far as being false). Rather, *the very sense of security offered by the notion of 'world' itself starts to give out.* The world 'wilts' on one, or thrusts itself upon one in ways that no world should.[7] This is why psychopathology is sometimes gestured at through such wonderful (gnomic) terms as (e.g.) 'the unworlding of the world'.

---

6 Compare here this intriguing remark, on p.62 of Wittgenstein's *Culture and Value*: '"It is high time for us to compare this phenomenon with something *different*" – one may say. – I am thinking, e.g., of mental illness.' (In a longer presentation, we should also consider the relevance to the present topic of Wittgenstein's insistence in *PI* 320–5, and 464–494, and in *On Certainty* 56–151, that doubt not only requires *grounds* but should issue in *action*: This is another way in which, as Louis Sass argues, psychopathological doubt-like phenomena, of which 'dread' is one, tend to be consistently strange, expectations-defying, abnormal.)

7 Powerful examples discussed by Louis Sass in his (1992) are remarks of de Chirico and of 'Renee', which are worthy of examination should the reader wish to take this aspect of my topic further. Compare for instance this passage from de Chirico (quoted on p.43 of Sass): 'One bright winter afternoon I found myself in the courtyard of the palace at Versailles. Everything looked at me with a strange and questioning glance. I saw then that every angle of the palace, every column, every window had a soul that was an enigma.' This led to de Chirico's famous conclusion that one had to 'live in the world as if in *an immense museum of strangeness*.' (italics added)

The key experiences of derealisation and depersonalisation, for instance are, I would suggest, profoundly *paradoxical*, and profoundly *resistant* to understanding. To experience the world, and yet not experience it *as* real world, or to experience the world and yet lose the sense that there is any self, any person at all who is having the experience … An actually experienced *epoche*, the thorough bracketing of things that are pre-suppositional to, and through and through presumed *in* our normal experience; there are in the end no words for 'this', no thoughts for it.

Louis Sass suggests that we can come to have *some* understanding of such experiences, of the crumbling of the world itself, through understanding the rational psychopathology of philosophical illusion.[8] That is: through Wittgenstein's delicate, dialogical understanding of one's attraction to one to those 'nothings' that masquerade as 'somethings'. These are forms of words/thoughts which *flicker* for us between different senses, and so as yet actually have no sense (at all). Sass suggests that the rational procedure by means of which one gets oneself convinced of solipsism, or ends up wanting to utter an inarticulate sound, is directly analogous to the paradoxical hovering between senses that characterises some (e.g.) schizophreniform experience. (Cf. 2.2, above.)

The logic of Sass's argument is that 'derealisation' is (among other things) a name for what it would be to try, absurdly, to take sceptical doubts about the 'external world' seriously. And a good deal of schizophrenia can be understood better than ever before as a lived/felt logical working through of (the contradictions of the nonsense that is) 'solipsism'.[9] Sass sets out all this in marvellous detail, recounting anecdata, interpreting 'schizy' works of art, presenting clinical case-studies – giving rich detail and examples that I will pass over here, but that are strongly recommended to the reader interested in taking study of these matters further.[10]

One can slightly taste such a remarkably alien(ated) mood, in some of de Chirico's marvellous and disturbing paintings, such as his 'The enigma of a day'.

8 Non-Wittgensteinian philosophy is as rational as psychopathology is… This, contrary to appearance, is however no insult. All these are human possibilities, *rational* possibilities . For more on how I certainly do not mean to be 'othering' the mentally ill, see the latter portions of Read (In preparation). For detail on the crucialness of the question of mood to philosophical illusion, see the Conclusion of Read (2007a).

9 For explication and detail, see Sass (1994: 29–75). Read (2007a) sets out also various worries about the limits of Sass's approach, about his sometime tendency to make it seem as if Wittgenstein's reading of solipsism etc. offers a *stable* comparator for schizophrenia, when actually it offers '*only*': something absurd, and a sense of how we are vulnerable to such absurdities. Cf. also 2.2, above.

10 This Section is not the place for examining examples, which are detailed at length elsewhere in Sass's work, mine, etc. This Section is simply setting out a possible mode for thinking of extreme aversive emotions, not attempting to prove that that mode is more fruitful than others. To get closer to such proof, I would recommend also, of course, reading memoirs by the mentally-afflicted themselves, some of which are discussed in Part II of Read (2007).

More recently, Sass (2007) has extended his reading of schizophrenic and some other psychotiform pathologies, to help understand how they manifest paradoxes and contradictions of emotionality, especially the so-called 'Kretschmerian paradox', the extraordinary, seemingly-impossible fact that schizophrenia-spectrum patients can (it would very much seem) *simultaneously* experience both exaggerated *and* diminished levels of affective response. I would add that similar phenomena appear during some of the worst recesses of depression: a state can be reached in more or less psychotic forms of/moments in depression which is at one and the same time both relatively non-aversive and utterly aversive, which is on the one hand both detached and devoid of feeling and on the other (or, perhaps, *because* of this devoidness) as intensely psychological painful as imaginable. Such paradoxes are, seemingly, sometimes experienced by the psycho-pathologically afflicted.

A common feature of a number of extremely aversive psychological conditions is what I tend to term *retreat* (though that word may make the phenomenon sound more *willed,* more reactive and independent, than it is).[11] What is perhaps in common between (what are otherwise very different): firstly, the sufferer from a bad panic attack, for whom the reality of her surroundings temporarily fades or withdraws; second, the kind of extremely depressed person just mentioned, who feels herself in a black pit or black hole, somehow devitalised and walled off from life and from other people; third, the person going through the paradoxical experience of derealisation,[12] knowing the world to be real and yet absolutely not feeling able to believe that it is;[13] and, finally, the paranoid schizophrenic continually trying

---

One recent memoir which would richly repay a thorough such reading is Jeff Cumberland's (2006). Though Cumberland fairly clearly mis-self-diagnoses (Surprisingly for a professional (academic) psychologist, he does not seem to realise (e.g) that derealisation and depersonalisation are not uncommon symptoms of major affective disorder episodes), his unusual degree of philosophical, psychological and psychopathological knowledge makes his memoir of neurosis and near-psychosis peculiarly reflexively powerful and insightful. (See also the interview that Cumberland did for the Lulu.com magazine, which (for ease of public access) I have (with consent) uploaded to my website, under the title 'Cumberland interview', at www.rupertread.fastmail.co.uk .)

11 I have in mind here Sass's excellent discussion, 'Act or affliction?' at the close of chapter 2 of Sass (1992). Our concepts give out here: there *is* no good answer to the question whether the sufferer from schizophrenia merely involuntarily undergoes suffering ('affliction') or acts in ways that co-create her condition ('act'). (Compare the closing discussion (below) of Wittgenstein's important remark from his (1998) on acts and afflictions.) Thus the word 'retreat' is itself no more than an useful 'object of comparison'. 'Retreating' *is* a *way of seeing* the phenomenon I am after here, *not* any kind of straightforward action carried out by the patient/sufferer.

12 And the case of depersonalisation is structurally-analogous, and we could consider it here, had we space.

13 If you are put in mind of Moore's Paradox here, that is certainly no coincidence. I think that Moore's Paradox too can be lived by an individual: see Read (2011a). This doesn't make it any less of a genuine (rather than a 'merely pragmatic') *paradox*; it rather

to systematise an understanding of her threatening 'world', in which the assumed background ontology that we are consensually used to is no longer reliable.

Such 'retreat' is perhaps what these four cases have common. A certain degree of more *or less* willed or inadvertent *inward-facing-ness* inevitably describes the emotional life of one who, roughly, doesn't *take* the world at all.[14]

As Sass intimates, dread results, roughly, from an inability to dissolve the deeply-problematic nature of such experiences. In Descartes's *Meditations*, one gets no sense of such dread; of the extremely aversive effects of feeling as if one is subject to systematic delusion, or losing one's mind.[15] Descartes's is a purely – I would argue, excessively – *rationalist* account of doubt. He splits the emotions, the 'soul', from the mind, in a way that makes it impossible to understand the felt reality of doubt. The way in which absence of world is *internally related* to the emotional experiences I am presenting here. From a Wittgensteinian perspective, one is bound by contrast to highlight this sense: the lived, embodied force of feelings of disembodiment, or of psychological confusion without redress.

---

broadens our conception of what a human life can involve, of 'where' it can lead one, unstably, to 'be'. The *concept* of belief *starts to break down*. And that means simultaneously that *what we ought to say* about what is happening starts to break down, or multiply. (This is one reason why Sass looks to art to illuminate the nature of schizophrenia. A scientific approach sits uncomfortably in a situation where there are always multiple things one can say to illuminate what is happening, and not just one thing.) The concept of belief as it were best fits central cases of itself; there are cases that one can encounter in psychopathology that in the end make it very moot whether one wants to call what is encountered a 'belief' or not. This can even be a genuinely reflexive realisation: see my remarks about Schreber's own awareness of his own nonsenses (which he nonetheless does not give up), in Part II of Read (2007).

14 In talking here of 'taking' the world, I have in mind Phil Hutchinson's work, and in particular his concept of 'world-taking cognitivism'. I am suggesting a possibility in line with that explored in 2.2, above, and not explored in Hutchinson's work: that perhaps there are ways of taking the world so extreme that they start *not* to be ways of taking *the world* at all.

15 See Read and Gregory (2007) for discussion. Therein, we lay out how, while Descartes explicitly states that only a madman would, as a matter of fact, be gripped and perhaps quite taken in by his kind of doubts (as in prolonged 'derealisation'), he just does not give any sense of how terrifying those doubts would be. As we put it there: 'taking seriously the scenario central to the *Meditations*, that of the malign demon, should have generated far more emotional language than Descartes allows himself. The terror that should be consequent upon the demon hypothesis, a terror at total cognitive penetration, and thought-disorder or terminal confusion, not to mention the terror consequent upon the possibility that I have a body and am therefore intensely suffer-able, is barely hinted at in Descartes's text. The focus on the reason of the meditator in radical doubt occludes from view the rational terror at unreason or at torture that should follow from Descartes's own hypothesis.' (Am I being unfair to Descartes in this Section? I think not: Descartes acts as though the doubts he has in mind can be contemplated as merely an intellectual curiosity; but that (e.g.) another being might actually have total control over my mind cannot be actually *contemplated* without dread.)

It is *too easy*, then, it is *insufficient*, to say something like: 'fear has an object, whereas radical *angst* or dread does not.'[16] Fear of nothing in particular (*viz*. lasting anxiety states) is the self-perpetuating attraction of the 'strategem' of retreat as a way of sealing oneself off against the 'threat' offered by objects-in-general.[17] There is *something* right about this insight; but it is as yet too easy, too quick.

The hermeneutical challenge is harder. For the different psychopathologies sketched above do not fail the reality test – they do not simply get the world wrong.[18] And while they are in large part based, I believe, in what we might think of as would-be 'self-protective' manoeuvres, of fearing and guarding against all kinds of possibilities that do not normally bother people much or at all,[19] their

---

16 'Objectless emotions' are generally seen as a problem for Cognitivist'/existentialist/Wittgensteinian accounts of the emotions, and thus some see them as tacitly supportive of 'neo-Jamesian'/Cog.Sci. accounts. See, for example Griffiths (1997). But these emotions concern *ways* of world-taking; Hutchinson's (2008) 'World-taking cognitivism' – in which the world can be 'taken' in radically different ways – completely solves the alleged problem with 'objectless emotions', I think. And his approach is highly-consonant with mine, as I hope is obvious. But, whether all this is so or not, my approach to *the extreme aversive emotions* sidesteps the alleged problem altogether. For I am suggesting that *these* are not properly construed as objectless emotions. They are emotions which are better characterised as internally-related to a *loss of* objects/of world, altogether.

It might nonetheless be claimed that I am assuming (without giving any argument to support the assumption) that 'world-taking cognitivism' is broadly true, because I am *assuming* that persons take the world cognitively/emotionally, or fail to do so, in consequential ways set out herein. …There is probably some crude level of truth in this claim – i.e. there is probably some trivial (non-)thesis along these lines that I don't see anyone being able to object to. Simply because I fail to be able to think about *human beings* at all, without making *some* such 'assumption'.

17 Laing brilliantly depicts this in the closing sections of his (1965). Furthermore, much of my enterprise in the present paper is clearly traceable back in intellectual lineage to Laing's (and Sass's) thoughts on 'ontological insecurity'. (Note that I use scare-quotes around the term 'strategem' because of concerns about whether what we are speaking of here is an act or an affliction. As set out elsewhere in this Section, whether intentional descriptions are fully appropriate hereabouts is itself an important – and imponderable could consider it here, had we space.

18 This claim is justified in Sass's (1994): Sass uses the metaphor of 'double book-keeping' to try to gesture at the far stranger reality of schizophrenic delusion: that the delusions do *not* generally displace an accurate grasp both in theory and in practice of the physical and social world. The world is somehow present and yet lost simultaneously; and this simultaneity of contradictories is itself a source of distress or confusion, a push to think through one's life and mind further, a thinking through that is usually pathological, increasing rather than reducing the problematic.

19 In a fuller presentation, I would wish to examine in some detail the key nugget of insight in the generally (in my view) unilluminating presentation of neurosis and psychosis alike in cognitive science and 'cognitive psychology' (NOT to be confused with 'cognitivism' in Hutchinson's sense); that nugget of insight is the possibility of generating anxiety, including even extreme anxiety, through the risk-averse strategy of considering

phenomenology goes (self-defeatingly) beyond even that. Perhaps their nature lies in the profoundly different *kind* of world that they phenomenologically present; or better, and harder still, in the absence of a reliable, stable world *at all*. It is the *possibility* of this paradoxical 'non-place' in 'logical space', hitherto all-too-little-noted in the history of thinking about emotion, reason and mind that I am seeking to draw the reader's attention towards in this Section.

In the kind of states I am considering, one is seemingly deprived of the world, of ordinary access to it and of natural presence in it in one way or another. And one is almost certainly further cognitively and morbidly absorbed, terrified or depressed by this, which hardly helps (and explains part of the difficulty in emerging from serious psychical disorder). The difference is, roughly, between 'objectless' fear or sadness –i.e. severe but nevertheless 'merely neurotic' anxiety or depression – and a generalised 'objectless-ness,'[20] a 'non-inhabitation' of a world in which there reliably are objects at all. No object of comparison which itself is capable of relatively straightforward stable statement, presentation, and appraisal is suitable for this possibility.

Winch, after undermining our presumption that we know how to 'place' Zande magic (i.e. as primitive science) had recourse to the object of comparison of *Christian prayer*, to help us see the Azande (see 2.1 and 2.2, above); Kuhn helped us see Aristotelian physics by first taking us about as far from the Newtonian conception as it was possible to go, and giving us another world view (see Part 1, above). The problem of what I am calling extreme aversive cognitions/emotions is harder. An object of comparison which illuminates by similarity must in the present case be an 'object' which constantly shape-shifts. The situation is worse even than just objectless-ness, for that too is, or at least *sounds static*. It is not that what I am calling 'dread' is fear without an object, but that it is the *absence* of a stable world in which to place anything so 'harmless' or sane as fear. One has *withdrawn* from the *world*; or again, the world has withdrawn from one. The world, the totality of facts that is all that is the case, is not, or is no more. Dread is arguably the emotion naturally '*appropriate'* to this deeply paradoxical state, for radical doubt is not, *pace* Descartes, something that can be merely intellectually contemplated as a curiosity. We must contemplate what it would be like to *live* it, to 'believe' it, if we are to know what radical doubt truly *is*. Anything else is merely going through the motions and is unserious.

Mutual aversion[21] between self and world gives birth to a profoundly aversive state. 'Dread', is the consequence of – I am inclined almost to say, it *is* – the state

---

even the slightest threat or potential threat as if it were a real and present danger. Such 'false positives' play for sure a major role in much anxiety.

20 *Perhaps* Heidegger can be read as meaning this when he refers to there being no 'where' from which anxiety or dread comes, such that a sense of uncanniness can pervade everything.

21 But what of a radical doubt of the world, such as at some moments in some forms of Buddhism, which is not aversive? Doesn't that indicate that my criticism of Descartes *is* after all plainly unfair, excessive? No. Buddhist practice, if it really brackets the world,

of torture of not being at home *at all* in the social and natural world. Dread is the emotion of the world's limits, and not of its facts or things that can be expressed in language (not even of things that are absent, missing or doubtful). Now consider, in light of the above, the following sequence (part of which we have already considered, in 2.2) from *Culture and Value*:

> The greatest happiness for a human being is love. Suppose you say of the schizophrenic: he does not love, he cannot love, he refuses to love – where is the difference?
>
> 'He refuses to' means: it is in his power. And *who* wants to say that?!
>
> Well, of what do we say 'it is in his power'? – We say it in cases where we want to draw a distinction. I can lift *this* weight, but I will not lift it; *that* weight I cannot lift (Wittgenstein, 1980: 87).

Wittgenstein captures the argument I have been essaying in this Section. Profound psychic disturbance can deny its sufferer the resources to be open to us making, with regard to them, the distinctions which are the bread and butter of our socio-psychological interactions with one another. Our concepts give out, hereabouts; our conceptual faculties reach a limit, a limit *of sense*, though not because of a poverty of concepts on our part, nor even on the part of the sufferer. We are not dealing here with a situation like that of understanding an animal, whose concepts are different from and more primitive/'primitive' than ours. Rather, we are dealing here with a systemic *unclarity*. The task of understanding cannot be 'completed', because there is nothing that would count as completing it; even the kind of understanding I am pointing towards in the current Section is strictly '*limited'*. It is limited in roughly the same kind of way as one's understanding of a nonsense-poem faces a hermeneutic limit which there is no such thing as transcending. One might think about it this way: what would it be to 'understand' a nonsense-poem? If one succeeds in 'understanding' it, then hasn't one *ipso facto* failed to understand it? (The logic here is the same explored in 2.2, above).

To return then to Wittgenstein's formulation: Any understanding will be profoundly difficult, to say the very least: How are we to understand somebody's being unable to make the distinction between 'does not', 'cannot' and 'refuses to', distinctions which we rely on as resources and as a matter of routine? What is it to understand the world of a person not subject to these elementary distinctions? Do we even recognise it as a world at all? The world of the unhappy and of the happy are very different, but they are at least both worlds …

---

involves *meeting* the demons of terror, of dread. Whereas Descartes pretends that one can entertain radical doubt without being in the least emotionally disturbed or unsettled. That is just untrue.

The kind of unhappiness involved in the total recession of love (that is the case which Wittgenstein considers) is of a profoundly different kind to that which we are used to trying to understand.[22]

In profound psychopathology, the distinction between *actions* (voluntarily undertaken by a person) and *afflictions* (sufferings undergone) *gives out*.[23] But this is itself a *limit* to our understanding, or at the very least a most severe impediment thereto.[24]

***

This Section has sketched the hazards incumbent upon insufficiently radical attempts to comprehend extreme aversive emotions, attempts that would in one

22 For largely like-minded orientation/understanding in the emotions more generally, see Heidegger's Parmenides, Agamben's (2004), and (especially) Chapter 4 of Hutchinson (2008). On p.146, Hutchinson writes, 'emotional responses to the world are responses of an enculturated being to loci of significance in its meaningful world.' Precisely right; and I am writing about emotions consequent upon a gross loss or excess of significance, or indeed, as I put it, upon a loss of world.

23 Some might argue that MOST human action is actually neither pure act nor pure affliction, and that it is simplifying grammar that forces us to choose and makes us think that things should always be described as one or the other. But I would respond: human *action* is more, or less, free. Then there are some things that we merely/simply undergo. What it means to say that in severe psychopathology this distinction between act and affliction 'gives out', is that there are no longer secure criteria, *for the sufferer themselves or (as a result) for anyone else*, as to whether one is acting more or less freely or merely suffering/undergoing something. Any attribution of act or affliction as the relevant category becomes increasingly impositional, as the degree of severity of pathology increases (for further discussion, see Part II of Read (2007a)). At some risk of over-generalisation, and awfully-quickly, roughly: Is a depressed person – in their depression itself, in an automatic chain of negative thoughts – acting, or afflicted? Hard to say. Is a severely/radically depressed or a schizophrenic person – in their psychotical experiences themselves, in a 'worldless' state – acting, or afflicted? *Impossible* to say. In principle, not only in practice.

24 And this kind of thing is what 'neo-Jamesian' theorists of the emotions, such as Hutchinson reads Paul Griffiths to be, can *in no way* make sense of. Their analysis, best-suited (I submit) to emotions common to both human and non-human animals (although in the end often pretty hopeless and vapid even there, as Hutchinson demonstrates), just has no bearing at all, where our conceptual faculties reach a limit, and so where a quasi-artistic presentation – in which we test our language and our facility with words to the limit – becomes essential. The neo-Jamesians *have nothing to say* about the emotion that fits a true felt '*loss* of objects/world'. For: It is not reflection nor introspection nor even vicious spirals of anxiety nor anything like that that we mean when we say that Jamesians are committed to the inward-facing-ness of emotions. Rather, they believe emotions to be, centrally, *perceptions of sensations*; this is their core committment. 'Cognitivism' in Hutchinson's sense is outward-facing, active, evaluative; Neo-Jamesianism is inward-facing, reactive, perceptual. When *I* talk of inward-facingness, it has a component of *consciousness* that is lacking from the neo-Jamesian account.

way or another assimilate what I am calling dread to 'mere' anxiety, or to fear. I have in particular tried to characterise the (severe) difficulty in comprehending true dread. Perhaps ironically, following the general strategem of Winch (and Kuhn), I have *thereby* (I hope) started to make it easier to understand. Sometimes the best – the only – way of approaching something is indirectly.

That is to say that what unites (to take but three examples, three influential examples for the present work, whose repetition in different contexts I hope enriches) Wittgenstein's remark from *Culture and Value* above, Winch's initial moves with the Azande, and Kuhn's attempts at making defunct science inaccessible, is the following thought: that the first move toward understanding truly the 'alien' (insofar as we can understand it at all) is to emphasise just how distant from us it *is*, and to undo the attractions of 'false friends'. What makes Winch and Kuhn profoundly Wittgensteinian thinkers is their active prevention of the premature rush to 'understand', to assimilate, their teaching of differences, and their emphasis on the provision of new, less expected objects of comparison.[25]

The felt lack of freedom of the mind in the grip of psychopathology, like its analogue in philosophy – here, the pregnant parallel that Sass exploits so effectively – is, I have suggested, yet stranger than what Winch and Kuhn are foregrounding: for the surety of mind, and world, and others, and love, *at least as categories*, is just what is no longer securely present. And thus, there is what I have called dread: being afraid of *everything*, including crucially of 'things' that are not things at all (such as 'sense-data' in themselves);[26] or, *infinite* fear (bearing in mind here Wittgenstein's understanding of the infinite as *utterly* different to the finite[27]); or, utter distress at being unable not to bracket; or, profound *aversion to the state of not being able to distinguish at all between mind and world.* Terror not at this or that, nor even quite at nothing, but rather at [[nothing]], or at ~~nothing~~ ; or at the alienation that this journey inevitably involves.

Not fear without object, but a torturing profoundly-terrifying and sometimes seemingly 'total' *loss of objects*.[28] This is as close as I can see one coming to an

---

25 For detail on what I have in mind here, see Baker's (2004) excellent late work.

26 Being afraid of everything might awfully sound like being afraid *sans object*; but that is why my qualification above is crucial. One is afraid of 'non-things' too; one's entire mode of experiencing the world has shifted, such that it is misleading to talk of 'the world' any more, for fear of reminding us too much of what that term is freighted with, for us. For detail, see for example chapters 8 – 10 of Sass's (1992), on 'phantom concreteness', 'world catastrophe', etc.

27 See my Part III of Read (2007a) for some discussion; I have particularly in mind remarks of Wittgenstein's (1964/1975) such as 'It isn't just impossible 'for us men' to run through the natural numbers one by one; it's *impossible*; it means nothing.' (p.146); and 'Where the nonsense starts is with our habit of thinking of a large number as closer to infinity than a small one' (p.157).

28 Not, that is to say, objectless fear, but a state where even the *issue* of objects (of fear) or their absence is no longer assured, is no longer central. Where having nothing to fear is no longer the main problem, because the environment in which it makes sense for

exposition of what I've called dread. That it may not be close enough tells one, I believe, something deep about the limits of the intelligibility of the quest of psychiatry.

This take on extreme aversive emotion is of such emotion as no longer based on a reading/active-taking of the world, not even just a very perverse one. Yes, all sorts of internal relations are active that shouldn't be so and aren't so for the sane/rational person, and *vice versa*; but, furthermore, that which internal relations *relate* to one – the world, others – has/have been lost.

Concepts come to an end 'somewhere'. One such 'place' (or, rather, set of 'places'), is the non-place – the un-world – that is (are) the hell(s) of loss of objects and others. In attempting to avoid misunderstanding such a non-place(s), one is therefore naturally 'reduced' to reaching for literary-philosophical presentations of the phenomena. One is required, that is, to use terms that deliberately court paradox (e.g. 'non-place'), and to use terms in ways that bump right up against the 'limits' of our customary understanding and use of them. One needs, as I have done – repeatedly – even in this short piece, to go around and around this (most unstable of) landscape(s), sketching, torturing language, in order to try to learn one's way about and to help others to do the same. Indeed, to have a chance

---

one to 'have' *objects* at all is no longer assured, no longer present (Cf. my discussion of 'Renee' in Read (2007a).

A further question naturally raises itself at this point, a difficult question which would take another paper at least to answer, but that it would be remiss of me not at least to mention: what makes the difference between psychopathology and mysticism? Don't some mystics too 'lose the world', but in a way that is not experienced by them as painful, and which does not incapacitate them?

In very brief: I think the essence of the difference lies in *confidence*, and *non-attachment*. I explain the former in my [In preparation]. The latter is examined with dexterity in Mark Epstein's work (especially his 1999). But what Epstein and a number of key Buddhist thinkers (including, crucially, Nagarjuna) have also pointed up is the absolutely crucial point that actually most successful mysticism does NOT in fact involve a loss of world (nor even a loss of self). It involves no *loss;* in a particular and oft-misunderstood sense, it involves finding self, world etc. to be 'empty', but this 'emptiness' is itself empty (rather than being felt as, as I believe it is in psychopathology, the one full thing). It involves only a loss of a fantasy of self, an *affirmative* realisation of the 'emptiness' of self – and an *opening* to the world. Whereas in one way or another, as in my four psychopathological 'scenarios' outlined above, most (though admittedly of course not all) psychopathology involves a kind of 'drowning' in self, and in fact a *substantiality* of self that delivers the *opposite* of what its constructor hopes for. It involves a retreat as if into something substantial, full.

In Buddhist terms, in fact: fear is a form of suffering; anxiety is the predominant form of suffering from that suffering, or at least an extended form of that suffering even when its 'conditioning' object is not present; and what I am calling dread is a kind of suffering felt as if beyond any suffering, because no objects at all are securely present any more (and nor in the ordinary sense is there any longer any security in categories such as 'mind' or 'suffering') and this is felt as a terrifying and unworlding lack, not as an insight nor simply an experience.

of *getting* dread, one has *in a certain sense* to *mimic* the systemic tendencies to delusion or confusion or loss that it involves.[29]

So: I have offered no theory, in this Section. I have offered some terms, some forms of words, some uses that (I hope) may make it at least *easier* to avoid misunderstanding the extreme aversive emotions. And to help one understand what the mind rebels at: their extremity; their (literal) unworldiness; something stranger perhaps than is dreamt of even in Heidegger's or Kierkegaard's philosophies.[30] *The objectlessness of psychotic dread becomes a lack not just of an object but of a world (at all)*. In saying things like this, I have inevitably bumped up repeatedly against the limits of our language: those bumps have been deliberate, and *themselves* offer what insight, if any, my writing here attains to. I have circled around and around – I have offered formulation after formulation, in the course of this Section – in an effort to come up with liberating words, words that do not *merely* bump.

And so then: if I have offered anything positively useful here, by extending and applying Wittgenstein's style of thinking and some gems of his thought, it is this: the initial delineation of a conceptual possibility for the nature and 'understanding' of extreme aversive emotion that has not ever previously been made clear. A more useful verbal object of comparison for dread than has hitherto been offered. An insight into how Wittgensteinian thinking can offer insight into human thinking *in extremis*, an insight greater than that offered by mainstream psychiatry, an insight necessarily bounded by its sense of its own limits and/as of the limits of sense.[31]

***

Before we turn to examine a specific different 'human science' (economics), a final word: about the possible implications of the discussion above for the 'politics' of social/human science, and for reflexive considerations hereabouts. To begin this, here is a long quote from a relevant recent article by Timo Jutten (2010) on Adorno and Kant:

> I can think of two ways in which Adorno might have developed his metacritical analysis into a phenomenology of defective self-conceptions. They examine

29 For detail on what my use of the term 'mimicry' here amounts to, see Part II of Read (2007), especially my discussion there of 'creative mimicry'.

30 Though that (e.g.) both objects and the nothing themselves noth is, as intimated at the opening of this Section, a pretty good place to start in getting 'someplace' in comprehending this unworldiness.

31 Thanks for suggestive ideas and helpful comments on this Section to Alun Davies, Jeff Cumberland, Louis Sass, Laura Cook, Eugen Fischer and (especially) to Anne J. Jacobson. Grateful acknowledgements to Phil Hutchinson for a very fine detailed set of comments on an earlier draft of this Section, comments which I doubt I have done justice to. All remaining flaws are of course in any case mine alone.

> what happens when modern subjects make theoretical reason authoritative over *themselves* or *others*. In the first case I have a form of self-reification in mind, which articulates itself in an attitude that Axel Honneth, following David Finkelstein, has called 'detectivist'. Here, 'the subject is conceived as a detective who possesses privileged knowledge of his own desires and feelings because he has undertaken a search in his own mental world and 'discovered' these desires and feelings'… As Adorno points out, in pathological cases (which are neurotic [and may well be psychotic, as in this Section]) the subject experiences its (sic.) own unfreedom when it cannot identify with its own inner nature. In the second case I have the attitude in mind that we take toward others when we consider them from the standpoint of theoretical reason. As Peter Strawson has pointed out, when we take such an 'objective attitude' toward others or treat them as 'object[s] of social policy', then we evaluate their conduct in terms of causal explanations rather than in terms of freely chosen intentions. Clearly, if theoretical reason becomes authoritative over us in either of these ways, our self-relation and social relations will be severely damaged, and the pathology of this damage will be traceable to the need to dominate nature in the first place. (Jutten, 2010: 26)[32]

Without wanting to commit myself to all the particulars of this diagnosis, I would like to broadly embrace the analysis Jutten makes here. What is I think particularly striking about it, in the broader context of Section 2.3 of this work (and of this work more generally), is this:

The 'first case', of self-reification, reflects some aspects of what has been under discussion here in 2.3 and also in 2.2, above. As Louis Sass has argued at length, there are extensive instances available of such psychical self-reification in the literature on schizophrenia (especially in cases of paranoid schizophrenia; of Laingian 'divided selves'; etc.). In other words, as discussed in this Section: this important form of self-reification is a natural result of some forms of psychosis (and of some forms of neurosis).

While the 'second case', of reification of others, reflects, as Jutten explicates or implies, the mode of 'being in the world' typical of social policy, of social science, of psychology and psychiatry considered as sciences…

---

32 The closing sentence here connects back to the epigraph from Heidegger to this Section, and forward to the final long footnote of my Concluding Summary, below. In the future work indicated there, I will seek to understand the way in which we incline to treat ourselves, others *and nature* as things by seeing them through the lens of science (and of economics, seen as a science). (In Jutten's terms, following and extending Horkheimer and Adorno's *Dialectic of Enlightenment*, this dangerous way of treating ourselves would be called an over-extension of 'theoretical reason' into the domain of 'practical reason', manifesting in reification (Cf. also McGilchrist (2009:333). From my standpoint, of course, there is a standing risk that 'Critical Theory', in its valorisation of social etc. science and of 'theory', itself simply repeats, in the name of denying it, the gesture of such over-extension.)

And *these two cases are two sides of the same coin*: broadly the same attitude, applied to *the self*, and to *others*.

Do you see where this is going? … The very attitude that philosophically-informed reflection can help us to see is of the essence of various psychopathological phenomena, when directed onto oneself, *is* the attitude that social/human/psychological science *recommends* one takes up with regard to others. The 'scientific' study of the mind of the other *mirrors the very pathology that it aims to 'explain'*.[33] Social/human/psychological 'science' is an *expression* of such pathology (And such pathology can only be understood, if at all, (only) from a different basis than that of such 'science': the kind of basis I have endeavoured to examine and to offer some fragments of, in this and the previous Section(s)). To approach the human world predominantly from such a (pathological) perspective is to approach it like an *alien*: like an anthropologist from a Mars that machines, rather than men, are from.

This, if true, is a remarkable and worrying finding.

***

The Section that follows displays, I believe, a classic for-instance of this: that of Milton Friedman's vastly-influential economic methodology.

33 An important for-instance of this can be found in 'Theory of Mind' theory. This theory aims to explain what it is that autists are missing. But in the course of doing so what it actually attributes to normals is the very thing that autists aim and struggle to develop to replace what it is that they are actually missing: an easiness at being-in-the-world, the normal 'affordances' of the world, etc. . It says that normals *have* such a theory: But *that is to make all normals into brilliantly-coping autists*, rather than to characterise adequately the difference between normals and autists. Claiming that we all have a ToM *theory*, that autists lack, and that the grasp of this theory by scientific psychologists is the scientific progress enabling us to see this, is to place *all* of us – normals, autists, *and* psychologists – on the Autism spectrum. To put the point just slightly polemically: 'ToM' is exactly the theory of mind that one would expect *a high-functioning autist,* rather than a properly right-brained normal, to come up with. Its wide acceptance indicates something deep, in my view, about how right McGilchrist is about the extent to which left-brain scientism has overtaken our culture, and also about how deep Wittgenstein's worries about the tendency of 'scientific' thinking to lead us to pathologically over-generalise and reify cut. *ToM is a heightened and subterranean form of the very disease of which it takes itself to be the explanation and solution.*

2.4 Wittgenstein *contra* Friedman

> Positive economics is in principle independent of any particular ethical position or normative judgments … [It] is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences …
>
> Milton Friedman, 1953, The Methodology of Positive Economics.

> DIFFICULTY OF PHILOSOPHY NOT THE INTELLECTUAL DIFFICULTY OF THE SCIENCES, BUT THE DIFFICULTY OF A CHANGE IN ATTITUDE. RESISTANCES OF THE WILL MUST BE OVERCOME.
>
> Ludwig Wittgenstein, opening remark in the chapter entitled 'Philosophy' of *The Big Typescript.*

This Section in effect juxtaposes the very different attitudes expressed by the two epigraphs that I have chosen for it. Friedman wants to model economics on the physical sciences; Wittgenstein by contrast thinks that science offers a misleading model for humanistic work, e.g. for work in philosophy (and I will here add: in economics, much of which, as Winch might have said, is 'misbegotten metaphysics' (or misbegotten maths)). Wittgenstein thinks that we are inclined to *resist* the change in orientation that he is calling for, and that it is targeting this resistance, and not the kind of intellectual puzzle-solving work typical of science, that ought to be our main focus of effort, hereabouts.

Thus this Section further essays and applies Wittgenstein's 'method', and consists of an outline effort to apply a broadly (and at one point a very specifically) Winchian approach to a key test-case, namely a leading 'scientific' approach to what is often thought of as the best of the human or social sciences, so far as its 'scientific' credentials are concerned: namely, of course, economics. (One of our questions then will be similar to the question we have asked, and answered in the negative, in 2.2–3, above, with regard to psychiatry: whether economics is a discipline that can prescind entirely from the character of philosophy, as Wittgenstein and Winch understand philosophy.)

It is over half a century since Milton Friedman (1953) published his 'definitive' methodological paper, *The Methodology of Positive Economics*. That paper is certainly not as widely and uncritically loved within economics as it once was; but on the other hand it has not been superseded, and is still in some places taught as a locale of reliable methodological maxims; most importantly, its influence still widely permeates economics textbooks. Or at least, many mainstream economics textbooks still read *as if* they are structured around the background more or less of Friedman's methodological approach …

Friedman taught Economics as a discipline comprehensible in a broadly instrumentalist or 'positivist' fashion,[1] prescinding entirely from the human status of its actors, and, just as crucially, and perhaps concomitantly, as a discipline that could (allegedly) prescind entirely from normative issues. Like all social science, Friedman's stance implies radically 'leaving' one's society in order to understand it. Over the past generation, the standing of Positivism in economics has been increasingly challenged, but there is reason to believe that it has never really been overcome or advanced upon within mainstream economics.

Let us consider a possible objection to what I have so far suggested: 'Friedman's paper was the definitive statement of economic methodology – of the 'philosophy' of economics – for about 25 years, up until the late 1970s; and it has not been replaced; *but* there has been a seeming gradual shift from the notion of constrained maximisation which conditions the Friedmanian approach to game theory, which has risen as an alternative 'paradigm' to Friedman's.'

Though I cannot argue the point in detail here,[2] my reply would be that the objection here that game theory, and 'rational choice theory' more generally, is actually not in any meaningful sense an alternative 'paradigm' to Friedman's; in *fundamental* respects, the two barely differ, and nor does the recent tendency toward the explicit introduction of psychological ideas into the positivist approach usually make any significant difference. A key reason why is that the 'rational choice' model and the psychological assumptions in question assume as their standard case (from which they allow occasional departures) the same selfish *individual* as is simply assumed in Friedman.[3]

The great advantage of positivism as (formerly) an explicit or (latterly) a perhaps-unconscious ideology in economics is of course that the scientific nature of the discipline is then – seemingly – assured. The longevity, in effect, of Friedman's work is, I believe, a symptom of the continued domination in the social sciences by the very images that Peter Winch questioned half a century ago, and, for that reason, I question the philosophy of the social/human sciences which is both implicit and explicit in Friedman's piece. Winch never explicitly considered Friedman's work, and Wittgenstein died before he had a chance to; my aim here is to do what they did not: namely, to invite the reader to doubt Friedman's influential

---

1 *For my purposes,* in the present context, there is no need to distinguish between positivism and instrumentalism; these closely-related doctrines have much the same flaws, and the same result(s).

2 I have done so in Read (2007b). Where I also make the case that economics is / ought necessarily to be philosophy / philosophical.

3 For anyone who doubts the fundamental idea here, of the way in which the 'agents' in game theory, and in psychological experiments in economics, are reduced to machines, I recommend Mirowski (2002). Compare also ethnomethodology's figure of the 'cultural dope', who bares certain resemblances to the 'robots' of Friedmanian fantasy. (Of course, strictly speaking, the 'cultural dope' would be much more socially incompetent than the stock characters of Friedmanian maximisation.)

claim to have produced a methodology suitable for the explanation of human/social action in the economic 'domain', and to doubt Friedman's grounds for the founding of a positive science of economics.

Once one has read Friedman with a roughly Wittgensteinian sensibility, one can start at last to think about the sense in which the social science (namely: economics) which seems to have the strongest claim to scientificity, so much so that it has at times eschewed even the prefix social, actually *is* a science. Or, indeed, is not. I shall begin by considering in some detail the example around which Friedman *built* his still-influential understanding of economics as a 'positive' science. I quote from the founding example, the seminal moment, in Friedman's paper:[4]

> Let us turn now to [an] example, a constructed one designed to be an analogue of many hypotheses in the social sciences. Consider the density of leaves around a tree. I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbours, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position. (Friedman: 1953: 19)

I will not dwell on the highly-problematic vagueness (and thus vacuity?) of this 'hypothesis', *even if* I were notionally to grant its stunningly unrealistic assumptions; the risibility involved, for instance, in having to think of each leaf as already existing *prior to* its 'choice' as to where to go, the lack of any attention paid to the branches and twigs that support and nourish each leaf. Can the leaves move to any 'desired and unoccupied position'... in the world? Of course not. Nor will I overly concern myself with the glorious ignorance of the actual nature of hypotheses in the physical or biological sciences that Friedman worryingly exhibits.[5]

---

4 It is worth remarking that Friedman's undoubted influence does not equate to the deservedness of that influence, even within his 'school': in a fuller presentation, we should probably focus as much or more on the more solidly, less incoherently-argued (though still, I would claim, ultimately quite worthless) claims of Paul Samuelson as to the scientificity of economics. See Mirowski (1989: 378) for amplification of Samuelson's claim (over Friedman's) to be the real apogee and fount of 'economics as science', in recent times. Samuelson is of course famous as a key *critic* of the paper of Friedman's under discussion here. See, e.g., Samuelson (1963). By my lights, Samuelson's is entirely an 'internal' critique, and ultimately a sympathetic critique. It fails entirely to register the real reasons why Friedman's *paradigm* needs overcoming.

5 Friedman's lunatic idea of what a paradigm example of an assumption in natural science might look like surely obscures the sense in which actual natural scientists arguably *do* insist on realistic assumptions. That is, they will not, in what Kuhn calls 'normal' science (i.e. in virtually all science – See Part 1 of the present work, above), even *countenance* any

Of more interest, for present purposes, is the following feature of this massively influential 'example' for Friedman's subsequent discussion: this 'analogue of many hypotheses in the social sciences' has the singular hidden advantage over (virtually) any actual instance in the natural sciences that human being and social action are 'smuggled in' to a picture in which they should not feature. The leaves precisely behave here as (if they are) conscious beings, and moreover, utility-maximising conscious beings. Thus, *Friedman smuggles into his key example of* '*natural*' *science the very tendentious vision of social science that he will want subsequently to foist upon his readers*[6]*,* but *also* a feature of human/social existence that he will want to ignore (or occlude)!

So Friedman wants us to forget that his assumptions here include, albeit in a debased 'utility-maximising' version, a model of human action/consciousness that the alleged economic laws he will mention or describe to us are supposed to occlude. Friedman's example 'works', and it is hard to think that this could be accidental, because it is presented as a '*natural* science' hypothesis, relying upon an analogical appeal to the human/social world. By a neat piece of symmetrical – though fallacious – reasoning, Friedman can then make it seem natural that *human/social* science 'hypotheses' should work because of an analogical appeal to the *natural* world. That is what he wants: for natural science to be the model for human and social science. *That* is what this example is supposed, by means of a deeply dubious and roundabout rhetorical method, to get for him.

Let us return to Friedman's text:

> Some of the more obvious implications of this hypothesis are clearly consistent with experience: for example, leaves are in general denser on the south than on the north side of trees but, as the hypothesis implies, less so or not at all on the northern slope of a hill or when the south side of the trees are shaded in some other way. Is the hypothesis rendered unacceptable or invalid because, so far as we know, leaves do not 'deliberate' or consciously 'seek', have not been to school and learned the relevant laws of science or the mathematics required to calculate the 'optimum' position, and cannot move from position to position?

---

assumptions which do not fit the actual 'furniture of the universe' that their paradigm allows for them. For detail on this aspect of Kuhn, see Sharrock and Read (2002).

6 It might be objected here that Friedman's conclusions do not depend on his dubious assumptions (or, if you prefer, the dubious lack thereof) concerning human nature and concerning science, but can be derived simply by considering the relevant maximisation problem as an 'engineering' problem. But that would be to beg the question against the approach I am pursuing in the present paper. I claim, more or less following Mirowski, that the analogy between economics and the engineering and physics ideas and models and metaphors it arguably depends on is in fact a dangerously inexact one, such that it is unclear whether we can buy into the idea of 'maximisation' which is central to Friedmanian economics (as it is also to the ordinalism (and in a way also to the marginalism) that Friedman drew together and pithily rendered) *without* buying into a *pseudo*-scientific and/or morally corrupting rendition of human beings as utility-maximisers.

> Clearly (sic.), none of these contradictions of the hypothesis is vitally relevant; the phenomena involved are not within the 'class of phenomena the hypothesis is designed to explain'; the hypothesis does not assert that leaves do these things but only that their density is the same as if they did. Despite the apparent falsity of the 'assumptions' of the hypothesis, it has great plausibility because of the conformity of its implications with observation. (Friedman 1953, 19–20)

The use of the word 'plausibility' in the last sentence quoted here is worthy of note: any reader who finds that Friedman has generated thus far any genuine *plausibility* for his 'hypothesis' is of a very different cast of mind from myself. However, let us grant, for the sake of argument, the following two – dubious and highly 'unrealistic', given what we have seen in Part 1 of the present work – assumptions: that roughly thus is how things go in natural science; and that there is in natural science no need whatsoever for realistic assumptions. Now, if so, what should one conclude about 'human/social science'? What are the 'plausible' parallels between the two?:

> A largely parallel example[:] Consider the problem of predicting the shots made by an expert billiard player. It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles etc., describing the location of the balls, could make lightning calculations form the formulas, and could then make the balls travel in the direction indicated by the formulas. Our confidence in this hypothesis is not based on the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or another they were capable of reaching essentially the same result, they would not in fact be *expert* billiard players. (Friedman: 1953: 21)

Something about the last sentence, I would suggest, generates a specific impression of dubiousness. One perhaps senses that Friedman has stacked the deck, in that his chosen example this time is one in which the laws of physics dictate the answer.[7]

7 Moreover, Friedman has chosen an example in which the action-problem can be precisely described as a computation-problem, which is something that, for example, the Austrian school would (rightly) say is very disanalogous to economic activity – see Mirowski's work for subtle discussion of this point. Hayek, Von Mises et al, in their critiques of the ambitions of 'scientific' economics (e.g. of Socialist 'planned economy' economics, but also of neoclassical economics), do not make the same errors; though arguably they do still assume what Cole (1999:33) terms 'the subjective preference theory of value' – i.e. they take the consumer as the fundamental economic unit, and thereby make (often dangerous and repugnant) political *assumptions*, under the guise of assuming a reasonable and 'neutral' account of human nature.

The 'as if' here is really just a stand-in for something like the following claim: making certain balls go in certain precise directions is a matter of physics.

However, how could that possibly be surprising, or enlightening? Friedman wants to explain human behaviour scientifically, but his example is one that has been chosen in such a way as to beg the question. All he has really described is the motion of certain balls on a flat surface, not any *human behaviour* at all! *Of course* the motion of billiard balls is a matter of physics: in fact, it has been used frequently by philosophers (think of Hume) as a *paradigm* example of such.

Of course, a different way of emphasising Friedman's chosen example here would be more helpful, in getting us some way toward having an account of human behaviour: one might draw attention to the *various* qualities involved in *being* an 'expert' on billiards: for instance, doesn't one for starters have to know *the rules of the game* very well? Doesn't one further have to know many things which are in no rule-book, such as how to strategise putting a big score together, *via* the various allowed moves in the game; and how to outwit or worry one's opponent? But these variegated things are *none* of them things that the laws of physics can tell us (more than) *the slightest thing* about. Recall what Friedman took his problem to be: 'predicting the shots made by an expert billiard player.' Well, in a way, Friedman's 'hypothesis' can tell us something about where the ball is likely to go once the player has begun to take the shot; but it can tell us nothing whatsoever about what shot the player is likely to choose, or why. For that, we would have to learn how to play billiards, the rules of the game, how someone becomes a *good* player, what the particular 'knacks' and tactics of this (as opposed to that) player are, and so on.

In short, if we were ignorant of billiards, then a little human/social study would certainly help us, and the heart of that study would be: really *understanding* the *game* (where *game* is a concept entirely remote from anything susceptible to scientific laws). That is what would start to give us a sense of for instance 'optimum directions of travel' for billiard balls. But then this way of putting Friedman's example would hardly look any more as if it presented us with a piece of human behaviour explained by a kind of quasi-natural science, for the 'explanation' would be an understanding, as from within, of a broadly hermeneutic or 'anthropological' kind. We would be returning, then to the territory best-described by the likes, not (of course) of Friedman, but of (the likes of) Peter Winch and of ethnographers (not to mention expert billiards players, commentators, etc.).

Friedman moves directly from his (failed) 'account' of human behaviour to an 'account' of social behaviour:

> It is only a short step from these examples to the economic hypothesis that under a wide range of circumstances individual firms behave *as if* they were seeking rationally to maximise their expected returns, and had full knowledge of the data needed to succeed in this attempt; *as if,* that is, they knew the relevant cost and demand functions, calculated marginal cost and marginal revenue from all

> actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal. (Friedman, 1953: 21)

As if, that is, these firms knew 'the laws of economics'. But, if my line of reasoning above is convincing, one has gained *no* understanding of this situation yet from the Friedman's 'analogies' with the leaves on the tree, or the billiard player.

What is rather striking about the new explicitly 'economic' examples Friedman mentions is that the assumptions he makes are perhaps not so unrealistic after all. Is Friedman's hypothesis here a *relatively* good one, not because he has *somehow* (how?) found a hypothesis that theorises the domain in question, albeit an allegedly unrealistic one, but rather because, with a little translation and a little less intellection, businesses actually *are* operated in roughly the way in which Friedman mentions here? Although if they are, this may well be largely as a result of firms (limited companies) having a *fiduciary duty* (to their share-holders) to do so. A quintessentially *human* (social, legal) matter …[8]

My point here is a direct analogue of Winch's important point using the example of 'liquidity preference', in the most crucial section (3:6) of *ISS*. In fact, it is almost as though Winch here were writing in response to Friedman, only without mentioning his name:

> [L]iquidity preference is a technical concept of economics: it is not generally used by business men in the conduct of their affairs but by the economist who wishes to explain the nature and consequences of certain kinds of business behaviour. But it is logically tied to concepts which do enter into business activity, for its use by the economist presupposes his understanding of what it is to conduct a business, which in turn involves an understanding of such business concepts as money, profit, cost, risk, etc. . It is only the relation between his account and these concepts which makes it an account of economic activity as opposed, say, to a piece of theology. (Winch, *ISS*: 89)

To return, thus armed, to the Friedman quote above: Aren't Friedman's assumptions in this, genuinely social-human case, *unlike* in his earlier examples, *not* entirely unrealistic? Aren't there, just as Winch suggests, people in most firms who actually do engage in activities that could very roughly be described as seeking rationally to maximise expected returns, even trying to establish demand functions,[9] calculating marginal cost, *via* accounting procedures that have been designed precisely to work out how much it will actually cost them to produce

8 See below, for more on this.

9 Perhaps via market research (which is itself surely more an art than a science, at least insofar as it does not fall into the same kinds of mythic errors as we are endeavouring to expose in Friedman).

some more of their product at the margin[10] and so forth? And, insofar as these assumptions are, indeed, realistic, is that not in part a *product* of a certain kind of (capitalist) society, and of the workers' and managers' *decisions*, albeit very much under constraint (under constraint of needing to eat, under economic constraint, under legal constraint, sometimes under physical coercion from security guards or police or goons, and so on), to take up certain roles in that society? And indeed of many of these agents knowingly or unknowingly having learnt at school or university or filtered through into the business world some economics, including (more than likely) 'positive economics'? [11] Strikingly, there just is *no* serious analogue to the latter kinds of effect in the domain of the natural sciences.[12]

We should note how Friedman himself, contrariwise, understands the analogy he hopes to have generated here: he believes himself to have shown that economics can be and is a positive science because it can treat human beings as if they engaged, to an utterly unrealistic degree, in a kind of *thinking*, but in a kind of thinking which, ironically, leads to no *thought*, but merely to obeying laws as if of nature, which leads to acting in an algorithmically determinable manner, and to algorithmically obeying 'the laws of economics'.[13] Once again: he has given us

---

10 Crucially relevant again here is Winch's short discussion of 'liquidity preference', in Chapter 3 section 6 of ISS. And this in turn could be filled out by means of reference to ethnomethodological studies of business and accounting. Cf. also Flyvberg's succinct critique of economics as a would-be 'second-order social science', on p.44 of his (2001).

11 One could also appeal to broadly Hayekian considerations here, to make part of the same point: There is a sort of Spencerian learning 'mechanism' at work here: the market system as a whole is capable of making 'information-processing decisions' which are not necessarily based on any particular piece of information possessed by any particular agent. As my scare-quotes imply, however, there are dangers attendant on this way of presenting the point, dangers which are sadly beyond the scope of the present paper. See Section 2.5, below and McGilchrist (2009: 429), for more critical analysis of the concept of 'information'.

12 This critically important point is the main subject matter of Ian Hacking's works of the last 20 years or so. See for instance Hacking (1999); though compare also the important corrections to Hacking offered by Sharrock and Leudar (2003). The broadly 'Hackingian' point which I am making is that the way in which economics involves 'feedback loops' that include the *consciousness* of the economic *agents* has no direct analogue in natural science, including in (non-human) evolutionary biology. The extent to which people have in their actions understood as non-reflex actions an understanding-in-action of economics is explored, via the concept of (what is misleadingly/derogatorily-termed) 'ersatz' economics, in, for instance, Garrett Jr (1999). For a roughly Hackingian view specifically applied to economics, see also George Soros's work.

13 There is a behaviourism or functionalism thus lurking here – behind the veneer of the 'as if' of hyper-thinking that Friedman begins with – that would be worthy of much further investigation, and that again is representative of most Economics of modern times. It is unsurprising that positivism here tends to yield something like behaviourism: the latter is *par excellence* a (pseudo-)'science' of the human, reducing the qualitative complexity of humanity away to preferences which can be cashed out further as stimulus-response pairs.

no reason whatsoever to assume that there are any such. He has simply assumed that there are.[14] So much is lost in the process: the account of behaviour in firms etc. that Friedman goes on to give just writes out so much from human and social being.[15]

A good way to approach the above point is *via* the following remarks of Friedman's:

> Confidence in the maximisation of returns hypothesis is justified by evidence in part similar to that adduced on behalf of the billiard-player hypothesis – unless the behaviour of businessmen in some way or another approximated behaviour consistent with the maximisation of returns, it seems unlikely that they would remain in business for long. (Friedman, 1953: 22)

This is indeed so, *if* such behaviour is really what is needed to stay *in business*, as opposed to in economists' *a priori* models of what 'business' is. Friedman's argument here is, we are now seeing, circular; or, alternatively put, he is simply begging the question he was supposed to be addressing.

Friedman has as yet, in other words, given us no evidence at all: he has simply reasserted his belief in the laws of economics as a useful mode of accounting for what goes on in 'the economic world'. We can *look and see* whether he is right or not. For example, I have heard some interesting and suggestive anecdata that some firms (e.g. in the brewery and pub industry) may well maximise size and turnover rather than profits. Of this possibility, which would have its own human determinants (e.g. desire for power among managers or bureaucrats), there is no glimpse, in Friedman.

But perhaps there is also *another* place to start in looking at what goes on in that world, in our world: namely, looking at what people actually understand themselves to be doing, and can *account* for. (When that gives out, *then* it may be worthwhile to engage in the kind of demystification exercises that the great economists such as Marx, Sraffa and Keynes accomplished.[16]) In other words, rather than assuming that we know what business is, and then theorising about it

---

14 To see how economics reached this sorry state, this low point, the reader is advised to study Mirowski's work. The history defies brief summary.

15 Compare here Douglas Dowd's (2004: 84–6) powerful attack on the early Stiglitz's (and by extension, of course, on the whole of mainstream modern economics) rendition of consumers as rational and self-interested, and of firms as rational and profit-maximising, (and, most ludicrous of all, of markets as competitive with price-taking behaviour).

16 For my reading of Marx as a demystifying philosophical and political economist, through and through, see Read (2002b). See the essays in that edited collection by David Andrews and colleagues for a similarly Wittgensteinian reading of Sraffa. See also Coates (1996) for related discussion of Sraffa, Keynes and Wittgenstein. The most striking such instance of 'demystificatory' anti-delusional thinking in Keynes's (philosophical) economics is probably his famous and vital challenge to the temptation to think of the economy of a nation by analogy with the economy of a household.

using questionable assumptions, why not look and see what people who actually engage in business in practice *understand themselves* to be doing?[17] Aren't they experts *in* their activity in a way that leaves are not (!), and that billiard players *qua* physical systems are not, while billiard players *qua* self-conscious players of games are? Then, building *via* the more or less *realistic* assumptions that *we* have thus been able to generate, we could come to understand *via* our understanding of these agents just what they are doing, and what the activity they are engaging in together, 'business', amounts to. We might (then) learn many things that conventional economics has shielded from sight.

Insofar as economic actors seem deluded about what they are doing, the task is one of de-seduction, of getting them to come to see themselves and their fellow buyers and sellers etc as *people* too. In other words, the task is inevitably to some degree political and ethical and involves persuasion. One way of putting what this task of demystification is would be to say: it is the task of getting capital-owners to be readier than they already are to ignore the social 'fact' that they are capital-owners, and to act in a way other than so as to maximise their accumulated profits. Historical materialism, in part through its scientism, but in part through a cold and calm realism, would certainly have it that this task of persuasion is going to be a fantastically difficult one. It may be as much a task of revolution. *But it is founded in the reality (occluded by Friedmanian neoclassical economics) that business is already at least sometimes like this.* Think of some 'ethical businessses' for instance; or think of Robert Owen.[18]

---

17 And one *might* be surprised then to find out, for example, the extent to which taking care of one's family or taking pleasure from building good relationships with customers or charity-work were important, even constitutive, elements of what business actually is. Perhaps what is taken to be 'business' often involves these things. Or perhaps it often involves the sheer coercion found in organised crime. Friedman's 'model' is not seriously open to either possibility. A fuller investigation here would of course be an 'empirical' and (preferably) ethnomethodological one, seeking to see how businesspeople understand what they are doing as is visible in their 'account-able' action, not just in their accounts of their actions. For some pointers in this direction, see Sharrock and Anderson (1989).

18 If the reader suspects that there is no plausible academic analysis that could underlie the bifurcation from neoclassicism implied in this parenthetical invitation and in the paragraph that follows, then I invite that reader to read Nelson (2006). Nelson examines in some detail the kind of possibility that I mean to be making space for in this Section: e.g. the possibility that even the fiduciary duty to maximise profits is a kind of empty place-holder: for a plausible case can be made for a wide variety of business-strategems to involve long-term profit-maximisation. Such strategems could include: building a strong reputation as ethical; and/or engaging in public charitable giving; and/or treating one's employees well; etc. (My point here echoes the argument I give in my forthcoming (2011b), in which I claim that there is no limit to the level of inequality which Rawlsians can seek to justify with reference to the economic well-being of the worst-off. Similarly here: Profit-maximisation does not guide the behaviour of firms except within the framework of making quintessentially human decisions about what stratagem, what path of profit-

One could easily go much further here. There is *much* in business that does not necessarily prioritise profit-maximisation: e.g. employee safety proceedures, maintenance of employment, non-maximally-publicised charity support, supplier and customer loyality, maximisation of *turnover*, power struggles between owners and managers, exercises of power by workers, etc. . To get a grip on most of this would require, for example, detailed studies of economic history and aggregated ethnographic studies of businesses at work (as practiced by ethnomethodologists etc fairly extensively, over the last generation.). In short: all this would suggest a way of turning traditional economics on its head. Rather than a fundamentally scientific exercise, constructed analogically on the model of the theoretical natural sciences,[19] it would become first an exercise in studying quasi-anthropologically (humanistically, historically) the methods of the people in question,[20] and then (as in Marx, Sraffa or Keynes at their best) would involve a demystificatory exercise in questioning persistent social illusions and delusions, undertaken from a point of view;[21] and then an activism, a (philosophically-influenced) 'therapeutic' praxis.

The problem I am raising with Friedman's views seems to me particularly important for the following reason: according to Friedman, we have to predict the behaviour of economic agents whilst accepting as given the 'game' in which they are involved. But this game has, potentially, many pernicious features: it is for instance based on the possibility of making a profit out of bets on the future values of stock options (as happens with some financial derivatives), i.e. in a way that is independent of the material productivity of firms and the level of wealth of a community. This state of affairs should not be accepted merely as a given constellation of facts but should be criticised: for this reason it seems to me necessary to introduce an element of understanding in and of economics, as a pre-condition of critique, but *also* to make such critique an inevitable consequence of such understanding. The latter, Friedman fails to do *by definition.* For what I am saying now might be put as strongly as this: that a 'positive', purely explanatory

---

maximisation one is going to attempt to pursue, within an open-ended range of possibilities. Such human decision-making is glaringly absent from what Friedmanian dogma constricts one's attention to.).

19 And note that it just doesn't mean anything to seek for realistic assumptions *in the sense* in which we are seeking for them here, in *natural* science (See the Lecture Transcripts which open the present work, above; and see Lecture 4, on where Friedman's monetarism starts to go wrong, in this connection.). We can get clearer on the reality of the social world by observing it as actual or possible actors in it, by asking people what they are doing, by seeing people's actions as account-able, etc. . A botanist or biologist who spent their time literally asking leaves what they are doing would by contrast soon cease to be regarded as a natural scientist at all, and might indeed – and understandably – be a candidate rather for the lunatic asylum.

20 The methods of the ethnos; thus '*ethnomethodology*,' the more or less Wittgensteinian, non-scientistic version of sociology founded by Harold Garfinkel.

21 I take the term 'point of view' from Gavin Kitching, and his impressive effort to overcome scientism and the fantasy of value-neutrality in Marxism.

economics is either a kind of absurdity, or a kind of abomination (or both). In other words, the understanding of the practices in which we are involved is necessary to their critique, which may be urgent and essential: but Friedman's framework is *inherently* indifferent to the possibility of such critique.[22]

To sum up this critical dissection of Friedman's worryingly influential text: It is striking that Friedman's would-be birthing of a science proceeds in a superlatively pseudo-scientific and rhetorically-loaded fashion. Friedman smuggles human being into his founding exemplars for economics as a 'positive science', and hopes we will forget that he has done so. He wants people to be both humans and not humans at the same time: he *hovers* [23] unstably between *eliminating* humanity altogether from the 'objects' of his economics and *relying* on such humanity.[24] This is quintessentially the kind of fatal failing that a Wittgensteinian sensibility unconceals. (It is a telling sign of a dire need for 'therapy', for intellectual liberation.)

Crucially, his (Friedman's) 'as-ifs' skate over the way in which these 'as-ifs' are thankfully (sometimes/often, at least) entirely eliminable in actual social studies: we can come to *understand* why people do things principally because

---

22 Thanks to Davide Rizza for thoughts that have greatly influenced me on this point. It might be claimed that this line of thinking may not undermine positive economics but only clarifies its scope: positive economics may still be the study of a distinctive (widespread) type of economy, even though the decision to 'plan' in a certain way and accept the crystallisation of a type of market is something that should be subjected to further reflection and critique. But these points fundamentally undermine Friedman's ambition just the same: they mean that positive economics as an alleged value-free science of universal relevance is dead and buried. Winch's approach seems to me helpful given that it can point in this direction: see for instance the discussion of liquidity preference etc., above. In more general terms, as established early in 2.1 above: Winch makes clear the inevitable inter-involvement of 'social and economic science' with philosophy, and philosophy inevitably brings with it the potential of criticism/critique.

23 Hereabouts, Friedman is fatally vulnerable to Wittgensteinian challenge. Some of economics is just 'applied maths'; no problem there, no worries from me about that activity. But when one actually *applies* the theory to the real world, then one has to eliminate one's desire to hover, to fail to choose what to mean by one's words.

Insofar as economics then offers us anything determinate (non-hovering, non-flickering), one can usefully see economics as offering us, in Wittgenstein's terms, *objects of comparison*. We can learn about the world and about ourselves by comparing and contrasting notions such as the utility-maximising individual or the 'game' (as in game theory) with our existing concepts, and (in a way) with reality. But such 'objects of comparison' leave *you* to do the thinking, and to do the work. The work of seeing by means of them, and of reflecting on the value or otherwise of what one has thus seen. The illusion fostered by Friedman is that the theory can do the work *for* one.

24 I alluded earlier to the probable vacuity or nonsensicality of Friedman's famous 'leaves' example even on its own terms; now we see that Friedman's 'as if's' transposed to the human/social science contexts where he wants to put them to work certainly make less sense still … These 'as if's' cannot be stably cashed out (to coin a phrase).

we (can) inhabit, vicariously or sometimes literally, their place in the social situation in question. Realistic assumptions, *even if* as inessential to the natural sciences as Friedman suggests, *are available* and invaluable in the social studies: it is an intellectual error of vast proportion to eschew them; it is a turning of *the world* on its head. For realistic assumptions are in fact *essential* in 'the human sciences' in a way that they are not, in the natural sciences: where, as we saw in Part 1 of this book, one has to be ready sometimes to countenance the most madly 'unrealistic' of assumptions, at moments of crisis. Think of the near-craziness, on first-inspection, of Relativity, or of the quantum. Assumptions in science, we might say, are realistic *relative to a paradigm*. That is: they must eventually come to *seem* realistic, though such seeming is paradigm-relative, 'world'-relative. (Of this, there is no hint in Friedman).

Friedman's suggestion that there is no reason to even try to adopt realistic assumptions in the social studies amounts to a refusal to acknowledge that society is made up of aware and (sometimes unpredictably) responsive human beings, human beings with more or less intimate and indeed 'internal' relations with each other. And a refusal to acknowledge the difference that makes. These human beings are not mere atoms or leaves: they are in an important sense *part* of one another. This has ethical *and* methodological/predictive consequences. This (return to) common-sense which social and economic theory obscures is a fundamental insight of Winch's. The endless 'paradigm' of everyday life, the way that social reality is *fundamentally* natural to us, is the alpha and the omega for social study.

Thus, what Friedman's suggestion amounts to *begs the question* against the 'therapeutic' philosophical claim that I have by contrast suggested: namely, that there is a kind of difference in kind between the subject-matter of the human/social 'sciences' and the natural sciences. In the end (and, in fact, throughout) Friedman has simply *assumed* that economics is a science, in the same way that physics or biology are. He has not provided the slightest *reason* to believe that this is the best way to look at economics.[25] This positivist stance simply leaves out so much; and, further, tends to militate implicitly/illicitly in favour of the right-wing

25 It will be objected that the reason Friedman has given us is his claim that economic theories should be judged on the basis of their ability to make predictions. (See the transcript of Lecture 4 at the opening of this book) and Read (2010b)). Friedmanian monetarism proved lousy in this regard, for reasons *internally related* to Friedman's *scientism*. Monetarism was blind to the way in which human beings as reflexive creatures who understand when they are being constrained and react against it are constitutively ill-suited to being scientifically understood and predicted. Monetarism *could not remain a 'scientific' positive economics* and still anticipate the extent to which economic actors would deliberately resist and *use* monetarist policy for their own economic ends.

It is worth adding that my discussion above, e.g. of Friedman's example of the leaves on the tree, already undermines the apparently reasonable pragmatism or instrumentalism involved in Friedman's position. It is not only the case that Friedman's theories yield lousy predictions, for reasons indissoluble from his scientism; it is the case also that there is either vacuity or absurdity in the very idea of judging science only on the basis of its successful

political philosophy that Friedman espouses, according to which the maximisation of wealth by individuals is rational, natural and unobjectionable.

The very purveying of this picture of humans as *Homo Economicae* [26] can tend to mold societies in such a way that the picture will come to appear more and more accurate, and the positive economics which elaborates it more and more well-founded.[27] The final irony of considering the complete travesty that is Friedman's 'scientific' founding of economics is that there is no way internal to Friedman's implicit philosophy of 'social science' by which to understand this effect. This effect is probably the most important effect of all of Friedman's essay. But the 'feedback loop' in operation here is one that a Friedmanian approach renders invisible. Thus *Friedmanianism blinds one to the very effect of Friedmanianism.*

Friedman's essay over time tended to make society look (and be) more like what his essay 'described' than it had done previously – but the very nature of this effect is one which is incomprehensible, on Friedman's *own* terms. Thus the influence of Friedman's essay made it look superficially as though what he was saying in the essay was correct – but the *way* that that influence came about provides in fact a final refutation of the vision of economy and society and of 'social/economic science' promulgated by him, and questioned throughout this book of mine. (Friedmanian thinking produces the very disease of which it takes itself to be the diagnosis …)

To overcome such scientism as is found in Friedman's essay requires, as Wittgenstein urged, an effort of *will*, a change of *attitude*. That is ultimately what I aim to be fostering in the present work (and that I explore the nature of in more detail, in the latter portions of 2.5, below).

I invite the reader in this context to reflect upon the extent to which economics, insofar as it remains more or less Friedmanian, could possibly genuinely attain to scientific status. I suggest that that extent has been much exaggerated. Meanwhile, Friedman's influence on the real world (*via* his methodology) is then inevitably a baleful one. Let us close by giving one powerful contemporary for-instance: the (broadly-Friedmanian) 'naturalisation' of selfishness and maximisational

---

predictions. A science without reasonable assumptions does not make specifiable testable predictions *at all,* except by begging the question.

26 This picture is at the heart of the troubling 'social theory' that economics tends to embody, project, or argue for. But from my – Wittgensteinian and Winchian – point of view, the very *wish* for a social theory – a *theory* to *explain* (the nature of) society – is confused. Economics as social theory is *not* science, but rather is philosophy – but, mostly, the wrong kind of philosophy. Philosophy as metaphysics of what needs none, as opposed to philosophy as liberating us to return to what we always already understand, especially after the intervention where needed of therapeutic philosophical undelusion: ourselves, as social and linguistic actors who *make* history, who *do* society.

27 It is worth comparing here the intriguing point that students of economics tend to behave more selfishly during game theorists' 'economic experiments' than do other undergraduate students … It is also worth comparing here Friedman's subsequent work, such as of course his deeply dubious and often morally repugnant *Capitalism and Freedom.*

behaviour is a root of the financial/economic crisis that we are currently experiencing. His methodology of positive economics encourages one to prescind from making realistic assumptions about how human beings behave: it makes it harder to engage in and take 100% seriously the kind of realism present in the thought of Hyman Minsky, for example, and his warnings about bubbles and about the multiplication of risk.

In getting one to think of fundamental economic concepts such as money as if they were concepts of natural science, Friedman helps one to lose sight of their fundamental *difference* from such concepts. As I remarked in the 'InterSection', above, money is as money does: regarding it as a kind of stuff leads one into absurd fantasies of 'monetarism'.[28] Moreover, imagining that one can securitise any'thing' and every'thing', and that uncertainty can be calculated into risk that can be calculated into money which can be relied upon, is a natural thing to do if one is working with the metaphor that money is a kind of stuff, that economics is a kind of physics, and that human beings and their accountable, reflexive etc. capacities are inessential to economics.

These are Friedmanian assumptions/metaphors. They bear some responsibility for the economic and financial disaster that we are living through. Some good policy-advice for a more genuinely *secure* economic future would in my view therefore be: Teach trainee-economists what is wrong with Friedman on economic methodology, and give critical philosophers of economics (including Wittgensteinian/Winchian voices) some counter-balancing power to the positivists, who have the events of 2007 to the present day as a dreadful legacy of their philosophy.

Friedman's followers should admit that Friedman's methodology for economics, in its lack of self-reflexivity, and in its getting systemically in the way of thinking the real human nature of money, uncertainty and bubbles, is one of the foundational reasons that we have been threatened with such a Depression as now may well be looming over the world. That is the 'cash-value' of the methodological matter that has been under discussion in the present Section.

28 On this, see again Read (2009b).

2.5 'Dissolving' the Hard Problem of Consciousness Back into Ordinary Life

> 'A man's thinking goes on within his consciousness in a seclusion in comparison with which any physical seclusion is an exhibition to public view.' // If there were people who always read the silent internal discourse of others – say by observing the larynx – would they too be inclined to use the picture of complete seclusion?
>
> Ludwig Wittgenstein, *PI* Part II, section xi.

I have sought in this book to suggest reasons to hesitate before thinking of 'social' or 'human' or 'economic' etc. 'sciences' as actually kinds of … science, and as actually having enough genuinely in common with our paradigm(s) of science to be unmisleadingly assimilated to it/them. Have I in the course of so doing depended on some dubious quasi-Cartesian assumption about mind, and especially the 'hardcore' of mind, consciousness, being a different kind of *stuff* from the stuff that real science investigates?

I very much hope not. This, the final Section of Part 2, aims to take up a satisfactory Wittgensteinian 'stance' on (or 'style' of engagement with) that most recalcitrant of 'phenomena', consciousness. That stance/style will not instantiate a solution, no matter of what kind to 'the problem of consciousness'. It will rather seek to dissolve the problem: but *not* through (what would conventionally be likely to be called) an argument, but (rather) through indicating some actual practices that one can engage in (and does engage in). In the course of so doing, further light will be cast upon the way in which 'the human sciences' are different from the natural sciences, *without* having to rely on any quasi-Cartesian move; *quite to the contrary*, in fact, as we shall see.

I suggest here reasons to believe that consciousness as a thing or object is not the right place to look, for what makes the difference between 'human sciences' and natural sciences that has occupied me increasingly as this book has proceeded. Thinking that it is *tacitly preserves a Cartesian framing of the issue,* and fails to achieve freedom with regard to the metaphors that we instinctively grasp for when trying to think the human, the mind, the 'subject', and so on. That is the importance of this section for the present work: that it resists a perhaps-natural – but in-fact-dangerous – assumption about where to locate the distinctiveness of (investigations of) the human.

Rather, as the ethnomethodologists insist, what are central are accountability, reflexivity, and (the understanding of) *action*, (of) conduct. The 'behaviourial and

cognitive sciences' are crucially modes of understanding activity, conduct. And it is, it 'goes without saying', understanders who are being understood.[1]

I argue that the interminable analyses that are made of 'the problem of consciousness' can perhaps be terminated – though only if we are willing to understand (and thus facilitate our being able to overcome) the misleading (or even vacuous) metaphors upon which the problem is largely based. Let me explain why I think this, and why exactly I think that all 'human scientists', and certainly not only philosophers, should care.

What is 'the Hard Problem' of Consciousness?

The 'hard' problem of consciousness is (allegedly) a 'sub-problem' of the broader mind-body problem. It is the alleged kernel of that problem. The 'hard' problem, as Chalmers has called it, is that of alleged 'qualia' the 'qualities of felt or sensed experience.'[2] It is the problem of why, besides 'intentional states' (e.g. understanding something about something in the world) and 'informational processes' (e.g. the sub-personal processes which allegedly constitute perception etc.), there should be actual conscious events and feelings. No; not so much *why* there should be these things, but *how there can be* these things – what it can be for there to be such things, and, crucially, how, if at all, their interconnection with 'the physical' can be understood. Someone like Colin McGinn seeks to shock by saying: It can't be understood at all (by us). This novel position in the debate is called 'transcendental naturalism'. But nevertheless McGinn et al keep the mind-body problem alive – it is almost as though they want to guarantee that it will never die – by putting it tantalisingly out of reach of our epistemic/cognitive powers. As Wittgenstein once remarked, people like to state what the limits of thought and

1 In a 'social science' (or 'social theory' or 'human science' etc.) that does seek to understand human action; but not all so-called social science does aim to do that. So what about, e.g., analyses that look at striking juxtapositions of such things as income inequality and inequalities of health and illness, etc.? These 'findings' are not directly about action; indeed the point of them is that what they (seem to) show is that there are effects of socially-distributed phenomena that happen fairly independently of the particular meaningful things that people do in their everyday lives, and largely outside of their awareness. With regard to these alleged findings consciousness is not particularly relevant, one way or the other, i.e. is largely not under dispute. This then is an important 'exception' to the general hostility I have exhibited in this book to social and human 'science': that certain kinds of 'policy studies' and the like are harmless (and indeed very useful) and do not fall within the ambit of the critique I offer herein. I have taken this for granted in the present work, having explicitly discussed it and argued for it elsewhere, e.g. in Hutchinson, Read and Sharrock (2008: 37, n.16).

2 For scholarly chapter and verse, see Owen Flanagan's (1992) impressive work.

knowledge are – because in so doing, they are thereby somehow seeming to start to see beyond them.[3]

It has been fairly *convincingly* argued by those dissatisfied with the classical cognitive-science paradigm that there is a very impoverished conception of the physical, and in particular of the human body (when viewed as broadly mechanical), at play here; that is, in the likes of Chalmers, but also in the behaviourists and the eliminativists, and perhaps in McGinn too; indeed, in virtually the entire Modern tradition of thought about mind and body. Merleau-Ponty has probably shown this better than anyone else.[4] It has also been repeatedly (and in my view efficaciously) argued that there is something badly awry with the conception of 'mind' and 'body' as (conceptually) separable in the first place: this has been argued, for example, by Wittgenstein.[5] Furthermore, what Hegel, Pragmatism, Merleau-Ponty, Wittgensteinians and Philosophical Feminism have contributed to – and what the Ethnomethodological sociologists have developed in the greatest detail – is the understanding of the *social* aspects of mind/body; but it would take us too far afield to go into that question here.[6]. One's view on the mind-body problem is surely deserving of real consideration only if it does not depend upon wrong-headed conceptions of 'mind' and 'body'. Add to this, that arguably it is only if one thoroughly re-thinks the philosophical tradition concerning mind and body that one will be able to escape from the 'dialectic' between – on the one hand – dualistic Cartesianism, and – on the other – its reactive 'opponents' (e.g.

---

3 See Wittgenstein (1998: 15). Thus my thought, that 'transcendental naturalism' is neither a solution to the problem of a consciousness, nor a way of getting rid of the problem – rather, it guarantees in a particularly powerful manner *the keeping alive indefinitely of the problem*. It offers the extraordinary psychological satisfaction of *both* a humble (yet privileged) 'scientific' statement of limits to the understanding, *and* the knowingness of being part of a privileged elite that, in stating those limits, can see beyond them. It fails to understand what Wittgenstein made clear in the Preface to the *Tractatus* that 'The limit can…only be drawn in language and what lies on the other side of the limit will be simply nonsense.'

4 For useful discussion of Merleau-Ponty's approach, see for instance Soori and Gill (1989).

5 And it is worth noting that the mistake I am alleging in the bulk of the philosophical tradition, apart from these alternative and non-canonical thinkers, is not ubiquitous among reflective *scientists.* See for instance some of the writings of renowned gerontologist and neuroscientist Raymond Tallis; for example, 'A Critical Dictionary of Neuro-Mythology' and 'The Poverty of NeuroPhilosophy' in Tallis (1999). Somewhat troublingly, Tallis seems not really to want to dissolve the mind-body problem at all (see e.g. pp.73, 89, 101, 123), and thus leaves the quasi-Dualistic impression that consciousness is a 'thing'. However, he is right, I think, in his deep suspicions of the claim that blurring the boundary between human and machine is any way forward in our understanding of the mind.

6 See Williams (1999); Button *et al* (1995); Watson's (1998), especially pp. 202–223; much of Anne J. Jacobson's work, and Glendinning (1998).

neo-behaviourists and eliminativists, who have not escaped from *the space defined by* Cartesianism).

I wish to develop a kind of synthesis of these related thoughts, of the genuinely and therapeutically anti-Cartesian arguments of Wittgenstein and co. For I think that the appeal of the mind-body problem as an interminable enigma will not be depleted unless and until one has looked carefully into the way in which the conception of mind and body as fully conceptually separable is first arrived at, and then noticed the peculiar and impoverished conceptions of *both* mind [7] and body – of *people* – which tend to structure the separation, and thus the debate.

Specifically, in the contemporary context in which I have initially framed my own discussion, in the context of philosophers like McGinn, Searle, Chalmers and the Churchlands, I think it is very useful to take a close look ('for example') at the way in which the computer 'model' of the mind has held virtually *all* of us captive, has metaphorically structured (deformed) the discussion such that it is almost impossible to get a clear view of the very peculiar nature of the question that is being asked, when 'the hard problem' of consciousness is raised.[8] But a clear view is what I am after: I think that, if and when one attains such a view, *the mind-body problem* as *a felt problem really can evaporate completely away.*

Without *depending* in my argument overly upon you already trusting in Wittgenstein and the other critics of the Cartesian tradition, I shall endeavour to persuade you, the reader, of this. (In doing so, I follow (I hope) in the footsteps of Lakoff and Johnson (1999), in their questioning of the computational metaphors of mind.)

'Information'

What do I have in mind? What I have in mind can be conveniently focused around the concept of *information.* This concept is absolutely crucial for 'Cognitive Science', in part because it funds the computer 'models' of mind which in one way or another is shared by philosophers of mind as otherwise diverse as Fodor and the Churchlands.[9]

---

7 To indicate a little more of what I mean here: Heidegger and Foucault are two of the philosophers who can help one to escape from a *thing-like* conception of mind, as of course can 'Wittgensteinians' such as Kenny and Ryle. For amplification of this thought, and of how it is crucial for sociology, see Coulter (1979), especially pp.1, 34 and Chapter 2.

8 In other words: The kind of solution that various philosophers impressed with or implicitly depending on or even deliberately working in explicit opposition to the computer metaphor for mind have in mind to the mind-body problem *already depends* upon non-compulsory presumptions about a separation between mind and mere body, and a concomitant impoverishment of the understanding of the human person. Those presumptions are *structured into* the metaphor.

9 For a background critique of these problematic models, see not only Lakoff and Johnson's (1999) excoriating critique of computationalism and other '1st generation

Now, the concept of 'information' is a contested one within contemporary philosophy of mind and cognitive science. There are debates, for example, between those who are suspicious about the general usefulness of the notion of 'information' (but keen to treat the mind as entirely computational in nature), and those who think that plants, non-human animals and human alike are all above all receivers of and actors upon information. But such debates are very much debates *within* the traditional 'Cognitive-Science paradigm' in philosophy of mind – a paradigm about which I am meaning to raise deep doubts.

Sometimes, more fundamental questions are raised about the use of the term 'information' to refer to sub-personal processes. I think that these latter questions *are* very pertinent, and that there is an almost inexorable risk in extending the use of concepts such as 'information' beyond their original domain of application. The use of the concept has been extended – 'bloated', we might polemically say – when it is applied as Dretske *et al* apply it. They therefore owe us an account of why it is wise to continue using the same term – 'information' – at all, if what they are talking about is so loosely connected with what the word normally, unmisleadingly, means.[10]

The original context of the concept is in situations such as the following: One is at a railway station, and asks a stranger such as the station manager, 'I need the following information, please: Could you tell me where I can find a decent hotel?' Or: one is impressed by the knowledge of a fellow theatre-goer, and remarks to a friend, 'That woman is a mine of information.' Or: one defends one's controversial choice of reading matter on aesthetic grounds by emailing one's friend to say, 'I don't read her books merely to acquire *information*!' Of course, we can also acquire information (and indeed knowledge) from sources other than our fellow human beings. Examination of fossil remains yields information concerning the nature of life on Earth hundreds of millions of years before there were any human beings.[11]

Cognitive Science' but also McGilchrist's (2009), especially the third chapter. Genuinely post '1st generation' Cog Sci, and Neuro-Science, are not within my target-range for criticism.

10 It might be objected (thanks to Davide Rizza, for this thought) that writers who talk about 'information', in Cognitive Science, etc. are not using the everyday notion: rather, they are 'referring' for instance to Shannon's information theory, which has originally been developed for different purposes (e.g. the transmission of electrical signals). But this objection doesn't take into account the wideness of my point hereabouts: Even to talk about electrical *signals* is to place oneself in a relation to our ordinary concepts of communication, information, etc. . Shannon's and cognate theories are called theories of *'information' because* of this relation. Moreover, the problem of transferring, even purely analogically, that notion of information to the brain or something more elusive still stands, regardless of this point.

11 It is striking that these usages of the term 'information' that I am drawn to imagine all concern, unsurprisingly, requests for/statements about matters of fact, simple fillings-in of specific small gaps in people's knowledge. In McGilchristian terms: 'information'

But even in this last case, 'information' remains, paradigmatically, a concept which clearly operates on a personal and inter-personal level, not on a sub-personal level. In other words, 'information' is paradigmatically a concept of something which *human beings* have at their fingertips, and there is a family of uses of the term which we *do* clearly understand. It may be 'messy' from some arbitrary and (purportedly) 'scientific' point of view, but such alleged 'messiness' or 'vagueness' is not normally any constraint whatsoever upon comprehensibility or utility. The utility of such uses of the term 'information' and cognate concepts is located at many and various points in the stream of daily life.[12]

The 'messiness' of such uses is only an *issue* for philosophers/theorists who are working from inside the 'analytic' attitude, rather than having any kind of 'practical' attitude toward their subject-matter. Careful attention to the actual use of terms such as 'information' (and indeed 'messy'),[13] to their actual contexts of significant use, can enable one to see ordinary uses of the term as rich and sophisticated, rather than as flawed or merely *sub-scientific.*

This point feeds in, once again, to the founding impulse of Peter Winch's Wittgensteinian philosophy of social science, equally applicable to most 'Social Theory' today, namely that 'scientific' versions of human practices tend to provide impoverished or dangerously ironising accounts of ordinary actors' linguistic and

---

is quintessentially a left-brain concept, a province of the left hemisphere. This should already give us a big clue as to why any 'information'-based approach to the mind and to consciousness is bound to prove inadequate: because it will miss out what are actually the most crucial aspects of the human brain, the parts least amenable to 'modelling' via computers etc.: the wholistic, and metaphorical, and novelty-spotting and –creating, and anomaly-spotting (and open to life rather than just to machines and bodies) ways of the right brain. The urge to acquire information is a urge to make the new old, an urge born out of knowingness, not a genuine desire for newness that is willing to tolerate an inability to categorise or to pin down (which would be the right brain's way: McGilchrist (2009: 164)). 'Informationalism' presupposes the possibility of ramming everything into existing categories, and has no space for (Kuhnian) revolution.

12 This turn of phrase is intended to allude both to Cora Diamond's (1989) magnificent discussion of the actual location of concepts (e.g. 'rule') which even 'Wittgensteinian' philosophers can be guilty of misusing, and to the kind of approach to the actual use of terms often found in Conversation Analysis and Ethnomethodology. It is worth noting, to forestall some possible understandings of my points about 'technical' concepts such as 'Information' in mind-body debates, that 'Wittgensteinians' such as Diamond, Cavell, Conant, Kuhn, Garfinkel and Lynch are clear in their work that for most purposes scientific and technical language as actually used should be considered *to be perfectly everyday.* The point is that sometimes terms 'go on holiday': When, that is, they are 'employed' metaphysically, as I am alleging they often are in philosophy/'cognitive science'.

13 See *PI,* 65–80, for an extraordinarily penetrating dissection of our prejudices surrounding concepts such as 'vague' and 'messy'. And see *PI* section 356 for discussion of the actual use of the term 'information'.

non-linguistic actions, unless they remain tethered at all key points to those actors' own accounts and concepts.[14]

Under pressure from the scientific impulse, however, and from what John McDowell has sometimes called 'the drive toward objectivity' in Modern thought,[15] it may appear as though this concept of 'information' were hopelessly wishy-washy, and that some 'hard' and pure and clear replacement for it were required. The term gets used in extended ways, in particular scientific or philosophical theoretical discourses, and these uses are 'not vague'; they are clearly laid out and demarcated. The theorists *define* their terms. So, for instance, there is then talk of animals or even plants receiving information from their environment, or of computers processing information. And such talk can seem clearer, easier to put in a box, than the very uses of the term 'information' from which that talk metaphorically derives.

It then starts to appear as though we understand what information is *better* in the new extended use than in the original context. We understand clearly, so it then can seem, what we mean when we talk of 'information' in the context of 'information-processers', and we can seemingly employ the new, putatively 'clearer' use of the term to retroactively reflect upon how humans work. Specifically, we perhaps feel that we can start to figure out how 'the mind' works, and how mind can be embodied, as 'software', for example. Is this *really* how to make progress on the topic of the mind-body relation?

Strictly Philosophical Approaches that Guarantee Failure to Connect Mind and Body

The as-yet-insoluble mind-body problem has seemingly been narrowed with the passage of time, with the development and intensification of the debate in recent and contemporary philosophy of mind etc.; but with each narrowing, a solution of the real hardcore, the 'hard' problem, seems to get further and further away. This again seems to me to explain the attractiveness, the *attraction* of, for example, McGinn's take on the problem; he, unlike nearly all predecessors, at least have something to say – albeit something I think is very confused – about the deep elusiveness of a solution.[16] What I am suggesting is that the elusiveness of a

14 See especially the 2nd edition Preface of *ISS*, and Winch's (1964: 83–90) 'defence' of the Azande against Evans-Pritchard's claim that they are clearly committed to contradictory beliefs.

15 An obsessional drive which, incidentally, McDowell thinks afflicts many 'anti-realists' (e.g. Rorty, and sociological relativists) just as much as it affects many 'hard-nosed' 'realists'. See Mcdowell (1989).

16 A useful way of seeing the terrain hereabouts may therefore be this: Mainstream approaches to the mind-body problem are almost invariably scientistic, assuming that the answer to the problem will one day be positively statable as some (e.g.) physical

solution which will really satisfy us, which will let us end our philosophising, is in fact *a structural feature of the way the problem has been approached.*[17] There is in common between virtually every single one of the protagonists to the debate the assumption, covert and/or overt, that the way to make progress in and with the problem of consciousness is to hive off from the problem more and more things which purportedly 'we can now explain', *via* neuro-science, or cognitive psychology, or A.I., or composites of these, or social scientific spin-offs from them (most obviously, some sociobiology/'evolutionary psychology'). More and more bits of the problem get (supposedly) rendered answerable, as we 'make

---

or biological proposition, which philosophers are at this point only able to sketch (and thus we get cognitivist sketches, materialist sketches, behaviourist sketches, etc.). (My formulation here is strongly influenced by the work of James Conant). Against such quasi-positivistic optimism, minority voices (e.g. some religious thinkers) sometimes counsel an 'ineffabilism', to the effect that we should not expect ever to be able to state the answer to the problem, and that the human mind/soul will always retain an element of mystery. The McGinn and co. view is a novel attempt at virtually-explicit synthesis of these two apparently-conflicting positions: McGinn says that the 'scientistic' view is wrong only in thinking that *we humans* will be able to succeed in stating the answer to the problem! Thus McGinn wraps in secular 'scientific naturalist' robes the transcendentalism and ineffabilistic 'mysterianism' of those who have traditionally denied that we will succeed in solving the mind-body problem. If McGinn were living in a different age, he would cut to the chase and simply say quite seriously: '*God* knows the answer to the mind-body problem, but *we* never shall.' (We can see how close he is – except for some perhaps-superficial features of his 'clothing' – to Descartes's late Medieval/early Modern worldview, when we see how close he is to saying, 'We have a Clear Idea of *Mind*, and we (scientists and philosophers) perhaps have a Clear Idea of *Body*, but no Clear Idea whatsoever is available (to humans) of their Union. That Union is a mystery to mortals such as ourselves.' (For some complications hereabouts, pushing McGinn still deeper into 'mysterianism' (where Nagel is waiting for him) see McGinn (1995)). My alternative approach is to interrogate and problematise the problem much more deeply than 'transcendentalism' does, and to seek to return us to everyday wholistic perspectives on persons, perspectives which (quite naturally) therapeutically eliminate our sense that there is a genuine problem (or 'mystery') here at all. This method should, if successful, enable one to transcend the 'positivistic/scientistic' *vs.* 'ineffabilistic' opposition, and attain a rigorously therapeutic and elucidatory conception of philosophy, as involving not the solving but only the dissolving of 'problems', in part on an *individual* basis. If one were going to risk entertaining Descartes-style 'Clear Ideas' talk at all, it would be best then to say something like this: That it's to *humans* that we have a 'Clear Idea' of what it means to ascribe 'information processing' powers; and thus our only 'Clear Idea' of information is *tied* to our notions of consciousness. We can have, roughly, a 'Clear Idea' of consciousness, if only we overcome the tendencies which abound in philosophy toward obscuring the wholeness of human minds/bodies/beings.

17 There is a connection here between my point and that of both Coulter and Watson in their writings referred to above. Just as methodological ironism and reductionism have been argued by them to be necessarily flawed methods – methods guaranteeing an endless fruitless succession of theories – in 'cognitive' or 'social' psychology, so I am suggesting that 'divisionism' and theory-driven philosophy of mind will have the same result.

progress' with the 'discrete' problems of intentionality, of information-receipt and information-communication, of perception, and so on. We strip these things away from the 'hardcore', and explain them in a manner which does not differ in kind from how we scientifically explain all sorts of other natural phenomena. We are left with a hardcore that more and more evades our scientific schemas. So we redouble our efforts, and so it goes on.

Now I am not denying that this divisionistic [18] 'stripping away' of bits of the mind-body problem for the purposes of genuinely scientific investigation can ever be productive. For example, neurophysiologists have of course learnt some interesting things. For example, we now understand the etiology of epilectic seizures, at least in brute causal/correlational terms, better than we used to. But we don't understand the causality (etiology) of such things better because we now know what 'information' really is, or because of anything remotely like this. We understand them better because of new technologies and careful observation – because of real science and medicine, not because of philosophising. The 'divisionist' and 'stripping away' stratagem, I have suggested, is in the end philosophically/methodologically disastrous. The problem of consciousness, as opposed to the problem of which bits of the brain are correlated more and less with what actions and events (which is a perfectly respectable scientific/medical problem), *will not* be illuminated by that stratagem. In fact – worse – as indicated above, the stratagem moves in precisely the *wrong* direction. It keeps pushing the hard problem forever *beyond our ken*. It can't re-bootstrap us into a better understanding: on the contrary.

In a remarkable and almost *faux*-Hegelian fashion, then, we can see how cognitivist 'optimists' concerning the mind-body problem and McGinnian 'pessimists' are made for each other. The latter lead the former forever onward, and into greater and greater aping of science – precisely by means of denying that the problem is fully soluble. 'Optimists' redouble their efforts, on hearing how the mind of God (i.e. what is at the heart of the human mind) is supposedly off-limits. What a great prize it would be, against impossible odds, scientifically to uncover the mind of God (i.e. the nature of the mindedness of 'Man')!

If the 'optimists' are ever to be weaned off their search, a different approach will be needed. One which suggests not, as the 'pessimists' suggest, that consciousness is a scientisable or theorisable object, only one that will forever be beyond the grasp of our science. Rather, an approach that, roughly, questions whether consciousness is that (*or any kind of* '*object*' *or* '*thing*') in the first place.

18 This term, 'divisionism', is my own invention. Recently, I learnt from McGilchrist (2009: 137) the following intriguing point: 'If one had to characterize the left hemisphere by reference to one governing principle it would be that of division.'

Sociological Approaches that Guarantee Failure to Connect Mind and Body

An approach to 'the mind-body problem' such as I have argued is actually needed might be sought in sociology. But I have already intimated that I think one is liable and likely to be disappointed in this domain, too. With certain very specific exceptions (especially, much ethnomethodology, 'Wittgensteinian' sociology), 'positions' in sociology tends to be structured around the very same stale debates – voluntarism versus constraint, structure versus agency, mind versus world – which, albeit sometimes under different names, make up the playing field, the 'legitimate' spectrum of debate, in mainstream Philosophy.[19]

I will just take one recent influential example, in order to sketch how this can go, how sociology can guarantee failure to bring mind and body into any proper alignment. (I intend this example to be illustrative, not of course exhaustive. If you are not interested in it, or already convinced that sociological theory is unlikely to offer a positive way forward here, then skip forward to the next sub-heading.)

Campbell (1996) is an attempt to rebut the 'social situationalist' doctrines he sees in the likes of Garfinkel, Goffman and Giddens alike. Campbell wishes to reinstate what he sees as Weberian 'action theory' as the basis of sociological explanation, rather than taking all action to be social action, as he thinks that those he criticises do. Campbell hopes to understand action as springing from mind (as opposed to society); he hopes to understand sociology as effectively underpinned by (or a branch of) psychology; and he seems to think that psychology will have full-scale scientific legitimacy and explanatory power, either on its own account or possibly by virtue of being tied into more basic sciences.[20] In short, he thinks that the mind-body problem can be solved by means of properly placing sociology in a causal-interpretive physiological-psychological context. He is thus precisely the kind of person who vests his hope in full-blown 'human science'.

Campbell does not exactly say himself what I have just said of him. But many of his presumptions and turns of phrase are pretty stark evidence for the extent to which he has at root just such a (broadly 'Cartesian') approach in mind, and is assuming throughout his book the legitimacy of the anti-Wittgensteinian idea that 'private language' is coherent and existent, that motives are simply 'internal states',[21] and so on:

---

19 See Sharrock (1989: 670) for detail on this point *vis-à-vis* disciplinary sociology and ethnomethodology's lack of 'fit' with it.

20 In Chapters 4 to 8 of his book especially, Campbell repeatedly attacks 'social situationalists for claiming to settle 'empirical' matters by reference to philosophy (especially Wittgensteinian and 'ordinary language' philosophy). He thinks that theirs is an unscientific and *a prioristic* approach. But this will inevitably appear, to the Garfinkelian or Wittgensteinian, simply to be begging the question against their account – which is not intended to be settling genuinely empirical questions but clarifying conceptual and grammatical ones.

21 It is worth noting that Campbell utterly mischaracterises and garbles Peter Winch's account of motive and reason, on page 73. This is perhaps a piece of supporting evidence for my suggestion that Campbell is so much a mentalist that he cannot even understand

> [S]trictly speaking, 'meaning' cannot be 'located' anywhere except in minds (Campbell, 1996: 45).
>
> The real problem which needs to be addressed is not so much whether actor and observer attach similar or different meanings to a given action; it is whether there are any grounds for believing that observers can *ever* know what constitutes the actions of individuals unless they take the trouble to ask them. (Campbell, 1996: 50, emphasis added)
>
> The basic truth is that our understanding of the actions of others is grounded in our own experiences of agency. (Campbell, 1996: 78)
>
> [O]nly actors can possibly know what their action consists of. But then that should not be surprising because *all action is ultimately performed alone,* undertaken by the individual as the sole agent. .This is because all true actions are the outcome of an 'act of will', a covert and personal event which actors can perform only for themselves...It is *essentially personal,* intra-subjectively created meanings which are the immediate and direct *'causes'* of actions. (Campbell, 1996: 162, the book's closing paragraph, emphases added)

I will not work through these remarkable passages. The previous and following portions of this Section (and of this book) should suffice to make clear my deep doubts and worries about them. A variant of the views of those (many) philosophers which I have been wanting to question in this Section can in fact be seen *particularly baldly* and starkly in Campbell's work. He has apparently been utterly unimpressed with Wittgensteinian 'anti-private-language arguments', is every bit as much a methodological/ontological individualist as most mainstream philosophers, and every bit as much a card-carrying mentalist as (say) Chalmers.

Campbell cannot even succeed in *conceiving* options besides the options a theoreticist Cartesianism leaves open to one. (Thus he cannot understand the possibility of the kind of alternative approach I have been intimating that we need with regard to these matters, and that I detail below.) This confusion and narrowness of vision on his part can be appreciated clearly by the simple device of juxtaposing two passages from his peroration. Campbell (1996: 151) writes that the 'social situationalism' which he opposes, because of its (supposedly) privileging the vantage-point of the social observer over that of the actor, 'has a strong behaviourist flavour'. Less than two pages later, he remarks that '[t]his stress on the individual as an observer rather than an actor also helps to account for the marked cognitive bias which is such a feature of situationalism.' Attributing

---

the possibility of not having a 'position' within the conventional space of 'Cartesian' options (Dualism, Physicalist Materialism, Behaviourism, etc.) as to how to account for the 'existence' of mind.

both behaviourism and cognitivism to someone is obviously absurd. Why does Campbell drift into such absurdities?

Let me indicate why. Campbell (1996: 151) suggests that according to the so-called 'social situationalist' paradigm, 'human actors typically possess neither mind nor body. The individual human being, who as a living, thinking actor was once at the very centre of the sociological stage, has been completely dissolved away, stripped of both mind and body, now no more than a ghost in the social machine'. Attributing this obviously absurd and nonsensical position to his opponents is only forced upon Campbell because he simply does not see *any possibility* other than taking up a theoretical stance that essentialises human beings into either mind and/or body, as these concepts are set out according to basically Cartesian parameters. He thinks he is defending and simultaneously just developing a project of scientifically-explaining consciousness; he is actually just developing a quasi-Cartesian/scientistic parody of it.

Before leaving Campbell's book, it is worth noting finally that he does not wish to be thought of as a 'causalist' or 'empiricist', but as an 'interpretivist', after the fashion of Weber. This just goes to show how very compatible 'interpretivism' can be with over-intellectualisation of the issue here, with what I have called 'Theoryism', with a conventional stance on the problem of consciousness, etc. Moreover, Campbell sees Goffmann and Giddens as (anti-Cartesian) opponents.[22] But from the point of view of someone impressed by Garfinkel or Wittgenstein, this would only go to show *how very deep and wide* the quasi-Cognitivist Cartesianism in sociology runs. For Goffmanian dramaturgy, and Giddensian (or even Geertzian) neo-interpretivism, are arguably – unless interpreted very charitably – simply ironies and (over-)intellectualisations of life.

Let us delay no longer. What exactly is the alternative approach to consciousness, to mind and body – an alternative to the approaches of Chalmers, Chomsky, the Churchlands and Campbell alike – which I have in mind?

An Alternative Approach

We can generalise now from what we observed above in specific connection with the concept of 'information'. If we really want satisfaction, if we want to get anywhere in relation to the philosophical problem that Descartes gave us, we should not and will not *strip away* more and more of the human in order to try to understand the human, to understand consciousness (We will not for example break off and 'operationalise' the problem of information). The hardcore becomes *more*

22 See Campbell (1996: 16, 49). Campbell makes some good criticisms of Goffman and Giddens, as of Veblen, Merton and Goldthorpe, on the grounds, roughly, of over-intellectualism in their understandings of mind and action – but these criticisms should in my view be heard as counting *against* Cartesianism and Theoryism, not *for* Campbell's hardline variant of it!

and more remote, the more we de-humanise aspects of mind such as information and perception and intentionality. The scientisation of consciousness if not just uncompletable – it is counter-productive. To get out of this unhappy dialectic, we need a *different* approach.

The problem will only really be being faced if we face up to it as a 'problem' that has to do with whole human beings, embodied, in a context (inextricably natural and social), at a given time, etc. . But once we really manage to succeed in seeing the 'problem' in that way there *is* no longer a problem. When one watches a graceful dancer, for example – or when one dances less or more gracefully – there is no mind-body problem. When one has an attitude toward another which involves seeing them as (say) dancing mindfully, being in the dance mind-and-body, or when one hears someone (as) speaking mindfully – when one simply allows oneself to see a 'body-self" in action – there is no philosophical problem. If we give up the scientistic-mechanical view of the body, and perhaps say along with Aristotle that the soul is the form of the body – and along with Wittgenstein that the best picture of the human soul is the human body – then the 'problem' vanishes. The metaphors of mind that, especially since Descartes, Lakoff and Johnson show have sorely gripped us, suddenly and dramatically lose their grip.

So, to return to what has been my main example: when one thinks of the way the word 'information' is actually used in its 'home', outside certain rarefied academic contexts – when, again,[23] one thinks of requesting some information from a librarian or from a ticket-seller, for example [24] – then it can become perspicuous to one that there is no problem. Only when one starts, say, to try to 'theorise' information across human and non-human domains (supposedly using the non-human – the animal (usually in turn thought of as mechanical) or the machine – as one's paradigm, and thus getting things back to front) does there start to look as if there is a problem, as if there is a residuum which, however good our 'theory' of information gets, will not be accountable for by such a theory. This elusive residuum is the 'hardcore' which I am saying that the traditional and dominant approaches to the problem – *all* the mainstream options, all the

---

23 The reader may by this point have become irritated by the (several) ways in which I repeat myself. Why do I do so? Because I am looking for what Wittgenstein called 'the liberating word'. I am looking for the way of putting these things which is *just right*. Not *correct,* but just right to dissolve a strongly-felt impulse to illusion. (Of course, there is not just one way of so getting things 'just right', for these things are partly personal. Thus any philosophic paper/chapter is in the end a second-best substitute for a conversation.)

24 These are, notably, contexts in which there is a clear link – normally, an interleaving – between information and *action*. The divorce of 'information' from action, almost inevitable if one dwells on computers as paradigms of 'information-processing', is one of the implicit – and disastrous – moves typically made by those who like to think of themselves as helping to solve bits of the mind-body problem. (On this, see the entry under 'Information (Processing)' in Tallis (1999).

I urge the unconvinced reader at least to stop at this point and *think through* one of these scenarios that I have given again in some detail.

'isms' ('Cognitivism', Reductionism (to the brain), Behaviourism, and so on) that one is offered in a traditional undergraduate 'philosophy of mind' or 'theoretical psychology' class – push *further and further* from our reach.[25]

One gradually loses all sense of such a residuum, however, if one takes the alternative track, a track toward a clear view, which I am endeavouring to point up. Human beings, body-and-soul, do not of themselves lend themselves to a mind *vs.* body split. Only, ironically, does the effort to explain human beings in apparently well-founded scientific terms (e.g. in the terms of 'information-processing'; meanwhile ignoring that there is a non-problematic core set of uses of the concept of 'information', attention to which actually would help us dissolve the sense that there is a problem here!). The perhaps-uncomfortable but I think desperately important general result of my diagnosis is now clear: *The mind-body debate is the very disease of which it takes itself to be the cure.* The very debate, the very effort to find the 'solution' to (the various 'fragments' or 'sub-problems' of) the problem, *the very conceptualisation of the problem,* is the very thing which ensures that 'the hard problem (of consciousness)' remains insoluble, and which perpetuates the existence of the problem. Thus philosophers of mind are driven to highly-desperate attempts at cure – the McGinn approach at one extreme, supernaturalistic dualism at another extreme, and Eliminativism, or Behaviourism, at yet others. The metaphors of mind have trapped us, absolutely: until we suddenly break their glass palace wide open, escaping the 'fly-bottle' that 'contained' us.

If one looks though to take the approach which I am recommending, one suddenly or gradually loses a sense of there being what McGinn calls a 'mystery', here. That *doesn't* mean that suddenly all human life is flat and dully obvious – far from it. One can actually *see human life,* at least rather clearer than before, if anything (including, sometimes: in its mystery). One can see the beauty of the dancer and the dance, the beauty of that unity, without perhaps being misled by reductionist questions about the mind and/versus the matter of the dancer.[26] No; what one loses is *the sense that there is a philosophical problem here that demands or begs an explanatory solution.* (Rather, there are metaphors that are no longer compulsory. Freed from their compulsion, with alternatives available, the whole debate that one was inside suddenly comes to appear peculiar, narrow, almost petulant.)

---

25 Perhaps it is now still clearer how being a behaviourist or a materialist is not really being 'anti-Cartesian' at all. These supposedly anti-dualist approaches have not reconceptualised 'mind' and 'body', and thus they retain the imprint of Cartesianism, like prints made from a negative, because they have not given up (divisionistically etc.) theorising. They have not returned to our everyday talk and life, in which there is a 'hardcore' of inextricable unity of mind and body. Instead, they keep the concepts of 'mind' and 'body' intact, and merely reduce the former to the latter. This is an entirely ineffective way of challenging the standard Cognitive-Science paradigm.

26 One can see it; one can see the person, mind-and-body: for nothing is hidden. See PI 435, Dupré (1998) and Shotter (1998).

As I have just suggested, one way of losing that sense is to notice that, paradoxically, it is the very demand for a solution to the problem that *guarantees* that one will never rid oneself of the sense that there is a problem here. The insistence that the mind-body problem is a problem, however much that leads one toward 'a partial solution', is the very act that ensures that one will never reach a satisfactory full solution, and ensures that the 'hard' problem will keep on receding out of reach.

One intriguing consequence of starting to come to terms with the 'alternative approach' that I am offering is this: consciousness 'itself' no longer seems quite so important. Under the microscope of 'divisionist', Cog.Sci., 'informationalist' etc. approaches, consciousness comes to seem like *a thing* that is always elusively slipping out of reach. But this quasi-reificatory attitude is a 'left-brain'-style attitude that is bound to miss the *difference* of consciousness and its *wholistic inter-relatedness* – that I have just been stressing – with life as a whole. Explicit conscious knowledge *tends to be itself a left-brain phenomenon* (cf. McGilchrist, 1999: 164): the dance of the whole brain, the whole body, the whole person in a real context, generally with others, through-and-through embodied in a world, is something else, something more. Thus we start at last to get a proper sense of consciousness not as the be-all and end-all, but as *one* irreducible, crucial aspect *of a greater whole*. It is this whole – a whole brain (not just the left hemisphere), a whole person, in a real social and (ultimately) ecological and cosmological context – that we need to bring into view, and that an unhealthy post-Cartesian obsession with consciousness *occludes.*

It is not me, the defender of the distinctiveness of the human, but the 'scientific' mainstream, it turns out, that is obsessed with consciousness, drawn to it as a thing that endlessly fascinates because it endlessly defies objectification. Only once we start to cease tacitly to reify, and 'dissolve' consciousness back into the liquid where it has its home – the stream of life, the life of the mind of a person engaged in action along with others, etc. – can we escape this obsession.

***

Some readers will as yet be unconvinced by my claims. In a persuasive effort, I am going to call upon Wittgenstein, in the *Philosophical Investigation.,* to speak for me – or rather, to open up a dialogical space. I am thinking now not just of the debates Wittgenstein stages in the anti-private-language considerations 'proper' (243–308; see also the epigraph to this Section), though those are of course highly relevant, but in the subsequent passages on the phenomenology of philosophical illusion; for example, in, on consciousness:

> The feeling of an unbridgeable gulf between consciouness and brain-process: how does it come about that this does not come into the considerations of our ordinary life? This idea of a difference in kind is accompanied by slight

> giddiness,–which occurs when we are performing a logical sleight-of-hand.[27] When does this feeling occur in the present case? It is when I, for example, turn my attention in a particular way on to my own consciousness, and, astonished, say to myself: THIS is supposed to be produced by a process in the brain!– as it were clutching my forehead.–But what can it mean to speak of 'turning my attention on to my own consciousness'? This is surely the queerest thing there could be! It was a particular act of gazing that I called doing this. I stared fixedly in front of me– but *not* at any particular point or object. My eyes were wide open, the brows not contracted (as they mostly are when I am interested in a particular object. No such interest preceded this gazing). My glance was 'vacant'; or again *like* that of someone admiring the illumination of the sky and drinking in the light.
>
> 'Now bear in mind that the proposition which I uttered as a paradox (THIS is produced by a brain-process!) has nothing paradoxical about it. I could have said it in the course of an experiment whose purpose was to show that an effect of light which I see is produced by stimulation of a particular part of the brain.–But I did not utter the sentence in the surroundings in which it would have had an everyday and non-paradoxical sense.' (*PI*, 412)[28]

It is important to be clear about what this passage does *not* do. It does not, of course, decisively dissolve the problem of 'qualia'. It does not, of course, try to prove preemptively that what the likes of Colin McGinn say about qualia (or indeed about passages such as this) is mistaken. Wittgenstein *is not interested in* trying to do these (impossible) things. Logico-philosophical insight, as Lewis Carroll understood clearly, in his famous parable of the tortoise and Achilles, is not like that. What it is like is coming no longer to be *compelled* by a picture or an obsession. What Wittgenstein gives one is tools which may enable one to overcome one's previous *insistence* that (e.g.) there is something weird and in need of explanation, about consciousness. He offers no more – and no less – than this.

This important passage from Wittgenstein seems to me then to encapsulate in a condensed form much of what I have argued (if that is the right word) above. Wittgenstein is trying to get us to consider the thought that it is not consciousness that is weird, a weird 'thing', but only our attitude towards ourselves that is (sometimes) weird, especially when we are either psychologically disturbed or doing philosophy (either explicitly, or in the implicit form of social or psychological theory). Then, we are inclined to forget just how specific are the circumstances in

---

27 In the ellipsis, there occurs the following, in parentheses: 'The same giddiness attacks us when we think of certain theorems in set theory.' For the parallel case there, see Read (2002).

28 I have emended the translation very slightly. Cf. also *PI* 339, and the brilliant discussion in McGilchrist's (2009: 20) of how in 'how' terms (rather than in 'what' terms) Descartes fails to separate mind and body *enough*.

which ideas of consciousness are actually invoked or mentioned, in everyday life. And, just as important, that consciousness itself is normally (arguably, always) itself intentional – i.e. that consciousness is consciousness *of* something. 'He was suddenly cognizant of the peril that he faced', or 'I am conscious of your capabilities in this field, Ms. Greenfield', or 'Right now, I am all too aware of the depth of the wound, doctor'; these are perhaps typical occasions for uses of the concept of 'consciousness'.

By contrast, philosophers frequently envisage no context at all for their invocations of the concept; this is very problematic. Wittgenstein was very aware of the specificity, practicality and intentionality of attributions of consciousness – outside of the even more specific and often downright peculiar or untethered context (or non-context) of a philosophy discussion, as the following dialogue evinces:

> 'Human beings agree in saying that they see, hear, feel and so on. So they are their own witnesses that they have consciouness.' – But how strange this is! Whom do I really inform, if I say 'I have consciousness'? What is the purpose of saying this to myself, and how can another person understand me? – Now, expressions like 'I see', 'I hear', 'I am conscious' really have their uses. I tell a doctor 'Now I am hearing with this ear again', or I tell someone who believes I am in a faint 'I am conscious again', and so on.
>
> But can't I imagine that the people around me are automata, lack consciousness, even though they behave in the same way as usual? [J]ust try to keep hold of this idea in the midst of your ordinary intercourse with others, in the street, say! Say to yourself, for example: 'The children over there are mere automata; all their liveliness is mere automatism.' And you will either find these words becoming quite meaningless; or you will produce in yourself some kind of uncanny feeling, or something of that sort. (*PI* 417, 420) [29]

Wittgenstein looks seriously at what the conditions are which alone make the problem of consciousness, 'the hard problem', compelling to one. He suggests that it is only the special situation that philosophers put themselves in (or perhaps that some of the mentally-troubled genuinely find or put themselves in) that makes them inclined to see consciousness as a *thing* at all, and that makes them wonder at 'it'

29 For a fascinating and compelling account of what happens if one actually does attempt to take dis-belief in others minds etc. seriously, see Sass (1994). Many schizophrenics, according to Sass, suffer from a real-life version of the typical philosopher's complaint of (to use Cavell's terms) being unable to *acknowledge* the world (and, especially, other people), and to insist rather on *knowing* it. In *PI* 420, this is what Wittgenstein is talking about – the acknowledgement of others *as* others, as 'souls'. For a fairly sympathetic exposition and critique of Sass, see Read (2001a); see also section 2.2, above.

being 'connected' to matter.[30] For from what perspective could it seem – does it (sometimes) seem – as though there is something weird about consciousness? From a perspective in which one's 'interiority' is foregrounded, and in which the material world is seen as, if anything, then a brute 'external' Other to that interiority. (When, for example, one stares into space while under the influence of a drug; or perhaps after a heavy bout of reading Descartes.) *But there is nothing absolute or privileged about this 'perspective',* about this peculiar state of mind [31] which philosophers may be especially prone to entertain. There is no such thing as a perspective from which it can be absolutely asserted that some or other aspect of the way the Universe is made up – e.g. that we are conscious; or that we talk; or that there is matter and energy, etc. etc. – is odd.[32]

Suppose it were said: 'The perspective from which it genuinely is odd that there is consciousness is the scientific – or more broadly, the 'objective' – perspective.' I suspect that the perspective from which *this* is being said is not '*the* scientific' perspective – some actual scientists, such as perhaps Raymond Tallis and Gerald Edelman and Iain McGilchrist, do not seem to find consciousness aberrant; that is, do not find it crying out for heavy scientific theorisation or replacement. I suspect that the perspective in question is broadly that of the 'unity of science' movement and its contemporary successors (that is, chiefly a philosophical and social scientific tendency) imbued with broadly reductionist prejudices. That is, only if one thinks that there must be a science of anything that really exists, and that such sciences must in some sense be unified or reducible to (a) fundamental science(s) (physics is the usual candidate), will one find consciousness alarming. But even among mainstream Anglo-American philosophers, reductionist and unity-of-science views are far less respectable than they used to be. Dupré has I think fairly decisively shown that such views are untenable, in part for reasons that can be appreciated simply by looking carefully at the actual practice of the natural sciences.[33]

---

30 This suggestion is also pivotal to the argument of Division I of Heidegger's *Being and Time*; and to the work of Cade-Hetherington (e.g., 1992).

31 See the quote from *PI* 412 above.

32 If anything is odd, 'mysterious', my own intuition runs somewhat differently. I rarely find the fact of consciousness odd any more; the thing that I tend to find 'odd' is that there is *any* thing – any such 'thing' as (and in) existence – at all. Not how the world is, but that it is at all, sometimes surprises me – as Wittgenstein: see (*TL-P* 6.44–6.52). See also Sass (1992), for detail on the phenomenology of philosophical delusion, in particular on the *circumstances* in which one is prone to philosophical illusion/delusion (and relevantly similar forms of psychopathological delusion).

33 See especially Dupré (1983, 1981, 1996: 108). Dupré's arguments undercut the notion that we should always worry if there are two enduring very different 'levels of description' of what seems to us in some sense to be the same phenomenon. Thus he undercuts even Nagel's unusual stance: that consciousness simply cannot fit into human beings' scientific schemata, but must be a real objective phenomenon all the same. For this point need only worry one if one has a reductionist view of the task of science, and more broadly some 'unified science' point of view.

No good reason has ever been given for us to suppose that there must be a science of something, if it is to be regarded as real. There is no good reason to think that there should be a science of consciousness, or of the mind, or of society, any more than there need be a science of numbers, or of universes, or of capital cities, or of games, or of constellations, or of objects whose names start with the letter 'E'. If there is/are (a) science(s) of the mind, let us please hear what its genuine puzzles are, what its exemplary puzzle-solutions are, etc.[34] By this point in the book, I think I am entitled to contend that none such that would genuinely satisfy are available: that virtually all would-be scientific 'human science' is through and through *programmatic* – whereas *scientists* (i.e. practitioners of one or another natural science) generally care barely a jot for *a prioristic* programmes or methods, but just get on with it, with scientific work, with solving their puzzles and advancing their fields.

This does *not* mean, heaven knows, that there is nothing interesting to say about human beings beyond biology: there is such a *wealth* to say, using the approaches of (non-scientistic) philosophy, history, literature, ethnography, etc. What there is not to say, I am suggesting, is anything analogous to the results of a science – and any attempt to turn some area of human studies into a science is likely to have exactly the kinds of *counter-productive* effects that I have been giving instances of in this Section.

So: there is no particular reason to trust one set of philosophical *intuitions*[35] (McGinn's, or Descartes's, or Searle's, or Campbell's, or indeed mine) over another, hereabouts. There is no particular reason to find consciousness innately surprising, and thus in need of 'scientific' explanation. I claim that, insofar as there is a problem or a 'mystery' which concerns consciousness, it tends to *dissipate,* when we turn our focus seriously to how people (ourselves included) actually talk, to what we actually do, to what actually happens to us in our world, to our actual existence. When we really meditate or look or listen, and stop idly thinking and theorising. For instance, I may get myself bewildered if I try to become self-conscious of *my consciousness*, and wonder how '*this*' could possibly be related to matter. But do I have any such difficulties if I simply become aware of my own breathing and thus of my mind-and-body (rather than trying to 'capture' or 'explain' my own thoughts); or of course if I look (with compassion, with openness) at someone moaning pitifully, or writhing in pain? And let us *not* think of the concept of 'pain' purely in the abstract. Picture

---

34 I draw here on a broadly Kuhnian framework for the understanding of why it is that we tend to call something a science – i.e. that it exhibits the features of what Kuhn called 'normal science', that it is not simply riven by schisms between schools, by foundational disagreements – as the 'human sciences' *are.* For detail, see Part 1, above. Cf. also the discussion toward the end of the 'Concluding Summary' to the present work, below.

35 For an impressive general case to this effect, see Hintikka's (1999). Particularly relevant for our present purposes is Hintikka's critique of the appeal to 'intution' of Chomsky and others, on pp. 132–6.

instead (say) the scene of a terrible railway accident, or something you yourself once saw (say) in a hospital.

Or, let us even take a somewhat atypical case – arguably a kind of case which could not be the paradigm, but which is a variant special case of pain in public: What if one is alone, and is oneself suddenly afflicted by some kind of pain? I would go so far as this: if one stops thinking of the consciousness-brain 'connection' obsessively, and instead has some pain inflicted on some region of one's body, one won't suffer from a sense of the 'mystery' of mind and body any more. Even in an artificial situation, where it's hard not to be prejudiced by all the philosophical theorising we've learnt in a culture saturated with dualistic *and* scientistic ideology, this still may work. Try it now. Give yourself some kind of hard blow, or at least a hard scratch. You feel it where you were scratched – of course – and that's not surprising. You are not surprised, merely slightly pained. You are a whole person, and it is *you* that thinks, or feels pain, not one of your organs or a part of your body.[36] And now I think we can see more clearly the location of a key mistake made by almost every mainstream thinker in the field. We need to *start* with the idea of ourselves as embodied persons, acting in the world, NOT with the idea of ourselves as brains with minds 'located' in them, or 'attached' to them – whether materially, computationally, supernaturalistically, or what have you.[37] If we don't *start* with that idea, we will certainly never be able to *reach* it through a process of theoretical reduction, of 'division', and so on! The thought-style we need, we academic students of mind and society and culture *especially*, is one we are especially resistant to: we need a methodology of thinking that places centrally our embodiment and our actions (and our interactions). That will be more productive than just thinking about and focussing on our intellectual problem-solving and our contemplation.

Wittgenstein gives us a good opportunity to overcome, by means of persuasion and redirection of attention, our tendency – especially we intellectuals – to alienate ourselves from our own ordinary embodiment, our seamless mind-and-body-and-soul-ness.

On the Persistence of Consciousness-boggle

I know that some readers will still not be satisfied. Some of you will be dissatisfied by my method. You will think that this Section lacks argument, and is too devoted

---

36 A more detailed version of much the same point is to be found in Hacker (1990). For instance, Hacker reminds us that it is misleading to think along the following lines: 'The brain is our organ for thinking and feeling, much as the intestines are our organ for digesting'.

37 And to think of ourselves simply as brains, without any mind 'attached', as eliminativists do, is still to think *within the same thought-style,* within the same quasi-Cartesian space, as dualism, transcendental naturalism, and the rest. I am suggesting a more radical break with that thought-style.

to rhetorical efforts at persuasion. To such readers, I can only say once more that this Section, and indeed this book, is indeed perhaps not exactly best construed as a work of argumentative non-fiction, but as a work of would-be *therapy*.[38]

Some of you, be you sociologists or psychologists or philosophers or what-have-you, be you even sympathetic to my general intellectual orientation, including even perhaps my philosophical methods, will still find yourselves feeling things like, 'But even after all this discussion, *even if* I might find myself intellectually rather convinced by Read's methods and arguments or at least very taken with their rhetorical force, I still sometimes want to say, while looking into the mirror: "How can I be *this*?" Or alternatively, if I really reconcile myself to my physical existence, I still want to say, contrariwise, "It is just so weird, that *this*/any physical thing can be *conscious*. How is consciousness *possible*?"'

As long as these questions persist – and though I myself feel their pull relatively rarely, yet it would be quite untrue to deny that I ever feel it – then the task of the Wittgensteinian therapist of philosophy and of 'social science' and of culture is not over. Why *do* these questions persist?

Does their persistence not indicate a weakness or gap in my line of discussion? Does the mind boggling at consciousness not presage a truth at the heart of the words of those who (like Campbell, like Chalmers) wish to regard the (heart of the) mind-body problem still as a genuine problem? Does it not perhaps support specifically the thought of those who (like McGinn) worry that we will always feel this boggle, that consciousness will always elude our (human) grasp *and* (specifically) our (Wittgensteinian) therapeutic deflations alike?

I do not think so. What is indicated, rather, is this: precisely that 'the problem of consciousness' is *not* in the end an intellectual problem, at least if by 'an intellectual problem' we mean a problem accessible to full resolution by the normal stratagems of academic debate in the human sciences/studies. It is rather a deep problem of our culture – and of our selves. It is, we might say, an intellectual *trap* that has its roots in our intellectual*ism*. In our continual reaching for intellectualist answers/solutions to problems that are in part problems of being captivated by metaphors that contain within them all such 'answers'; and in part problems of *mood*.

Thus, my cautious talk above of my alternative approach offering an *opportunity* to return to the everyday; of an immersion in real life (in dance, in *meeting* others, etc.) as *tending* to foster a diminution or dissolution of 'the

---

38 Why 'perhaps'? Because it would be a silly pragmatic self-refutation for me to say that my Section here is therapy *as opposed* to argument. That would imply that I can survey the terrain of the debate about consciousness quite from outside, and tell you what kind of debate it *really* is. Whereas, all I wish to do is to offer a characterisation that you may find helpful: try thinking of what I am doing in this paper as engaging in a therapeutic conversation with myself, and with you, as providing a series of provocations to better self-knowledge. I believe that *what philosophers have traditionally thought of* as argumentation hereabouts, by contrast, misfires, and tends, as I have argued (!), to *worsen* and not to improve our state of understanding.

problem of consciousness'; of *gradually* losing one's sense of a necessary boggle; and above all, perhaps, of being convinced or persuaded of something by means of *a redirection of attention*, not by means of a sequence of logical arguments. In the sense of the word 'argument' common in Analytic philosophy and in 'scientifical' sociology, it would be reasonable to say: there are at the crucial junctures no arguments at all in my paper. I have not proved anything decisive about consciousness, or even about language. I have not refuted dualism, or eliminativism, or even any kind of scepticism.

I have not tried to argue that 'qualia' pose no philosophical or human-scientific problem. Nor have I tried to argue that we should set aside such problems. Rather, I have made effort after effort not to generate illusory problems with the ordinary, and to discourage you from doing so. I have no argument with which to solve or even dissolve 'the hard problem of consciousness', nor do I have an argument that there is no such problem. If I did, I would be a conventional philosopher. And such conventional philosophy never succeeds, never settles anything. My unconventionality consists in trying only to get you to drop your insistence that there *certainly is and must be* such a problem. Following Baker's (2004) rendition of Wittgenstein's methodology, I simply, doggedly, try to make available an alternative possibility. An alternative perspective, from which 'your' dogged certainty itself appears to *be the problem*. My task is potentially endless; for, even if I succeed in re-orienting your thinking such that the problem of consciousness dissolves for you, there can be no guarantee that it will not be provoked into returning again, in you, or indeed in me.

The boggle at consciousness (a boggle manifested most powerfully in 'physicalist scepticism' in the eliminativist incredulity at their being anything more than 'blank body' in the world), and the symmetrical boggle at body (a boggle manifested most powerfully in Cartesian scepticism and its successors), are not in the end, I suggest, intellectual problems in the standard sense. Contrary to what many 'optimistic' philosophers, 'cognitive scientists', psychologists, sociologists etc. would like to think, these problems are not amenable to solution by means of argument;[39] or at least, not by means of anything like argument alone. They are historical problems; they are cultural and even political problems; and they are individual psychological problems (i.e. problems of 'mental health').[40] They are, as Wittgenstein suggested, problems of practice, of attitude, of the will. In sum, they are, as Heidegger and Stanley Cavell have suggested, in large part problems of 'appreciation' (of life), and problems of *mood.*

Take Cartesian scepticism. What Wittgenstein, and Cavell, and Heidegger have been after, and what has made them so difficult to assimilate into 'the academic world', is that they do not treat the Cartesian impulse as principally to be evaluated by means of arguments for and against its philosophical position. They take such

39 And not because they could be solved, though not by us, but only by God or super-clever Martians.

40 See for instance Section 2.3, above.

arguments to be ultimately impotent, and indeed a distraction, a sublimation, an avoidance of real (and scary and harmful, as well as in a way revelatory) experience. The root of the Cartesian impulse – the impulse that eventually results in 'Dualism' – is an attitude, an anxiety, a *mood,* in which the world doesn't feel reliably or completely *real*. All premises and conclusions are impotent in the face of such a mood. This mood (of unreality) is, when felt, very real, and often *very* frightening. When I myself have been subject to it, I have felt the utter impotence of all 'refutations of solipsism' and 'arguments against scepticism' and 'solutions (or even 'dissolutions') of the mind-body problem. If there is persuasion back out of this mood, it is the kinds of persuasion I have used and mentioned and gestured at in this Section. For example, a 'persuasion' of one person talking *with* and being *with* (and dancing with?) another, not a persuasion of one person forcing another to accept the rationality of a conclusion.

Moods such as 'the Cartesian mood' can be understood only by entering into what one not in the grip of such a mood will usually find to be nonsense – but that something 'is' nonsense doesn't always stop one from feeling persistently as though it isn't. And moods can be altered only by processes which are partly orthogonal to 'the space of reasons'. Hume, Wittgenstein, Heidegger, Cavell and Cora Diamond can help us to understand these moods by giving us entry points into them 'imaginatively' – not by literally making sense of them. They can help us by offering us strategies for understanding their etiology, and not by supposedly reducing them to nothing, or repressing them by rational argument. By offering us quasi-behavioural strategies for taking our attention and our practice into places where these moods are less likely to arise or persist – not by denying that the phenomenon of boggle at body (or, symmetrically, of boggle at consciousness) is a phenomenon which people experience, suffer, and even use.

When Chalmers insists on the tenability of Mind-Body Dualism, or when Campbell insists that individual minds are the true locus of all meaning and intention, the underlying motivation, I wish to suggest, is a persistent boggle at body (which includes, crucially, a boggle at there actually being other people). This boggle is, I am suggesting, above all a mood, and not a logical conclusion. It is a way of seeing the world, a particular set of qualia and intellections, a pathological way of being-in-the-world, or even of *not*-being-*in*-the-world (cf. 2.3, above). I say this not to mock it – I would be mocking myself. I have been in this mood, just as I have occasionally been in the 'opposite' mood of incredulity at consciousness (including even my own), and somewhat less occasionally in the rather different, orthogonally-boggling mood of incredulity at there being *anything* at all, anything *whatsoever*. Such moods require 'therapy'– whether it be Wittgensteinian therapy, or psychotherapy, or the therapeutic comfort and reorientation offered by dance, by love, by walking and talking in the park or in the hills, even repetitive work.

The specifically Cartesian mood, the felt mood of boggle at body, is the scariest of all, in my experience. I think that we would do ourselves and our philosophical and cultural inheritance a great service if we were honest about this, and if those of us who have experienced such moods wrote honestly about our experiences, and wrote

honestly about (I suspect) the irrelevance to such experiences of all the gassing about knowledge and evidence and doubt, about mind and body, structure and agency, self and other, and so on. It would also be of great service if those of us, if there are any (and I strongly suspect that there are) who have truly never experienced such moods, confessed that we were engaging in debate about mind and body from a position of mere abstract interest in argument, in mental gymnastics, and *not* from a position of comprehending what it is like to be a participant in a lived experience, an experience which motivates, as desperation motivates, a real debate or discussion or confession or oration. The concern I have about all of the standard 'positions' on mind, on consciousness, even that of McGinn, Nagel *et al,* is that they dress up as an intellectual conclusion what is actually the outworking or sublimation of a perhaps inchoate but nevertheless lived mood.

This is part of what I mean by saying, as I want to say, that the mind-body debate is a zombie. Unless we find ways of laying it to rest, perhaps over and over, in our own lives – in our culture(s), in our societies – and (not just) in our academic work, then it will 'live' on – and *we* will risk becoming zombies as a result. That is, we will risk either immersion in distracting, substitutional, intellectually bankrupt and ultimately empty theories, or (still worse) we will risk possession by moods which empty us of our capacity for any kind of sane life. 'Theory' (as opposed, to put it slightly crudely, to 'therapy') is largely *impotent* when it comes to addressing the *underlying* malaises of our 'mental lives'. This is the final (and actually, humanly important) lesson that comes from reflecting upon 'the problem of consciousness'.

This is a lesson that makes this section, 2.5, important for the argument of the entire book. For here we have reached a point quite clearly beyond Kuhn, and probably beyond Winch too. Here it has become clear that there are aspects central to our lives with thinking that are not susceptible to answers through thinking unless we radically question our paradigms of what 'thinking' is. The present Section functions in a way then as a kind of pre-conclusion to the entire book: it suggests that, at the end of the road of thinking through the methodology of the sciences, a Wittgensteinian perspective returns us to ourselves and offers us the outline of a 'cure' of ourselves; a cure that we have to *live*.[41]

41 Thanks to Ken Westphal, Angus Ross, Emma Willmer, and Rod Watson (especially), and also to John Collins, Chris Cowley, Kelley Dean Jolley, John Arthur, Richard Hamilton, Ayo Mansaray, Jerry Katz, Jim Morley, Bruce Wilshire, Ranjit Nair, Jonathan Smith, Louis Sass, Jeff Coulter, Wes Sharrock, David Smith, Mark Sacks, Espen Hammer and the Philosophy Senior Seminar at the University of Essex, and an anonymous referee, for useful ideas and comments on earlier drafts of this Section.

A Concluding Summary

> Philosophy asks 'Why does it have to be thus?' and 'Why should it not be thus?'; questions that are beneficial in science, because they clear away prejudices.
>
> Ludwig Wittgenstein, MS 133, 60v.[1]

> Science: enrichment and impoverishment. The one method elbows all others aside. Compared with this they all seem paltry, preliminary stages at best. You must climb down to the sources to see them all side by side, the disregarded and the preferred.
>
> Ludwig Wittgenstein, *Culture and Value*, 69.

The fundamental conception of this book has been to introduce a 'therapeutic', liberatory, Wittgenstein spirit into reflection on the methodology of the sciences (and of the 'sciences'). Wittgenstein has been widely misunderstood as hostile to science.[2] What Wittgenstein was in fact hostile to is only: scient*ism*.

Scientism imagines that all of 'science', and ultimately all of whatever can be understood or explained, can be so via 'the scientific method'. We are now in a position to see just how horribly wrong this ideology is.

The scientific approach approaches the world like we sometimes imagine an alien would: As completely alien. As needing to be exhaustively categorised, in terms we cannot anticipate *a priori.* There is *something* right about this, so far as it concerns science: It captures an aspect of normal science (the relentless effort to categorise and explain) and an aspect of revolutionary science (the strangeness, at root, of the 'new world' that is presented by a Copernicus, a Lavoisier, an Einstein, etc.). It is strikingly wrong as a would-be approach to the *process* of understanding science itself: here, we have to apply historical and hermeneutic etc. methods (See *ISS* 88–9).

Analogously: the scientific approach is wrong for the 'subject-matter' of 'the human sciences'. The approach in their case should be, roughly: 'from the inside'. *Via* familiarity, of an appropriate kind (which may take some finding: see 2.1, above; we have to re-allow a kind of 'alien-ness', in cases of genuine social etc. puzzlement). In relation to human studies, then, the strange is only: what won't fit (yet) with what we already know. This can sound strikingly similar to the enterprise of normal science, but there is a vast difference, to which McGilchrist is particularly attuned: For the 'knowledge' we already have in the case of the human world is in turn knowledge 'from the inside', knowledge we 'inhabit'. Knowledge

---

1 Thanks to Oskari Kuusela for pointing out this quote to me, and to Philip Wilson for help with translating it.

2 A useful recent source for making clear Wittgenstein's closeness, in many regards, to science, is Janik (2006).

of the living, as by one of the living; knowledge which isn't (overly) *knowing*. Knowledge, that is, which lacks *knowingness*.[3]

This is what I have sought to emphasise periodically in this book: especially at the close of 2.3, and also, in a preliminary way, at the close of 1.3: That the difference between ordinary scientific understanding and the kind of understanding characteristic of the human studies is not exactly a difference in subject-matter (For that would be the left-brain's way of putting it). It's more a matter of *subjects* (as opposed to objects). And: Subjects are not a subject-matter. They demand a different way, a different how.

My reader might frustratedly be asking: 'Please can these metaphors be 'cashed out', these elliptical ways of speaking be simplified and factified!' But that demand itself reveals a failure to take the difference being unconcealed here. We are inevitably in the domain of metaphors here, a domain which cannot be escaped without violence being done to the phenomena, without *over*-literalisation. We are in the domain of what Wittgenstein called 'objects of comparison', the domain of 'therapy'. (From a 'left-brain' perspective, this is endlessly unsatisfying; from a 'right-brain' perspective, it is inevitable.[4])

In the academy, and in the world at large, 'theorising' and (most forms of) 'explanation' may indeed be on the ascendant – theoryism and scientism run rampant, their models clearly rule the academy (look for instance at the way that all funding councils are modelled on science funding councils; at the way that the catch-all term 'research' so patently ill-fits many of the humanities, such as creative writing; and so on) – but this a castle built on quick sand, outside of its natural home.

Why? Well, it's a question of logical priority. 'Theorising', as an activity, is in some respects a natural language pursuit, *and is inevitably the more so,* the less it is reducible to maths and *the more its would-be subject-matter is subjects* (unless the theorising loses any tether to socio-human reality, as happens sometimes in for instance economics, in which case it reverts to being simply something like 'applied' maths), for reasons we have explored at times in part 2, above. Theorising employs linguistic resources many of which have not yet been clarified, have not had their logical grammars already made explicit and analysed.[5] Until the resources – resources taken [6] from our manifold metaphorical 'systems'

---

3 See McGilchrist (2009: 96–8). Kuhn and Winch, I suggest, manifest a wonderful lack of knowingness. By contrast, scientism and post-modernism alike are horrifically *knowing*.

4 The urge to make the 'inexplicit' explicit is the endless urge of the left-brain; also, of the pornographer. See the final chapter of McGilchrist's (2009) for discussion. 'Making explicit' can reduce/crudify/destroy the very phenomena it means to 'capture'.

5 This for instance is why Lakoff and Johnson, like Wittgenstein before them, found such rich pickings in philosophy, and in relation to linguistics, psychology, mathematics, economics, and so on.

6 In ways that are in many cases usefully explicitated by Lakoff *et al.*

of conceptualising, inevitably manifesting or parasitic on the concepts actually used by members of a culture, the methods of the people – used by 'theorisers' are treated as topics for explicit attention [7] rather than as unexamined, unexplicated, taken-for-granted resources, theories *cannot* be expected to be coherent, and will always rely on (and be compromised by) the largely unknown properties of those resources.[8] Even if one sees it as a valid objective to 'theorise' (which, obviously, I tend not to, in relation to 'the human sciences', let alone to the humanities proper), *theorists remain in need of Wittgensteinian clarifications,* etc. . This latter is not (just) a 'definition of terms' but an examination and investigation of the linguistic resources that actually shape philosophical etc. theories.

Of course, once more, none of this implies in the slightest that there cannot be science proper, nor even that our idea of the epistemological status of actual science needs significantly revising. For scientific theorising is, as Wittgenstein put it, capable of undergirding new uses of words (provided of course that the theory is well-attested) in ways that philosophical etc. theorising is not: 'In a scientific perspective a new use is justified by a theory. And if the theory is false, the new extended use has to be given up. *But in philosophy the extended use does not rest on true or false beliefs about natural processes. No fact justifies it. None can give it any support.*'[9]

In Sharrock and Read (2002), we showed that a Wittgensteinian reading of Kuhn was possible, and argued roughly that the more Wittgensteinian Kuhn was, the better. Kuhn too has been widely misunderstood as hostile to science, when in fact he was only hostile to widespread mischaracterisations of science. (The book was about as charitable as possible to Kuhn, from a Wittgensteinian angle. As I noted in the Preface, above, some, such as Fuller (2005), have argued that actually the interpretation offered was of Kuhn as he should have been from a Witttgensteinian perspective ('Kuhnenstein'), not of Kuhn as he actually was. Possibly. What matters in the end is developing with the reader's aid the right philosophical/methodological/therapeutical orientation – not whose name to put to it.)

But Sharrock and Read (2002) did not particularly aim to take up a distinctively *therapeutic* conception of Wittgenstein's philosophy, the kind of conception present for instance in Crary and Read (2000), and in Baker's later work (his (2004), etc.). What Part 1 of the present work does is attempt to correct that omission, or at least to fill out somewhat what a therapeutic Wittgensteinian vision of Kuhn as a methodologist offering us for the first time ever a decently-complete vision of the concept of science (i.e. of our paradigm of science, natural science) would be.

---

7 This would be ethnomethodology's way; and my thanks to Rod Watson for the idea of this paragraph.

8 This is why, for instance, it is a fool's errand to search for THE single unified nature of ethics, as most 'moral theory' does. Ethics/morality is inevitably plural/disunified (as Lakoff and Johnson lay out).

9 P.44 of his (1980); emphasis added.

The way that I undertook outlining and filling in that vision was as follows. In 1.1, I reminded the reader of what is often forgotten, the explicit presence in Kuhn of elements of a therapeutic understanding of the process of science itself, at times when science gets 'ill' (when, that is, the paradigm in a discipline is 'monstrous', or when it is on the point of breaking apart or multiplying into different discrepant forms). I then offered a reading of the most controversial chapter of Kuhn's *Structure of Scientific Revolutions* (on 'world-changes') highlighting the delicately more-or-less therapeutic way in which it is *written*, a manner of careful composition which draws the sting from the still-widespread uncharitable reading of that chapter as committing Kuhn to (what *would* be clearly un-Wittgensteinian) a substantive semantic relativism. If my reading is successful, it defeats the strongest textual evidence there is for a problematically relativist reading of Kuhn.

Thus 1.1 assembles crucial evidence for Kuhn as a therapeutic thinker in a more or less Wittgensteinian mold, and (thus) suggests some reason for thinking of Kuhn as a truly Wittgensteinian philosopher of science. This sets the scene for the remainder of Part 1, and to some extent for the book as a whole, insofar as what the book is concerned with is assessing the extent to which it is helpful to regard a *variety* of disciplines as 'science(s)'.

1.2 – 1.3 explored the great objection to the 'programme' for reading sketched and examined in 1.1: namely, the enduring *attractions* of reading Kuhn as committed to a dogma of strong semantic relativism. It did so initially in a way bypassed in my earlier book: by *inhabiting* those attractions. In other words, the conception of the progress from 1.2 to 1.3 *is itself a fundamentally therapeutic one*: My aim [10] was to invite the reader to explore the attractions of that influential relativist reading of Kuhn (including the genuinely powerful motivations (stressed by Kuhn himself) for applying Kuhnian thinking about science to the philosophy, epistemology and methodology of science) *so as to find the most effective possible way of exhausting them*. Thus the conception of Part 1 of the book is in this respect analogous to the conception of Wittgenstein's *Tractatus* on the therapeutic reading of that work: to inhabit a philosophical temptation, an attraction, so as better to be able to know and to *overcome* it. By the end of 1.3, I hope that the reader will have thoroughly exhausted the desire to find in Kuhn's central concept of incommensurability a relativistic semantic doctrine, and to have reached instead the broadly Wittgensteinian (and Winchian) moment of seeing incommensurability as a fundamentally *methodological*/therapeutic device. What 'incommensurability' is, above all, is a means for the historian of science to understand defunct paradigms without assimilating them to our current interests in Whiggish fashion (and thus without losing sight of their 'alienness'), and for certain (self-aware / reflective) practicing scientists or anyone interested in reflection on the nature of scientific progress to understand how such progress renders the past of science a place that is difficult to explore without such Whiggish assimilation.

---

10 Which would have been undermined had it been blatantly disclosed at the opening of the book; better to foreground and state it directly only at the end of 1.3, and now.

Thus 1.2–1.3 exhausted what McGilchrist would think of as the (revealingly widespread) 'left-brain' endeavour to interpret Kuhn as purveying a *doctrine*, a theory about science and about meaning. By the end of 1.3, one is left instead with the outlines of a more 'right-brain' wholistic, unscientistic picture of what a Wittgensteinian (Kuhnian) orientation toward the natural sciences would/should consist in: An unrelenting trying to *make sense*. To make sense of the world, and (following the methods of 'human studies', of history etc.) to make sense of previous/'alien' visions of the world (i.e. of out-of-date science).

There then followed (1.4) a brief investigation of what is too often left aside when incommensurability is discussed; namely, Kuhn's important remarks on incommensurability of scientific *values*. I related these directly to Kuhn's main reflections on incommensurability. The importance of these for the general approach of the book was once again to make clear how Kuhn is no Relativist, and thus to head off pre-emptively a potential misunderstanding of Part 2 of the book (as a set of exercises in Relativism).

And Part 1 was rounded out (1.5) by a still-terser account (one clearly building on 1.2–1.4) of just why it just isn't terribly helpful to see Kuhn as having a 'model' of science, and of the relation of this point to questions of science policy, via the suggestion that allegedly Kuhnian relativism or allegedly anti-Kuhnian 'social epistemology' as put forward by Fuller *et al* offer an equal poverty of policy advice, and that a richer place to start is with the thought that Kuhn facilitates for us a sense of the considerable (but inevitably non-total) extent to which science (but not technology) thrives on and is entitled to a freedom from broader societal dictates. (This leads naturally into Part 2 of the book.)

What Part 1 is supposed to leave one with is an enriched sense of what one oneself will take to be the relevant features of science that Kuhn's conceptualisation of scientific methodology enables one to bring focally into view: the nature of normal science, and of its occasional transmogrification into extraordinary science. Normal science as a generally 'left-brain' activity, revolutionary science as opening more to the novelty and wholistic re-evaluation that is the domain chiefly of the 'right-brain'. And the effort to understand all these matters as *itself* largely 'right-brain' in nature: more akin to understanding another person than to doing some normal science. Thus making Kuhn's own role, the role of any philosopher or methodologist, more akin to the matters under exploration in Part 2 of the book than to the matters under exploration by practicing scientists.

I emphasised in the opening sentence of the previous paragraph a personal dimension: Another, crucial way in which my work is intended to be therapeutic is that it does not aim to impose upon the reader. As stressed at the opening of the present work, in the Preface, while I have strong opinions, these are not the nub of the matter: ultimately, *the reader has to come for themselves* to a (tentative, malleable) decision as to how best to employ this value-laden term of praise, 'science', and on what features of its employment in the domain in which its employment is relatively unproblematic are worth highlighting, emphasising, holding onto.

In Part 2 of the book, after being 'thematised' by the 'Inter-Section' that leads from Part 1 to Part 2, this 'decision' (of the reader's) was put to work. Part 2 takes one on a journey through some of the 'human sciences', and especially through most of those disciplines most inclined to claim that they (perhaps unlike other alleged human or social sciences) genuinely *are* sciences. Part 2, in other words, puts Wittgenstein to work among the human sciences.

2.1 introduced this project by building on my, Hutchinson's and Sharrock's (2007),[11] by sketching some of the failures of (some of the more intelligent of) Winch's alleged critics to understand how Winch can be read [12] as offering a genuinely therapeutic Wittgensteinian take on the methodology of the social studies, and by offering an indication of some of the reasons (reasons explored in the remainder of the present work) why we should be at least suspicious of the claim to scientificity of these disciplines. One should perhaps be suspicious *prima facie* of *any* discipline too keen to call itself a science, and furthermore of why/how such claims frequently obscure the *independent worth* and different functional nature of the social studies. 'Science' is not the measure of all things. Humanity/sociality/society have their own measure(s).

The all-too-predictable irony of this, in self-reflexive terms, is that 2.1 sadly details exactly what Winch's own thinking would predict: that there has been an insufficiently serious effort *to understand* Winch, among those who would wish to criticise him. That, if one wants to criticise, one has first to understand, and this

---

11 The huge irony of the programmatic, science-aping nature of much 'social science', as explored in *There is no such thing as a social science,* is that in this regard 'social science' manifestly utterly fails to successfully ape (natural) science – for (real) sciences developed *not* through aping other sciences, but through actual empirical etc. study that eventually issued, through something like or prefiguring anomaly, crisis and revolution, in paradigms that gave birth to more unified or at least novel research traditions. In the very act of attempting to copy (natural) science, 'social science' invalidates its own scientific pretensions – for science did not and does not proceed by such copying. (For detailed exposition of this final point, see Sharrock and Read (2002).

But, similarly to the case of my co-authored *Kuhn*, the conception of Winch's philosophy offered in the co-authored *There is no such thing as a social science* was not, perhaps, quite as thoroughly *therapeutic*, as influenced by Cavell, Diamond, Baker, etc., as is the conception which is both explicit and implicit in Part 2 of the present work. This can be partly gleaned by the contrast between its polemical, provocative title and the more exploratory work, leaving more to the reader, that I hope is foregrounded in the present work. This is I think mostly only a difference in emphasis and in style, but it is still a difference that makes a difference. What polemical elements there are in the present work are, I hope, hedged and nuanced by the repeated invitation to the reader to decide for themselves where they end up in relation to the questions that this book raises.

12 Again, one might doubt whether this is the best available reading of Winch. I think without doubt it is 'best' in the sense of most charitable and most useful. However, whether it is 'best' in the final analysis in terms of being most exegetically accurate is a judgement I can only – and am quite content to – leave to the reader.

first base is all-too-rarely attained. That, in order to have a shot at understanding the strange, one sometimes first has to put it at a *greater* remove from one, from the kind of thing one is used to thinking about (say) a theory being (indeed, in this case (of Winch), as in his of the Azande: one has in particular to be prepared to consider the possibility that *what* one is trying to understand is not a theory at all. One has to be ready to open one's mind beyond scientism and beyond theoryism). In sum: *the primitive misunderstandings of Winch that one generally encounters* mirror *closely the very primitive misunderstandings of the Azande etc. that Winch sought explicitly to overcome!* (The truly primitive culture, we might say, is in an important sense at least just as much that of Frazer, Evans-Pritchard, that of social science, *and* that of Winch's critics, as that of the Azande…) This naturally put a question-mark over the frequent moves in sociology etc. to figure Winch in just such a – primitive – way.

2.2 and 2.3 plunged one into a discipline – psychiatry – that frequently trumpets its own allegedly scientific status, but that, in order to truly function, must (I argue) come genuinely to be open to and to understand (to the extent that such is possible) the phenomenology of the phenomena that are its subject-matter: the lived reality and (sometimes) self-perpetuating course of psychopathologies. Self-perpetuating, in that the *phenomenology* thereof is a crucial part of why they are difficult to escape or to emerge from. My argument in both Sections is that Wittgensteinian thinking has something substantive to offer in this regard: Wittgenstein's therapeutic diagnosis of solipsism and of other philosophical maladies can offer one vital clues to the phenomenology and thus to the aetiology and course of some psychopathologies. The terrible danger of the quest to make psychiatry 'scientific', whether this be *via* biological reductionism, or *via* cognitive-scientific modelling of 'abnormal cognition', is that it obscures this kind of possibility, and also obscures the relevance then of humanistic understanding to the essence of the subject. (Thus the real and consequential force of the question: Ought we to figure psychiatry as a science?)

The radical human possibility that I am aiming at in these sections is: the possibility that sometimes in human life, practice and phenomenology there is nothing but the sound, the jingle, of sense, with nothing lying behind it (cf. Read, 2007a: 78). I mean to be describing *a conceptual possibility* (in (severe cases only) of schizophrenia etc.) that Louis Sass neglects – that of complete loss of world and of sense. It is important to be clear, in understanding these Sections, that calling something nonsense is (for therapeutic Wittgensteinians such as myself) an endlessly provisional category, unless one gets the agreement or acknowledgement of the speaker. In cases where such agreement is not possible / not available, then nonsense remains an *endlessly* provisional category/judgement, *not* an absolute and irrevocable verdict.

Winch's real topic, in papers such as 'Understanding a primitive society', is *avoiding mis*-understanding. But this is because it is reasonable for him to assume that there is some form of embodied first-order understanding present in the practices/language-games of the people in question. But just this assumption is what

I need to question and (so) aim to question, in relation to the subject-matters of 2.2. and 2.3. That points to the great loneliness of some psychopathological suffering (and our loneliness too, in relation to these sufferers). There is no analogue for this in the case of people who think that they carry their soul around in a stick, or other examples of strange practices that anthropologists have encountered – we are not sundered from the soul-stick-carriers as we are sundered from some sufferers of schizophrenia etc. .

In 2.2, furthermore, I connected this topic back to 2.1 by offering a sketch of/an overview of a possible *taxonomy of interpretive options* for psychological etc. phenomena, a sketch with a broader range of possibilities present than are likely to occur to one if one enters into thinking psychiatry by way of a set of scientistic assumptions. Standard scientific modes of understanding in psychiatry etc. are *included* in this taxonomy, but the taxonomy is not *restricted* to them. The taxonomy provisionally offered here is intended itself as an 'object of comparison' (see *PI,* 132), not as a set of quasi-scientific models. It is intended to help mobilise possibilities for beginning reflection, on those occasions (whether in sociology, anthropology, politics, psychology, psychiatry, economics, management studies, or what-have-you[13]) when one is confronted by a situation or a phenomenon which defies ordinary understanding, or appears to, or at least that appears to require explanation or something like that. For, following Winch, I suggest that, unlike in the case of natural science, the social studies and the 'human sciences' are only ever *called-for* when one is confronted by such a situation. The norm in social life is for situations and phenomena to not be in need of any explanation *at all,* beyond that which we are all preternaturally capable of and routinely exercise as 'masters' of the language (and as always-already accustomed to and acculturated in and to the society of which we are a constitutive part). Natural science, I would suggest, is a project that can and should expect to apply to everything in its domain; the social studies and *'sciences humaines'*, by contrast, need only be there to fill gaps.

Thus we come to see some key reasons for suggesting that expertise in social understanding is in the first instance a maturational *art*,[14] not a science open to expertise in any 'academic' sense of that word.[15] There is and can be, I would

---

13 My hope, in other words, is that this 'taxonomy' may prove useful to the reader in looking at new cases, anywhere across the '*sciences humaines*'.

14 For more on this, see also the conception developed in Flyvberg's (2001, *passim*), on the basis of Dreyfus's work.

15 Thus the risk is that 'social science' tends to crudify and reduce to an over-simplified unity (or some such) sophisticated contrasts and variegated contextual understandings that are available to and made manifest in the practice of ordinary people in their everyday lives. *The practice of ordinary members of society* vis a vis *(e.g.) reality is already more sophisticated and reliable than theorisations of it made by philosophers and sociologists*. This point is on my reading central to Winch's work, just as it is implicitly manifest throughout Wittgenstein's and Harold Garfinkel's: social science, usurping philosophy's role, which in turn generally usurps and over-simplifies/crudifies the role of the competent

suggest, no *elite* of experts in (the genuine content of) social and human 'science.' In an important sense, we are *all* such experts – as, very roughly, we are all (all – or at any rate most – readers of this book) experts in the English language. For Winch, Wittgenstein, and ethnomethodology alike, by utter contrast with mainstream philosophy and sociology, the closest there is to an expert in the 'field' is *an ordinary broadly-competent social and linguistic being*.[16] The social and human studies, I would submit, unlike the 'social and human sciences', not only begin but also end with non-academics, with (competent) members of a community. Social study is above all something that we do most of the time, *we humans*. As McGilchrist's work makes wonderfully available: the humanities, and 'human/social *studies'*, is (are) the heir(s) to a whole different way(s) of seeing/hearing/being in the world than that recommended by any science.

In 2.4, I turned to economics, another case in which theorists and practitioners are often extremely anxious to distinguish themselves from their 'lesser' cousins such as sociology and anthropology, and to undergird their pretensions toward being regarded as truly scientific. My topic here was Milton Friedman's vastly-influential founding methodological statement, '*The methodology of positive economics'*, still a mini-bible of many economists and economics-students today. I offered a close critical reading of the crucial early part of that essay, drawing out in the process various ways in which Friedmanian positivism and instrumentalism, influenced by an image of the alleged nature of real science, sets economics on a disastrous pseudo-scientific footing. In the negative of Friedman's essay (and under the more positive influence of the likes of Wittgenstein, Keynes, Winch, Hacking, Mirowski, and Harold Garfinkel) one can start to see once more the kinds of features of human and social phenomena – hermeneutic in some cases, involving 'feedback-loops' of self-consciousness in some cases, involving normative commitments in many cases, involving lived capacities for correction and explication and accounting (involving, that is, ordinary routine social and human competence) in virtually all cases, and so on – that tend to be obscured by a scientistic presentation and ambition such as we find in most economics. From this, I drew the methodological/policy moral that moving beyond a scientistic understanding of economics is likely to be crucial to avoid repeating the kind of financial-plus crisis that has gripped the world since 2007. Broadly Friedmanian assumptions concerning the scientisation of money and motivating 'financialisation', including the attempt to convert uncertainty and the open-

---

member of a society, almost always yields something less subtle, less reliable -- less (we might almost say) *scientific*? -- than the deliberations and understandings of those who are its 'subjects' (objects).

16 Such 'competence', as Rush Rhees and Adam Phillips have emphasised, is of course open-ended: it shades up into *excellence*(s). Ultimately, there sort-of is something it is to be an expert in human studies: namely, to be a great(-hearted) human being … There is no straight analogue for this, once again, amidst natural scientists. Being a great natural scientist is less closely correlated with being a great human being.

endedness of human action and conceptualisation into calculable (and thus financialisable) risk, are partly substantively responsible for that crisis.

Finally, 2.5 took us onto the alleged terrain of 'Cognitive Science', including along the way some related reflections upon neuroscience, upon sociology, upon psychology, upon psychopathology, and upon philosophy itself.[17] My overarching aim in 2.5 was, drawing once more upon Wittgenstein, to deflect what I take to be the most common underlying motivation for both scientism and irrationalism in the 'human sciences' (and in philosophy): namely, the recalcitrant 'phenomenon' of consciousness. I tried here to dissolve away the attractions of the self-contradictory Cartesian impulse to model the mind ('the mental realm') on the spatial and bodily ('the physical realm') while taking it to be a totally different kind of …'thing',[18] an impulse which I think still very much underlies projects as apparently-diverse for example as Tom Nagel's and John Searle's. My therapeutic aim is as far as is possible to set to rest the boggle at consciousness which causes so many fatally-flawed research programmes, and thus to undermine at base the felt *need* for a science of consciousness and (more generally) for a 'science of the mind'. There is of course a perfectly good place for neuroscience, and we have to *look and see* what 'Cognitive Science' succeeds in coming up with, rather than pre-judging it;[19] but there are good reasons to be wary of – to be guarded against over-hasty conclusions from – much of what it comes up with, given the conceptual unsteadiness of its foundations.

What 2.5 made explicit, drawing together threads from earlier Sections, is one aspect of why my book is likely to infuriate many academics, especially those attracted to investigating the methodology of the sciences (for such people are probably likelier than the average person to be impressed with science, and thus are at grave risk of being inclined to presume that their *own* approach should in some way be reflective of normal scientific approaches): that, in the sense of this word most common in English-speaking philosophy, my book has surprisingly little by way of *arguments* in it. Yes: the therapeutic approach is one that aims to reorient, to elicit new possibilities, to offer warnings: *not* to produce supposed chains of reasoning that must bring all to the same, quasi-factual, conclusion. If I am right in thinking that the approach manifested in this book is by and large

---

17 One comes to see in fact that the divisions between the various named subjects subject to investigation in Part 2 of the book is to some extent artificial, in the sense that, while each subject *must* be investigated in its own specificity (compare our discussion in the Interview, below, on this topic), it is nevertheless *natural* much of the time to move frequently one from to another (as I do), as many of the same morals and lessons apply *multiply* across sociology, the 'social studies', psychology, economics, etc. .

18 Cf. *PI* 339.

19 And, as the reader may by now have gathered, I am in fact a big fan of some 'second generation' Cog.Sci., such as aspects of the work of Lakoff and Johnson; and of course of most Neuro-Science, as explicated for example by McGilchrist and by A.J. Jacobson.

ultimately a 'right-brain' approach (in McGilchrist's sense), then this is inevitable. The detailed scrutiny, the theory-building, the fitting into pre-formed categories of the 'left-brain', is not the procedure of a Kuhn, or a Wittgenstein, as is crystal clear to one ready to read with attention the Preface and the Introduction to *SSR*, and the Preface to *PI*. The intention of this book has not been to produce a series of arguments that compel assent, but to offer to one interested in receiving it some aids in considering the possibilities:

- that science itself ought not to be reduced to the kinds of procedures that the left-brain is comfortable with (i.e.: extraordinary science can be something genuinely new),
- that the 'human sciences' ought not to be reduced to being sciences at all,
- and that instead there could be a different basis for intellectual human encounters with the (human) world.[20]

Still it will be claimed, as a last redoubt against the considerations that I have adduced here, that it is *I* who am being *a prioristic*. How can I say ahead of time that there cannot be a science of *x* (where *x* is some social or human category)?

Well, then, as I've asked more than once in the present work: must we consider the possibility that there might be a science of morals? A science of abstract objects? A science of things beginning with the letter 'e'?

The question is: *who is the onus on* to show that there can be or is a science of *x* (where *x* is society or something else seemingly thoroughgoingly human). I submit that the onus is on those whom I am questioning in Part 2 of this book to demonstrate an intelligible sense in which there can be a science of *x* in this case, just as in the above cases. It is not me but my detractors, the 'mainstream' who have the orthodoxy of academic structures on their side (e.g. There are far more Faculties of 'Social Science' than of 'Social Studies'), who are being *a prioristic* and *dogmatic*: *For they insist*, overtly or covertly, *that there is only one legitimate method of human inquiry* or knowledge-acquisition ('science'),[21] while I allow and suggest that there may be several, or even many. They are already convinced, before any discussion begins, that science is the alpha and omega of epistemic respectability; whereas I am open-minded. They are convinced that, to be worth

---

20 How would our disciplinary geography look different if we adopted my reluctance to think of our best social knowledge as a form of scientific knowledge? That is too large a question for here and now; it is one I hope to return to in future work. The final footnote of this Concluding Summary indicates one direction in which that difference should be pursued; and I'll add that an intriguing and promising proposal, one I have *some* sympathy with, is made in the second half of Flyvbjerg (2001).

21 This is, I think, true of Chomsky (and like-minded philosophers and linguists) as regards the possibility of 'knowledge of language' and with regard to those other areas of inquiry to which they believe the same 'restriction' applies. See my essay on Chomsky forthcoming in my *A Way with Paradoxes, after Wittgenstein* (Lexington).

anything more than just a collection of anecdotes, social study must be social science; I keep looking for whatever social study or 'human science' actually is and can be.

The rhetorical move of those who insist on their reasonableness by suggesting that *I* am being somehow dogmatic and *a prioristic* in 'refusing' to allow that there might be a science of *x* for any value of *x* is finally turned on its head. Rather than allowing 'the scientific method' to elbow everything else aside, the dangerous ideology of *scientism* is what needs gently elbowing aside, if anything does. We should, as Gordon Baker's Wittgenstein urges, consider *other possibilities*.

And: We should *not* impoverish, we should *not* crudify.[22] We should seek out, as Wittgenstein put it (see once more the epigraph to this Concluding Summary) the 'disregarded', and not merely the 'scientifically' 'preferred'. As Nietzsche remarks, in explicating what he called *The gay science* – and perhaps this will be a good remark with which (open-endedly, as it were) to bring to a 'close' the present work – 'Above all, one should not wish to divest existence of its *rich ambiguity*'[23]…[24]

---

22 As Geertz has pointed out (see e.g. p.40 of his (1995)), the risk of the simplification (away from quiddities and specificities) that science effectuates, in the human domain, is always that it does *not* clarify. The concomitant risk, worse still, of crudification or impoverishment is of course the central risk that Wittgenstainn and Winch and ethnomethodology are determined to highlight, and that motivates really all of Part 2 of the present work.

23 Emphasis in the original; (1974), p.335 (Section 373). Cf. also his lovely remark (at p.182 of his (1968)) that '[a]ll the problems of politics, of social organisation, and of education have been falsified through and through…because one learned to despise 'little' things, which means the basic concerns of life itself.'

24 If in the end Part 2 of the book convinces, then what next? Where else might Wittgenstein's therapeutic sensibility be usefully applied? Well, in the Lecture-Transcripts that open this book, I speak a little about the importance of thinking about the status and nature of the 'environmental sciences', that clear and yet highly-diverse hybrid of 'human sciences' and natural science. But that topic did not enter into the main body of the book. It deserves a book to itself.

For the inter-discipline of 'Environmental Science' is hugely-important in the contemporary world, for reasons that need no rehearsing: anyone with a basic concern for life or living or the future, providing that they are possessed of a modicum of knowledge about the state of the world, can now understand this.. Policy-implications for it outstrip in import most of those generated in the present work (with the possible exception of those concerning economics: see 2.4, above).

However, there is a problem. For it's often tacitly or sometimes explicitly held, including by impressive major figures in the field such as van der Bergh or Gowdy, that the successfully-claimed scientificity of this field is enough to enable it to answer all important questions about our environment. It is frequently deduced from this that questions as to what to do to preserve a liveable or tolerable or sustainable ecosystem are answerable with exclusive reference to Environmental Science.

Moreover, it is also often tacitly held that 'the humanities' can or ought to remain essentially intact as they are, even after taking seriously our ecosystemic placement in the

world. I will argue, following Roszak's epochal (1992), that this assumption is…no longer sustainable, and that taking this seriously radically recasts what the humanities and the human sciences can be (including by thrusting forward the claims of animal experience as needing to be taken much more seriously).

My argument in the book that succeeds the present work will then be in part that terms such as 'sustainable' (and 'ecology') are inevitably deeply-morally/-evaluatively inflected. There cannot be any purely biological definition of sustainability; and an ecological definition of it (or indeed a questioning of it) will always be or involve a moral claim, an ethical decision, a substantive evaluative position, and one which will have revisionist consequences for the humanities and the human sciences (The situation there will not be left 'as it is'…). There is a danger in the ruling assumptions of Environmental Science (and of 'Political Science') of a kind of technocratic prejudice. I believe that this technocratic prejudice can potentially be extirpated through an ethical, philosophical, methodological reflection.

I will investigate (i) the very limited extent to which environmental science can indicate courses of action without a *commitment* to particular human (though *not* of course necessarily anthropocentric) values, and (ii) the very great extent to which it is possible to think a broadly Wittgensteinian approach to endeavours that are fundamentally and ineradicably value-laden (such as environmental science) *without* needing to be committed to any troubling Idealist metaphysics. The Idealist interpretation(s) of Winch and Kuhn and of Wittgenstein himself, as we have seen in outline already in the present work, yields to a humanly or deflationarily naturalist interpretation. The harmlessly hermeneutical approach to the natural world which Kuhn argued is unavoidably implicitly present in the paradigms of natural science is implicitly present in the paradigms of environmental science. The key *difference* is, I would argue, that questions of value (and *not* moreover only questions of scientific values, (truth, simplicity, beauty etc., which Kuhn of course argued are ineradicable in science – see 1.4 above)) are ineradicable in the environmental sciences, and that this puts a limit on the extent to which they can truly be sciences, and places them partly among the so-called 'human sciences', which are best in the end *not* best thought of, according to the line of thought I have endeavoured to make credible in the present work, properly as sciences. Ecology as a field is human in a way that biology as a field is not – but what it is to be human must itself be rethought …

If the methodological clarity that I hope may have emerged from the investigation in the present work of what happens when Wittgensteinian therapy is set loose among the natural and the human sciences is genuine, then it will surely be of some value in thinking ecology (and ecological economics, and so forth), too. Thus my next book-project, a successor to this one, will be a Wittgensteinian investigation into the foundations of and the nature of the 'environmental sciences'.

Rupert Read: Interviewed by Simon Summers

S: Very few professional philosophers and young researchers would deny Wittgenstein's vast influence on our subject; and perhaps his greatest acknowledged influence in the analytic tradition, according to a fairly-well accepted interpretation, was on the Oxford school of 'ordinary language philosophy'. The influence of that style of philosophy has waned, though, and many would argue that research programs developed in recent years have led to considerable philosophical progress, in fields such as the philosophy of science, the philosophy of the social sciences and the philosophy of language. One shared aspect of these research programs might be said to be a return to *theorising*, and to the search for *explanations* of phenomena, rather than to the descriptive, clarificatory or therapeutic aims of Wittgenstein and the Oxford school. So, perhaps due to this reorientation, in today's philosophical environment, Wittgenstein's work is often marginalised, ignored, or even treated with open hostility. To paraphrase an earlier formulation of your own, how do you respond to the serious charge that Wittgenstein is now, with the perspective afforded by history, 'bordering on the insignificant in light of the wider picture – of the progression of our subject – that we now have?' (Hutchinson and Read, 2005, p: 432).

R: Well, indeed, and, as explained in that article: my work goes against the current. McGilchrist's work, as well as Wittgenstein's, greatly helps one to understand just *why* the current is so strong. Understanding this, in turn, can greatly help the would-be methodologist of the sciences.

A basic point here would be to dispute the picture of philosophy itself as progressing, in the manner of a science. (I indicate reasons for why I dispute this at various points throughout the book, such as in 1.2, 1.3, 2.1, 2.4, 2.5 and the 'Concluding Summary'.) For Wittgenstein, it is a fundamental error – and, one might add, a gross lapse of taste – to think that philosophy itself is or should or could reasonably aspire to be a scientific discipline.

By the way, Ordinary Language Philosophy has itself been grossly-misunderstood and traduced. At its worst (e.g in Flew, or in certain weak moments in Strawson), it deserved exactly the condemnation and obscurity it has had; at its best (e.g. in Ebersole, in Strawson at his best, in Austin at his best), it accords beautifully with the 'New' (resolute, therapeutic, Cavellian, later-Bakerian) reading of Wittgenstein that I and others champion. 'Ordinary language' is, for us, not a body of law nor a policeman (but nor is it simply a sociological set of facts about usages): it is not in that way normative, a resource, something that can

be looked to as an 'external' standard. Rather, it is simply: language. Everything that is not the nothing that a failure to mean or to make sense is. It is simply us (competent speakers performing, as we all do every day) meaning what we say. 'Ordinary language' offers only a call to reason, or a claim of sense: A call to us to return to ourselves, to be honest, to ask ourselves whether we are really on balance willing and wanting to use such and such a word in such and such a way. This is the authority of philosophy, the only authority that it has: the authority that each one of us has as a user of the language.

I am thus very sceptical of the alleged progress of philosophy in recent years. Do you think that there are good genuine examples of such 'progress', Simon? If so, I'd be keen to hear what they are.

S: I think a good candidate is the relative clarity which we now find in philosophy of language, following Grice and others, between the areas of syntax, semantics and pragmatics, and some of the excellent work being pursued in these fields. The study of syntax is now treated as an autonomous discipline, in large part due to Chomsky. You wanted to include a chapter on Chomsky and linguistics in the book. I was resistant to this, so I feel I owe you (and our readers) an explanation as to why! It seems to me that there is a clear difference between the *scientism* that the present book interrogates, and the *science of language* that is associated with the work of Chomsky and other linguists and semanticists working at the interface between syntax and semantics. Actually, it seems to me that Wittgenstein and Chomsky share some common ground; firstly, in their opposition to the construction of so-called 'ideal languages' to remedy the putative 'imperfections' of ordinary languages; and secondly, in their opposition to the notion that such formal calculi capture all that is essential to what Frege called 'conceptual content' and what might today be called 'semantic content' or 'meaning'.

Chomsky has always maintained that sentences and words don't carry their full 'meanings' with them. Arguing against 'meaning theories' such as those proposed by Dummett and Davidson, he has written that "[i]t is rather ironic that these moves should be presented as in the spirit of the later Wittgenstein, who constantly argued against the practice of constructing artificial concepts, divorced from ordinary usage, in defence of certain philosophical doctrines" (Chomsky, 2000: 51). This remark is still deeply relevant to much contemporary philosophy of language.

I would not wish to overstate the common ground, though. I agree with Chomsky that the scientific study of the syntactic structure of natural languages, operating with the assumptions and hypotheses developed within linguistic theory, might well contribute to understanding aspects of what might be called 'meaningfulness'. I accept, of course, that this is something many Wittgensteinians (including you?) might wish to question, or even deny outright.

To return to the question of progress in philosophy, relating it to the particular case of the philosophy of language and what I have said above, this remark of Austin's seems to me to be particularly to the point:

> In the history of human inquiry, philosophy has the place of the initial central sun, seminal and tumultuous: from time to time it throws off some portion of itself to take station as a science, a planet, cool and well regulated, progressing steadily towards a distant final state. This happened long ago with at the birth of mathematics, and again at the birth of physics … Is it not possible that the next century may see the birth, through the joint labours of philosophers, grammarians, and numerous other students of language, of a true and comprehensive *science of language*? Then we shall have rid ourselves of one more part of philosophy (there will still be plenty left) in the only way we ever can get rid of philosophy, by kicking it upstairs. (1956/1979: 232)

I think that we are closer now than 50 years ago to the science of language which Austin (over 50 years ago) is alluding to here. It is important to note, though, that the explanations of psychological phenomena sought by linguists in the generative tradition are intended to be *partial* characterisations of the human mind; they are not theories which seek to capture *all* that is essential to meaning, or to verbal communication. To seek such a thing would, I think, indeed be a form of scientism, and Wittgenstein's reminders to this effect remain as powerful and relevant as ever.

R: Thanks Simon. I certainly agree that there are positive points of contact between Wittgenstein and Chomsky, and have consistently found so when engaged in discussion about these matters with various colleagues at UEA and elsewhere. One of those points of agreement concerns the hubris of the quest for a total science of communication or meaning. I am however not as 'optimistic' as yourself and as Austin was in the final years of his too-short life about the prospects for a 'science of language' (or: of syntax). I have already sketched why, in my previous publications on the subject, and so I won't go over that ground here. (Additionally, I take the savage critique of Chomsky's programme in Lakoff and Johnson's *Metaphors we live by* and *Philosophy in the flesh* to have been very effective.) What I will be arguing in the piece on this topic that will appear in my forthcoming book, *A Way With Paradoxes after Wittgenstein* (Lexington, 2012), is that the 'poverty of the stimulus' argument is equivocal – because Chomsky never really clarifies what a *rich* stimulus would be – and that there is an effort in Chomsky's approach to language to take up a stance 'external' to language in order to be able to scientise it – but that such an effort is (for the kind of reasons writ large in *The New Wittgenstein* and also I think in Travis's and Lars Hertzberg's work emphasising how context goes 'all the way down') nonsensical and self-defeating.

I don't in the end believe that the separation off of syntax from semantics and pragmatics – a *sine qua non* for Chomsky's programme, for his vision of a science of (part of / one aspect of) language – can be accomplished. Pure syntax: there is no such thing. Without meaning and context, there isn't any language to try to effectuate a science of. Until a context is found for 'Colourless green ideas sleep furiously' (as has been done), it is, or rather was, no better off than 'Green

colourless furiously ideas sleep'. Sentences (*Satze*, would-be sentences) are segmented with regard to meaning, use. It is a compositionalist fantasy to suppose that we know how to segment sentences grammatically prior to any potential use of them.

S: Thanks Rupert. One difficulty here is that I don't understand your claim that there is 'no such thing as pure syntax', if the term 'syntax' is understood as used in syntactic theory. A syntax is a grammar (identified with a system of internal rules which generate linguistic representations) acquired by every competent speaker/hearer under normal conditions of development. There is clearly such a thing, though the phenomenon is not well understood, although it is arguably *much better* understood than fifty years ago. Syntax in this sense is not identified with the properties of 'sentences' in the world, but with the system which generates a discrete infinity of them. To deny the existence of such a thing is to deny the existence of competent language users.

Linguists in the generative tradition depart from many philosophers of language who subscribe to the assumption that any inquiry into syntax needs to be informed by antecedent 'semantic' notions of meaning, understood as a set of relations between utterances of 'would-be sentences' and their worldly 'referents' or 'meanings'. Rejecting that assumption is part of the idea behind what is often labelled the 'autonomy of syntax' in linguistic theory; with this methodological principle of autonomy in place, it is perfectly consistent to treat 'colourless green idea sleep furiously' as meaningless (or 'nonsensical', if you like) in the semantic sense, whilst being struck by the fact that as English speakers we intuitively recognise the sentence as *well-formed*. We can, for example, negate the sentence, transform it into an interrogative, emphasis or otherwise modify some aspects of it ('they *do* sleep furiously') and perform a range of grammatical (syntactic) operations on it. These options are unavailable for 'Green colourless furiously ideas sleep.'

These points of contention aside, let's return to your claim that 'there is no such thing as pure syntax'; a claim to the effect that 'there is no such thing as *x*' does not, on the face of it, sound very therapeutic. Can such a seemingly categorical assertion have philosophically therapeutic value, and if so how?

R: First-off, nothing of course could be further from my intention than to deny the existence of competent language-users, in the 'ordinary' (non-Chomskian) use of those terms. Exactly to the contrary: my concern is only that a Chomskian rarefied sense of the term "competent" does no justice to our actual competence/performance, which I suggest is/are inextricable, and 100% undeniable, and not in need of any scientific 'foundation' or theorisation.

In terms of the more interesting dispute concerning the possibility of a science of syntax: Note that I spoke of "'pure' syntax". I submit – I offer you the suggestion – that your practice and that of Chomskians generally is already enough to show that you yourself don't believe in any such thing … even though you are and must

be theoretically *committed* to doing so, and explicitly avow belief accordingly. Thus I aim not to essay a categorical assertion, but rather, therapeutically, to get you to question whether you really on reflection want to say the things you say. Let me explain:

'Green colourless furiously ideas sleep' probably can, with enough effort, be parsed. It could be a line in a poem with a stranger form than that of John Hollander, 'Coiled Alizarine':

> Curiously deep, the slumber of crimson thoughts:
> While breathless, in stodgy viridian
> Colorless green ideas sleep furiously.

It would perhaps have much the same meaning (or use) as the last line of this poem, only it would be harder to parse. Why? Because its form is less familiar to us. But in speaking of 'form', have I not admitted the existence of 'pure syntax'? No, because all that 'form' amounts to, in such a context, I'd suggest, *is a matter of such taken-for-granted familiarity.*

When we see 'Colourless green ideas sleep furiously', we model it on sentences which resemble it, with which we are already familiar (and we have much less familiarity with sentences resembling 'Green colourless furiously ideas sleep'). Notice that, roughly, this necessitates us understanding-in-practice 'categories' such as (very roughly) adjective, noun, verb and adverb. But I would suggest to you that such understanding-in-practice *already involves a degree of semantical understanding*. Very roughly, for example: verbs involve a process, in a way that nouns do not. (Though even to say this is pretty misleading, in that it implies that we could have verbs or nouns by themselves, which, except at the limit (in 'one-word sentences', etc.), we cannot. It courts what I regard as fantasies of compositionalism. But let that pass for now.) As, in their different ways, Merleau-Ponty and Lakoff-and-Johnson argue: at the most basic levels of thinking/speaking, there is meaning, and lived body, not just 'pure form'. (Our most basic uses and understandings of language (and of the world) probably spring from and are involved in our brains and in our biological and social development with our motor experience and our understandings of that, etc. .)

Why should we not succeed in parsing 'Green colourless furiously ideas sleep' by treating "furiously" as a noun, and "ideas" as a verb, etc.: i.e. modelling our understanding of this sentence *closely* along the lines of the successful effort we may already have made with 'Colourless green ideas sleep furiously'. Why would you not want to do this? We could do this. I put it to you that the real reason why you do not want to is that *you* want to say that 'Green colourless furiously ideas sleep' is simply meaningless word salad (That is not something that *I* want to assert; it is something *you* assert; just as *you* and not I assert that 'Colourless green ideas sleep furiously' is nonsensical even though syntactically well-formed. For me, there are not two types of nonsense: well-formed, and ill-

formed ('word salad'). There are simply: some strings for which we have not yet found a use). But why doesn't 'Green colourless furiously ideas sleep' mean anything, if it doesn't; why do you regard it as something like 'word salad'? Because you have assumed that the individual components of it carry meanings with them: that they occupy unproblematically individually identifiable lexical categories. They are words which play one role rather than another in sentences. But that itself surely means that you don't really believe that there is such a thing as anything that would on reflection be worth calling "pure syntax": because the examples by which you seek to demonstrate it already tacitly smuggle in semantics. Because if for example 'ideas' could be read as a verb, as (roughly) processual, then there would be nothing to stop 'Green colourless furiously ideas sleep' from being read as no worse off than 'Colourless green ideas sleep furiously'. You tacitly take 'ideas' as *meaning* what it in fact generally does, as playing that role in the language, and that, I submit is what leads you to see a difference between Chomsky's famous sentence (if such it is) and that that I have compared with it here.

(Actually, as I suggested in my answer to your earlier question, I think that the situation is worse than this. I think, as Travis and Hertzberg and Lakoff-and-Johnson suggest, that you/Chomskians tacitly read back into syntax what you are only entitled to through *pragmatics*. We are in danger of ignoring this possibility, in our discussion here, because our discussion is at times dangerously abstract, sometimes completely abstracting from actual cases where someone might say or write 'Colourless green ideas sleep furiously' or whatever.)

It is in virtue of my claim being about your practice that my claim is a therapeutic one. It is up to you now to respond to my suggestions, with integrity, and helping me to understand possibilities I may have missed: and so the dialogue goes on. I do not dogmatically assert that "There is and can be no such thing as pure syntax – no matter what one takes "pure syntax" to mean". For clearly, given enough leeway to re-interpret what "pure syntax" means, such a dogmatic assertion is implausible / false / pointless. Rather, I suggest that you are hovering in the way you use the term '(pure) syntax'. You need to decide whether to use it one way or another. I suggest that the alleged autonomy of syntax is actually in question 'even' from within the totality of the perspective of my interlocutors, such as yourself.

S: Interesting; I'm not sure now who is asking the questions, you or me! In any case, moving on to a different (though not unrelated) topic: Your attempted 'therapeutic diagnosis' just now raises a question concerning the overall aim of the book, characterised in the Preface as "intellectual liberation, satisfaction and clarity", by means of a therapeutic and dialogical method, as opposed to presenting arguments. Could you comment upon how the book allegedly achieves this aim without recourse to traditional argumentation?

R: Yes. If one looks at the methodology of science, one finds elements such as hypothesising, theory-building, and 'argumentation'. It seems to me that none of

these things are particularly characteristic of philosophy – at least as Wittgenstein wants us to understand philosophy. Wittgenstein is in search of a mode of philosophising that has integrity, a conception of philosophy in which it is not reducible to science (or to maths). He asks what philosophy can then be.

Philosophical thinking is of course sometimes close in some respects to what goes on in science at moments of rupture, at moments involving extraordinary reflection and the 'conceptualization of new possibilities' as Kuhn describes it. But the fundamental 'nature' of philosophy – the fundament of the philosophical method – for Wittgenstein, as I understand him, is, roughly, 'therapeutic'. It is not about trying to build theories, and to test theories by experimentation, or to argue for one theory and oppose others. What (therapeutic) philosophy is about is reflection upon the *presuppositions* that one oneself makes in any such activity.

In terms of thinking about (what we take to be) science, it would be for instance about reflecting upon what Kuhn calls 'our paradigm'. This is something that scientists normally virtually never do, because they virtually never *need* to do so (they *look right through* their paradigms, they are *transparent* to them, in the kind of way focal to Lakoff and Johnson's important point about how we tend to and generally *need* to look right through our metaphors); but this is the quintessential activity of philosophy, and this is necessarily 'therapeutic', because it involves making figural and even putting into question those presuppositions that one holds most dear, and perhaps coming to awareness of some presuppositions which one hadn't even *noticed* that one held to at all. This is one of the inspirations that Wittgenstein takes from Freud – a concern with those aspects of one's thinking and one's presumptions that are 'unconscious'.

So, the process is a therapeutic one in the following way: one is trying to help oneself, or one's reader, to gain clarity, by subjecting to scrutiny what are potentially – and perhaps in some sense unconsciously – one's most fundamental assumptions. This is analogous, in relevant respects, to the method of the therapist helping a client to bring to consciousness certain background conditions or assumptions which they may not even have been aware of. A crucial feature of this analogy is that unless the client is *willing* to acknowledge what the therapist is trying to draw attention to, the therapist ultimately has no good grounds for asserting that it is in fact there (This is what I was trying to do, admittedly without much prospect of short-term success (and that is usually true: philosophy, as Wittgenstein held, is more or less *necessarily* a slow cure…), in the earlier part of our exchange, concerning Chomsky etc.).

In other words, there is no hierarchical position of authority or expertise here – I am *not* saying, as a Wittgensteinian philosophical therapist, that 'I know what your/whoever's problems are, and I (know I) can solve them'. That would pretty much return us to the pointlessness of trying to insist on a definition, something *I* have insisted throughout the present work. No: It is rather an attempt to enter into a dialogue with central aspects of and assumptions underlying the disciplines which one is attempting to better understand, and to see if one can find a way of appreciating, interrogating and perhaps (verbally) rearranging them, in such a way

that their own practitioners will come to realise that what is being said is right. Of course, if the practitioners come to no such realisation, one has no good grounds for asserting that it *is* right. This is one of the important ways in which *clarity* requires *charity*, as Peter Winch, an important figure of course in Part 2 of the book, clearly understood. The therapy which one is trying to practice has to be a genuinely dialogical and collaborative enterprise.

S: So, in therapeutic philosophy, as you understand and practice it, the onus is on the reader – or interlocutor – to attain a position of self-awareness and clarity regarding the impulse to raise particular philosophical questions, or to see things in a particular way, or to call things by particular names, and so on, by means of a process of self-interrogation. The aim of such a self-interrogation would be liberation from the view that such and such *must* be the case. Is that a fair characterisation?

R: Yes. It's a question (in Kantian terms) of coming to a position of autonomy rather than heteronomy, by virtue of (in Wittgensteinian terms) no longer being held captive by pictures which insist that such-and-such *must* be the case. Such pictures may, for example, suggest that (a particular) human science must be modelled on the natural sciences in order to be worthwhile, or to yield interesting or fruitful results, to the exclusion of other possibilities. I explore some of those alternative possibilities in some of my previously published work, and most notably in Part 2 of this book.

It is implicit in what I am saying here that the view of therapy taken here is as a process which enables a person to help herself gain clarity, increase self-understanding, and achieve greater freedom. This is in important respects a (transformed) remainder of the epistemically-optimistic Enlightenment project, once some of the darkness-triumphant of that project in its actually-existing form has been overcome. Of course, the way in which therapy (and philosophy-as-therapy) can increase and deepen our knowledge is then not at all the same way in which scientific investigations enlarge and deepen our knowledge. As for instance I aim to bring out in 2.5, above: therapeutic epistemology is irreducibly an affective (as well as an intellectual) process.

S: You have been speaking there about the second part of the book. But just how is your Wittgensteinian reading of Kuhn in Part 1 supposed to relate to your project in Part 2, the investigation into some of the putatively human sciences?

R: Natural science is our paradigm, our prototype of science. What Part 1 proposes, building upon my previous work on Kuhn, is an account, roughly, of our *concept* of science, of science as we actually find in our best examples/cases, namely in the *natural* sciences. This involves reflecting on the role of extraordinary/revolutionary science, as figure, against the crucial ground of what Kuhn calls normal science. This exercise is a matter of inviting the reader to 'construct' a

conception of science with which she is satisfied. In other words, the approach taken here is (or aims to be) once more a thoroughly *therapeutic* one. This hands the power over to the reader. In Part 2, the reader is invited to take that concept – or perhaps better that *conception* – of science which the reader will have grasped, and to some extend have *constructed* for herself, and to think it through in relation to the cases which are there presented. The cases invite reflection upon the relevant similarities and dissimilarities between the hard cases (for my case) of (chiefly) psychiatry, economics and 'cognitive science', on the one hand, and our paradigm or *model* of what science is, on the other. So, the Wittgensteinian reading of Kuhn in Part 1 offers an 'object of comparison', in relation to which the cases considered in Part 2 can be assessed, such that a decision can be reached regarding how they are – or are not – relevantly similar.

S: The account of science you offer in Part 1 is, you have said, not intended as a definition, nor a set of criteria demarcating science from non-science, but rather as a *reminder*. Could you say what you mean here by a 'reminder'?

R: Thanks for this question: because we need to be very careful when considering how Wittgenstein uses the term "reminder". It isn't supposed to be a reminder *of* anything, or at least not a reminder of any 'thing'. What one is 'reminded' of relates to the nature of fundamental concepts and conceptual categories, which one may be inclined either to lose sight of, or to find oneself standing in a confused relation to, in the course of one's linguistic practices, activities and reflections in (or on) a given discipline, including the discipline of philosophy. But these concepts/categories do not get imposed; they are always being reconstructed. They just are not anything that can be simply stated. I'll say that again: What one is reminded of is not something that can be simply stated, like a thesis. (*This* is *why* I've stressed in the past that one isn't reminded by Wittgenstein (or indeed by Plato/Socrates) of any *thing* at all.) It would be, in my view, quite pointless to come up with an 'authoritative' criterion of what science is, or what it is demarcated from, because such a criterion would only – at best – fulfil the function of *itself* being a kind of 'object of comparison'. It is essential for *the reader*, and ultimately for the practitioner of any given discipline, to assess for themselves what their relation is to the matters under discussion. If I were to lay down a criterion of what constituted science, or scientific practice, anyone who was not already comfortable with – or broadly sympathetic to – such a criterion would simply reject it. If I lay down a criterion for science, I am attempting to police the language, and to say: 'This is how you're allowed to use the word 'science''. This is a completely pointless manoeuvre, and I think that some philosophers, Peter Hacker for example, make a fundamental mistake when engaging in such methodological prescription via their reading of the later Wittgenstein. It has been a baleful influence of 'Wittgenstein' (I use the scare-quotes advisedly, because of course it wasn't *Wittgenstein* who actually thought this, but only his legend, the ghost of his ghost) to seem to license such word-policing. That is why my approach to applying Wittgenstein's

philosophy aims to leave this 'influence' decisively behind, difficult though that is to do.

What is *actually* needed is for people to understand and to come to terms with the way(s) in which they themselves are happy to use, or not to use – to apply, or not to apply – a term such as 'science': just as in *PI* (especially in sections 1-80) Wittgenstein suggests that the reader has to go through such a process in relation to the term, or concept 'language'. What one has to do, according to Wittgenstein, is to (get the reader to) think about / reflect on / work through / decide the conditions under which you/he (the reader/thinker/subject) is happy to apply or withhold the term 'language', thereby coming to be reminded of what language 'is', in relation to the role it plays in, say, our linguistic activity and our conceptual reflection. This same method is applied in considering the term 'science' in the present book – the process of reflection which the reader is invited to participate in is *essentially* a dialogical and therapeutic one. This is why any prescription on my part regarding what is, or ought to be, regarded as 'science' would be a pointless and, worse, a counterproductive way to proceed.

Rather, I *ask* a Chomskian or a Friedmanian or whoever: Pray explain to us how this that you do actually does have the character of our paradigms of science, or consider reflecting on whether it actually is sufficiently continuous therewith (without distorting your subject-matter) to merit being so-called. There is a kinship I think here to the situation described by Wittgenstein in *PI* 308:

> How does the philosophical problem about mental processes…arise? – The first step is the one that altogether escapes notice. We talk of processes…and leave their nature undecided. Sometime perhaps we shall know more about them – we think. But that is just what commits us to a particular way of looking at the matter. For we have a definite concept of what it means to learn to know a process better. (The decisive move in the conjuring trick has been made, and it was the very one we thought quite innocent.)—And now the analogy which was to make us understand our thoughts falls to pieces.

As Phil Hutchinson writes, commenting (at p.96 of his piece on 'Thinking and Understanding' in Jolley (2010)) on this passage:

> [T]alking of mental processes is fine, so long as you either (a) acknowledge that you are employing the concept of 'process' in a new way, a way that does not necessarily imply continuity with nor draw upon your employment of that concept in other domains; or (b) you furnish us with a justification for your claim that the notion of 'process' that you are invoking in the term 'mental process' *is* continuous with previous uses of that term in other domains.

That is the kind of thing that I invite my interlocutor, my reader, to do.

S: Okay, let's consider in more detail the application of the therapeutic method which we've been discussing, focusing on two examples that you have already gestured at. In Part 2 of the book, you address Economics and Cognitive science, two disciplines which approach the 'human' in very different ways. Friedman's 'positive' Economics aspires to be a science, a *behavioural* science of cause-and-effect behavioral relationships. Cognitive science, on the other hand, concerns itself with human cognition, mental operations and structures. We are dealing with rich and variegated fields of enquiry here, where methodologies and assumptions differ greatly, even *within* particular fields or disciplines. It is extremely important that a therapeutic philosophy takes these differences into account and seeks to understand them on their own terms, isn't it? Do *you* take those differences into account?

R: Sure – though of course what I offer here in Part 2 of this book only scratches some parts of those surfaces. … For, whilst it seems to me that when one is engaged in these kinds of investigations, the same problems and questions arise repeatedly, one's investigations *have* to be 'retail' rather than 'wholesale', as my old teacher Richard Rorty put it; this is what Wittgenstein meant by saying that we needed to 'enrich our diet of examples' and avoid the philosopher's tendency to over-generalise. One particularly powerful example of this is scientism itself, which Wittgenstein considered to be a very influential form of such a tendency. So yes, the cases are different and multifarious, and they have to be looked at in their specificity. One of the things I am trying to do in Part 2 is to look at most (though not all: we have come only in this Interview to Linguistics!) of the allegedly *'strongest cases'* for claims to scientific status amongst the so-called 'human-sciences': cases such as Economics and 'Cognitive Science'. Each of the disciplines I look at each sees itself as having a strong claim to the honorific term 'science'. But yes, their credentials are different; the question of why people may want to use the term 'science' to apply in each of these cases, and how helpful or otherwise it is to apply the term – if it is to mean what it meant in Part 1 of the book – *has* to be examined in its specificity to particular cases. (This is one key reason why it is possible for someone such as yourself, Simon, to agree with me on Economics and to disagree with me on (some) Cognitive Science.

I hope that this is what I achieve in this book, though may others go much further. As we've already discussed, it is certainly not a question of saying: 'Here's what it would be for something to be a science – let's see if these cases fit.' Rather, we've hopefully 'reminded' ourselves, in Part 1, of what the term 'science' can usefully mean and why we tend to use it to describe disciplines with particular features, and in Part 2 we look at the individual cases on offer, and see whether those features that we ourselves have highlighted are present or not.

For example, in looking at Economics, we wonder about the extent to which the discipline has bought into a positivistic theory of what science is – itself a problematic conception, of course, relatively rarely actively present in, say, physics. We also wonder about the extent to which the kind of methodology we saw Kuhn

rightly focusing on in the natural sciences can be present, straightforwardly, in cases where the *humanity* and the *reflexivity* of the participants in the phenomena are themselves a central part of the phenomenon in question. It is both of these worries which I raise with regard to Friedman's extremely influential attempt to argue that Economics is a genuinely scientific discipline. Firstly, Friedman imports a dubious conception of what science is – a conception which is of little interest to most real scientists – at the outset; secondly, the phenomena with which the discipline deals and the way that human beings are able to, say, step back and *reflect upon* alleged 'laws' of Economics make a difference to the subject matter of the investigation, a difference which Friedman arguably fails to take even remotely seriously.

S: Playing devil's advocate for a minute: Why do you think that all this matters, except to academics? Why does it matter how Friedman gets his own subject wrong provided that the actual work he does *in* the subject works?

R: Well, the neglect of the difference that I just highlighted may have very serious consequences in political economy: The tacitly positivist presumptions of Finance Theory are, I believe, at the root of the financial crisis which swept the world and almost demolished the real economy, from 2007 to the present. The conceptualisation of money as a commodity, and the fantasy of being able to turn uncertainty into (quantifiable, ultimately commodifiable) risk, are ultimately consequences of Friedman-style thinking about 'positive Economics'. Moreover, insofar as Friedman's essay influenced many *on how they ought to pursue the practice of Economics* (and of business!), this is a serious matter, and we really can I think attach some blame to Friedman and say that his influence has now been proven to be a baleful one, upon our *world*, upon the actual jobs and livelihoods and bank balances of real people.

This is how the method I pursue in the book applies to Friedman, and Friedman's 'positive Economics'; but one needs, as you said, Simon, to reflect upon what might be an entirely different set of questions in relation to cases such as psychiatry, psychology and the cognitive sciences.

As regards 'Cognitive Science', one thing I would begin by saying is that we need to look and see how terms like "mental representation" are actually used; as for instance Anne Jaap Jacobson does. This is, of course, a quintessentially Wittgensteinian point: we can't assume that we already know what the term means, and too many so-called Wittgensteinian philosophers think they know what a mental representation is and think they know that there is no such thing, or that it is nonsense. That is not the right way to proceed. There has been some very interesting work indeed done in recent years by Jacobson on these issues, suggesting that the 'true roots', if you like, of the term 'mental representation' are better understood via the disciplines of neurology and neuroscience rather than via contemporary cognitive science (with its roots in Early Modern Philosophy, in Descartes et al). She raises interesting connections with ancient and medieval

philosophy in this regard, suggesting that Aristotle and some of his successors may help us to better understand the philosophical roots of the term; this may be more helpful than the appropriation of the term by (both allegedly 'Wittgensteinian' philosophers on the one hand (e.g. Hacker), and) linguists/cognitive scientists (on the other).

In Section 2.5 of the book in particular, I criticise *certain* tendencies in the Cog. Sci. area or 'paradigm'. *Some* work under the huge banner of 'Cog.Sci.' seems to me very worthwhile. As I've made clear, I am quite a strong admirer of George Lakoff's work, though it seems to me that his best work doesn't actually depend upon any controversial assumptions about the nature of mental representation. I'm inclined to think of much of Lakoff's excellent – and broadly Wittgensteinian – work (since his breakthrough book, *Metaphors we live by,* which of course, interestingly, is co-authored, as much of his work is, with a philosopher, Mark Johnson) as a kind of either philosophical or (sometimes, especially recently) thoroughly *applied* linguistics, in the sense that what he is doing is analysing sentences, expressions, and relations between sentences and expressions. The notions of cognition, mental representation and so on play a somewhat epiphenomenal role in his discussions of the metaphorical systems that we live by, in relation for instance to argument, politics, and the other cases which concern him (and me). So, 'cognitive science' is a very broad label for many different projects. *Some* of these, such as Lakoff's, actually fit quite unproblematically under a broadly Wittgensteinian heading, and don't require much debate, whether methodological or ontological, about the nature of mental representation. In other words: I think that Lakoff at his best needn't necessarily be thought of as really a 'cognitive scientist' *at all,* in the conventional sense of that term. I think he is (questionably) leaning on '(cognitive) science' for *authority*, but actually his (and Johnson's) central thinking about metaphor is basically a brilliant, novel form of deep linguistic/philosophical reflection, not requiring much at all in the way of theorising or hypothesising or experiment.

So yes, as you say, Simon, the term 'Cognitive science' covers a multitude of virtues and vices.

S: You suggest that the therapeutic value of a Wittgensteinian approach lies in the possibility of releasing us from certain 'pictures', pictures which we may be tacitly committed to, and which, as you put it, might give rise to 'incompatible forms of thinking'. Such incompatible forms of thinking might manifest themselves in, say, the economist's desire to model his activity on an assumed picture of natural science, whilst at the same time acknowledging or manifesting deep differences between his subject matter and the subject matter of, say, physics. Could you describe rather more what 'releasing' someone from such a picture might amount to?

R: Yes. This comes back to my earlier remarks about 'autonomy' as opposed to heteronomy and about 'liberation' (and by the way once again I think of Lakoff

as actually in this regard a *practitioner* of a Wittgensteinian project of liberation, a project both personal and cultural/social/political). One becomes aware that a particular picture does not exhaust all the possible ways in which one might proceed in addressing a given subject matter. This creates an awareness of alternative possibilities, as Gordon Baker emphasises in his later work. For example, one may start to think of the hermeneutic dimensions of a so-called 'human' or 'social' science. One may start to realize that there is something which is – in a certain natural sense of the word – *sad* about assuming that one's only model of epistemic excellence is inherited from the natural sciences.

More positively, one may begin to see that a way of giving a discipline its due 'respect' may be in allowing in to diverge from, or to have an independent centre from, the natural sciences. As I discuss in the first Section of Part 2, one may wish to emphasise the respect in which our *living* of human life – not in the hermeneutical sense of 'interpretive' life, but simply our *practice*(*s*) of life – is something which always necessarily involves the possibility for reflection or interpretation, even if that possibility is not (need not be) actualised. Such an emphasis provides a way of conceiving of a kind of *subject* matter which is fundamentally different to the subject matter – which is always an *object* matter – of the natural sciences.

Coming to awareness of these possibilities, as alternative 'pictures' – or at least as ways of overcoming or bracketing a picture which one inherited and which one perhaps was not conscious of – may be productive as a way of unleashing what we might call the latent 'power' of the discipline in question, enabling it to achieve its own potentialities. Also, crucially, this coming to awareness may bring us back to noticing the extent to which this unleashing has *already been achieved* in actual life (and this is something which the ethnomethodologists strongly emphasise, and rightly so). So, for example, we no longer assume that the person to turn to, if we want an understanding of a social phenomenon, would necessarily be a sociologist. To give a famous example: if we want an understanding of suicide, then the person to turn to might not be a sociologist, but a coroner. Somewhat similarly; is it always the case that an economist is the best placed person to comment upon economic activity? I would say that it isn't; it could be for instance that a businessman, a trade-unionist or an activist is better placed. One could construct similar cases across a wide range of social phenomena and activity.

S: Is there such a thing as a 'picture-less' perspective? Is this the perspective which you adopt or aim for in the book?

R: This is a great question, and an important point, one that I have often debated for instance with my colleague Angus Ross, in relation to Kuhn, and Wittgenstein. I certainly don't advocate a 'view from nowhere'; if that is what one's idea of a 'picture-less perspective' is, then it is not my perspective at all. One of Wittgenstein's fundamental ideas, in relation to philosophy and metaphysics, is that there is something fishy about 'Realism' and the various forms of 'Anti-Realism' which are reactions against it. Neither of these positions is what we actually desire/need,

but perhaps rather something between – or orthogonal to – them. If one is in the grip of a picture, and then one has that grip loosened, one has probably become aware that there are other possibilities, alternative pictures. That is often what made this loosening *possible*. What does one do in such a situation? Does one necessarily move from one picture to another? This is what Kuhn says one does, in the case of science; but it is rather different in the case of the kind of reflective activity that we are engaged in here: we are reflecting upon the *methodology* and *philosophy* of the natural *and* the 'human' sciences. Postmodernists will say that there is no alternative to having a theory, albeit perhaps of a very new kind. To me, as a Wittgensteinian, this is just another version of Anti-Realism, and as such does not escape the metaphysical impulse. (This is why Postmodernism is in the end fundamentally conservative and *un*-novel.)

S: So, you are resolutely opposed to Post-Modernism, and obviously to Relativism; but do you for example see any salient analogies between Wittgenstein's therapeutic methods and recent 'Continental' philosophy, including post-structuralist philosophical projects such as Derrida's 'deconstruction'?

R: Again, the key point of rupture between Wittgenstein and Derrida is that Derrida is pessimistically insistent that there is no escape whatever from the condition of metaphysics. (See 2.1, above, for a little more on this. Sadly, Deconstruction and Post-Modernism are in the end just as 'left-brain-dominated', just as addicted to 'theory', as most Analytic philosophy.) Whereas, by contrast, it seems to me that we ought to hold out the possibility that one can, as a philosopher, reach a condition of not being in the grip of any *particular* picture.

But, in the final analysis, I think it is 'as you please' – to quote Wittgenstein from *PI* 16 – whether one says such a thing or not. It doesn't really matter whether one says 'we attain liberation by not being in the grip of any picture' or 'once we come to see the possibility of various pictures, that is itself is a new meta-picture'. I'm doubtful that the latter formulation is a helpful thing to say, and I am sceptical of the compulsive 'Derridian' desire to insist upon it; but it doesn't really matter, as long as what is kept clearly in view is the perspective expressed in the first formulation, in the sense of being in -- and escaping from! -- the *grip* of, say, scientism, or a particular vision of how Economics or Psychiatry *have* to be, if they are be 'sciences'. If this is called a 'meta-picture', still its difference in *kind* from the picture(s) one inhabited or was gripped by previously is vital (Partly, because such a 'meta-picture' is knowingly held, freely acknowledged.). Freedom from particular pictures brings with it a kind of flexibility, grounded in an appreciation of (say) the variety of activities going on within a particular discipline, and an appreciation of the reasons for saying – or not saying – that the discipline has such-and-such in common with a scientific paradigm. There will never be a completely firm answer to the question 'Is this a science or not, in the relevant sense?' This is not the kind of question for which a yes or no answer can be profitably provided … but something can be learned from *asking*

the question of a discipline and being open to the possibility of finding similarities and differences, perhaps of an unexpected nature, in the exploration that follows.

What is crucial, and here I follow the later Gordon Baker, is that in philosophy, in methodological reflection, we don't get *gripped* by one picture, especially unawarely. Here, Wittgenstein (and also Buddhism) are terribly useful: It is not essential to have a 'pictureless' perspective (though it is troubling dogmatically to insist, as Derrida does on my reading of him, that there could not possibly be anything worth calling such a thing), but it *is* essential not to *attach* unhealthily, like an addict or a dogmatist, to one's perspective.

For those interested in following up further this kind of critique of Derrida, I recommend Chapter 2, section 5.4 of Hutchinson's *Shame and Philosophy*. If we had more time, I'd love to talk now about what I think is in fact the closest point of connection in recent 'Continental' philosophy to Wittgenstein: namely, the work of Michel Foucault. They share the idea that work in philosophy is really work upon oneself; they share the sense of the importance of enabling the reader to inhabit other possibilities than those which the hegemony of prevailing thought accustoms us to; I could go on. (Foucault and Kuhn, obviously, have important things in common too, partly through a common interest in / inheritance from Bachelard and other related figures in 'Continental' philosophy of science.)

S: Foucault was of course a profoundly *applied* philosopher. Could you then say a little more about how *Wittgenstein among the sciences* develops and builds upon your previous work in applying Wittgenstein's philosophy?

R: I see Wittgenstein as a philosopher with relevance across a vast range of human activity, phenomena, and concepts and conceptions. I think that Wittgenstein's potential relevance to the sciences, social studies and humanistic disciplines has not been enough explored yet. Some people have tried, but I think that, on the whole, these attempts have been somewhat unsuccessful. This book could be seen as exploring the work of a master and two emissaries, the master being Wittgenstein and the emissaries being Kuhn (in Part 1) and Winch (in Part 2) who I interpret, as in my previous work, as strongly Wittgensteinian thinkers. For example, in Part 1, I ask whether Kuhn can be interpreted as a 'Wittgenstein of the sciences' in relation to the philosophy and the methodology of the natural sciences. In Part 2, I think through a Wittgensteinian approach to methodology in allegedly human or social sciences, in domains where it hasn't been considered in particular depth. I try, therefore, to go *beyond* only those domains where Peter Winch does explicitly think through and apply Wittgenstein in this way, which I considered in previous work.

As I've said, I am not hugely bothered any more whether my reading of these 'emissaries' is 'right' or not. Perhaps Steve Fuller is right that I don't give myself enough credit for the (as he sees it) original philosophical position that I am developing in Kuhn's name. Perhaps Stephen Turner is right in his analogous thoughts about the way in which my (and Sharrock's and Hutchinson's) Winch

moves on from the actual historical Winch. Perhaps Jouni Kuukkanen and Angus Ross and Bojana Mladenovic are closer to getting Kuhn right in the end than myself and Sharrock are. I don't know. The real point is a philosophical one: My rendition of Wittgenstein's two 'emissaries' is either what they did say or roughly what they should have said … What I have done here, building on my previous books, is to give as therapeutic account as it is possible to give of Kuhn and Winch, and of a methodology of and for the natural and 'the human sciences'. I have developed a 'therapeutic' perspective on these areas, and have homed in on specific 'problem' cases (for me) such as economics, psychiatry, etc., as we've discussed: it doesn't much *matter* whether the perspective is mine or theirs. What matters is the perspective itself. (In the end, the same goes for Wittgenstein himself: Either what I have offered here is a way of seeing Wittgenstein among the sciences, or it is a way of seeing some of what Wittgenstein should have said about same. (Though I guess I am more fully confident in the case of Wittgenstein than in the case of Kuhn and Winch that I am broadly in tune with the quintessence of his thought.))

I do strongly believe that Winch and Kuhn have been misunderstood, and that this book in particular and my writing in general bring out an aspect of them that has been neglected. I strongly believe that they, like Wittgenstein himself, have been treated as theorists when they are not. And so I am delighted to have had the chance to offer a therapeutic perspective on the disciplines interrogated in this book, in their names; I am happy for the reader to judge whether the perspective in question is 'really' theirs or mine. Doubtless I 'idealise' them at times. Think of this as being an exercise in charity, to *counter-balance* against the decidedly uncharitable and simply uncomprehending readings of them which have tended to prevail previously.

In sum, this book is an attempt to think along the lines laid down by Wittgenstein, Kuhn and Winch, being as clear as possible about what it would mean to think about the natural sciences and the various disciplines addressed in Part 2 through a Wittgensteinian lens. Naturally, I believe that the results are useful, and that is why I believe that applying Wittgenstein where hitherto he has not been applied – at least, not well-applied – can be valuable. Of course, the reader will have to assess whether or not this is true, and whether the conception of the book has been fundamentally worthwhile.

S: And should policy-makers and their ilk think it worthwhile? Why? Do they need to be bothered about realism, idealism, relativism and whether there can be a social science: Why should they care?

R: If this book has been worthwhile, then yes, part of that worth will lie in the implications for policy which may and should be seen to emerge. Part of the burden of Part 1 of the book is to suggest for instance that a therapeutic/Wittgensteinian take on Kuhn will provide a better basis for science policy than the rather unpleasant musings of, say, Steve Fuller. Kuhn counterposes nicely the need to actually

understand the science in a specialised domain if one is to meaningfully engage with it / decide how to use it, etc., on the one hand, with the (complementary) irrelevance normally of scientific understanding (in such a domain) to other areas of science or to questions of a non-scientific nature (including questions such as: which technologies ought we to develop/to prohibit, etc.), on the other. Scientific authority is limited to the discipline – the 'paradigm' – in question.

Somewhat similarly, part of the burden of Part 2 is implicitly to suggest that there is something profoundly dangerous and counter-productive about the attempt, now widespread for instance in Funding Councils, to model everything on (the model of) science. Science policy ought to be *science* policy, and ought to take seriously the differences, that Wittgenstein aimed to teach, between different areas of human life. I sought to show in Part 2 the ways in which even the best candidates for scientific status among 'the human sciences' show some very clear signs of such difference, such divergence. Let alone less strong candidates (sociology, anthropology, etc.). *Let alone* the humanities.

As we have already touched upon earlier in this Interview:

- If Economics were reconceived less scientistically in general and less positivistically in particular, the world would be less vulnerable to Minskyan moments such as we are currently still experiencing.
- If Psychiatry were reconceived less scientistically and more philosophically (if less of it were, roughly, 'misbegotten epistemology and metaphysics', as Winch might have put it), then we could have a much richer understanding of the actual dynamics of psychopathology and of the limits to the understanding thereof.
- These would not be trivial outcomes. They give some sense I hope of the potential importance of the thinking that I have begun in Part 2 of the present work.

Another example, which I am starting to look at in my current work (see for instance my paper just out in the *Journal of Evaluation in Clinical Practice*), is the rhetorically-dubious, over-blown and under-investigated concept of 'evidence' at work in the 'Evidence-Based Medicine' movement. The kind of anti-scientistic approach taken to EBM already by Steven Poole (see for instance this pithy short posting http://unspeak.net/evidence-based-medicine/), and by Michael Loughlin in various academic works (including his (2009a and b)), could naturally be extended and complemented by the *kind* of approach exemplified in the present work.

For the over-arching reason why policy-makers and the like should be concerned at least a little bit with apparently-abstruse questions of 'realism', of 'relativism', and so on, is that there is a serious danger in our contemporary culture that 'science' will come to be seen as the measure of all things. So we need to interrogate 'Scientific Realism', and get clear on where if anywhere it is plausible, and so on.

You and I clearly differ on the subject of Chomskian Linguistics – that's the one clear area where you are not doubtful of the wisdom of attributing a quasi-natural-scientific-status to it while I am. But I hope you can agree with me, as a general point, that it would be very worrying for science to be tacitly taken to be the measure of all things … I am thinking here for instance of this remark of Wittgenstein's, which might be a nice quotation to finish with (*Culture and Value*, p.70, emphasis added): '[W]hy am I so anxious to keep apart [different] ways of using "declarative sentences"? Is it really necessary? Did people in former times really not properly understand what they wanted to do with a sentence? Is it pedantry? – It is simply an attempt to see that every usage gets its due. Perhaps then *a reaction against the over-estimation of science. The use of the word "science" for "everything that can be said without nonsense" already betrays this over-estimation.* For this amounts in reality to dividing utterances into two classes: good and bad; and the danger is already there. It is similar to dividing all animals, plants and rocks into the useful and the harmful.'

S: Thanks Rupert. Yes, despite our specific disagreement over Chomsky, and perhaps over some related portions of cognitive science or psychology, I think we can probably agree on that as an important general warning, in closing.

R: Thanks Simon!

Bibliography

Agamben, G. 2004. *The Open: Man and Animal*, edited by Kevin Attell. Stanford: Stanford University Press.

Austin, J. L. 1956b/1979. Ifs and Cans. *Proceedings of the British Academy*, 109–132. Reprinted in Austin (1979) 205–232.

Baker, G. 2004. *Wittgenstein's Method*: *Neglected Aspects.* Oxford: Blackwell.

Baker, G. and Hacker, P. 1984. *Scepticism, Rules and Language*. Oxford: Blackwell.

Bhaskar, R. 1979. *The Possibility of Naturalism*. Brighton: Harvester.

Blankenburg,W. 1971. *Der Verlust der Naturlichen Selbstvertaendichkeit*. Stuttgart: Ferdinand Enke V.

Block, N., O. Flanagan and G. Güzeldere (eds). 1997. *The Nature of Consciousness: Philosophical Debates*. Cambridge, MA: MIT Press.

Bloor, D. 1983. *Wittgenstein: A Social Theory of Knowledge*. London: Macmillan.

—— 1996. The Question of Linguistic Idealism Revisited, in *The Cambridge Companion to Wittgenstein*, edited by Sluga and Stern. Cambridge: Cambridge University Press.

Boland, L. 1979. A Critique of Friedman's Critics. *Journal of Economic Literature*, 17, 503–522.

Button *et al* (eds). 1995. *Computers, Minds and Conduct*. Cambridge: Polity.

Cade-Hetherington, S. 1992. *Epistemology's Paradox*. Lanham, MD: Rowman and Littlefield.

Campbell, C. 1996. *The Myth of Social Action*. Cambridge: Cambridge University Press.

Cavell, S. 1990. *Conditions Handsome and Unhandsome: The Constitution of Emersonian Perfectionism.* Chicago: University of Chicago.

Chalmers, D.J, and Searle, J.R. 1997. Consciousness and the Philosophers: An Exchange, Letters between Chalmers and Searle. *New York Review of Books*, 15.

Chesterton, G.K. 1959. *Orthodoxy*. New York: Doubleday.

Coates J. 1996. *The Claims of Common Sense.* Cambridge: CUP.

Cole. K. 1999. *Economy Environment Development Knowledge.* London: Routledge.

Conant, J. 1989. An Interview with Stanley Cavell. *Bucknell Review*, 32, 1.

—— 1991a. The Search for Logically Alien Thought. *Philosophical Topics*, 20, 115–180.

—— 1991b. Throwing Away the Top of the Ladder. *The Yale Review*, 79, 328–364.

—— 1994. *Introduction* in *Words and Life*. Cambridge, MA: Harvard University Press.

Coulter, J. 1979. *The Social Construction of Mind*. London: Macmillan.
—— 1983. *Rethinking Cognitive Theory*. London: Macmillan.
—— 1989. The Human Agent as Unconscious 'Information-Processor', in *Mind in Action*. Cambridge: Polity, 69–80.
Crary, A and Read, R. (eds). 2000. *The New Wittgenstein*. London: Routledge.
Crary, A. 2000. Wittgenstein and Political Thought, in Crary and Read, 2000, 118–145.
—— 2002. The Happy Truth: J.L. Austin's How To Do Things With Words. *Inquiry* 45(1), 59–80.
Davidson, D. 1974. On the Very Idea of a Conceptual Scheme. *Proceedings and Addresses of the American Philosophical Association,* Vol. 47, 5–20.
Diamond, C. 1988. Losing your Concepts, *Ethics*, 98, 255–277.
—— 1989. Rules: Looking in the Right Places, in *Wittgenstein: Attention to Particulars*: *Essays in Honour of Rush Rhees (1905–1989)*, edited by Phillips, D.Z. and Winch. P. London: Macmillan.
—— 1991. *The Realistic Spirit*. Cambridge, MA: MIT.
—— 2000. Ethics, imagination and the method of the *Tractatus*, in *The New Wittgenstein*, edited by Crary and Read, 149–174.
Douglas, M and Hull, D. (eds), 1992. *How Classification Works: Nelson Goodman among the Social Sciences*. Edinburgh: Edinburgh University Press.
Dowd, D. 2004. *Capitalism and its Economics*. London: Pluto.
Dupré, J. 1981. Natural Kinds and Biological Taxa. *Philosophical Review*, 90, 66–90.
—— 1983. The Disunity of Science. *Mind*. 92: 321–46.
—— 1993. *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*. Cambridge, MA: Harvard University Press.
—— 1996. Metaphysical Disorder and Scientific Disunity, in *The Disunity of Science*, edited by Galison. P. and Stump, D.J. Stanford CA: Stanford University Press.
—— 1998. Against Reductionist Explanations of Human Behaviour I. *Proceedings of the Aristotelian Society*, Suppl. 72, 153–71.
Edelman, G. 1992. *Bright Air, Brilliant Fire*. New York: Basic Books.
Epstein, M. 1999. *Going to Pieces without Falling Apart*. New York: Random House.
Evnine, S. 1989. Understanding Madness? *Ratio*, 2, 1–18.
Flanagan, O. 1992. *Consciousness Reconsidered*. Cambridge, MA: MIT Press.
Flyvbjerg. B. 2001. *Making Social Science Matter: Why Social Inquiry Fails and How it Can Succeed again*. Cambridge: C.U.P.
Francis, D.1994. The Golden Dreams of the Social Constructionist. *Journal of Anthropological Research,* 50:2, 97–108.
Friedman, M. 1953. *Essays in Positive Economics*. Chicago: Chicago University Press.
—— 1962. *Capitalism and Freedom*. Chicago: Chicago University Press.

Fuller, S. 2000. *Thomas Kuhn: a Philosophical History for our Times*. Chicago: University of Chicago Press.

—— 2005. *Kuhnenstein: or, the Importance of being Read*. Philosophy of the Social Sciences, 35, 480–498.

Garfinkel, H. 1965. *Studies in Ethnomethodology*, London: Prentice Hall.

Garrett, R. Jr. 1999. *What do Economists Know*? London: Routledge.

Geertz, C. 1995. *After the Fact.* Cambridge, MA: Harvard.

Gibbs, P. 1997. Schizophrenia as Philosophy: a Grounding for Philosophical Counselling. *Contemporary Philosophy*, XIX, 47–52.

Giddens, A. 1984. *The Constitution of Society*. Cambridge: Polity.

Gill, J and Soori, M. 1989. *A Post-Modern Epistemology*: *Language, Truth and Body*. Lewiston: Edwin Mellen.

Gill, J. 1996. *Wittgenstein and Metaphor.* London: Humanities Press.

Glendinning, S. 1998. *On Being with Others*. London: Routledge.

Griffiths, P. 1997. *What Emotions Really Are* Chicago. Chicago University Press.

Guetti, J. 1993a. *Wittgenstein and the Grammar of Literary Experience*. Athens, GA: University of Georgia Press.

Guetti, J 1993b. Idling Rules. *Philosophical Investigations*, 16:3, 179–197.

Guetti, J. and Read, R, 1996. Acting from Rules. *International Studies in Philosophy* XXVIII.

—— 1999. Meaningful Consequences. *Philosophical Forum* XXX: 4, 289–315.

Gunnell, J.G. 1998.*The Orders of Discourse. Philosophy, Social Science, and Politics*. Lanham. MD: Rowman and Littlefields Publishers.

Hacker, P.M.S. 1990. *Analytical Commentaries on the Philosophical Investigations*. Oxford: Blackwell.

Hacking, I. 1975. *Why does Language Matter to Philosophy?* Cambridge: CUP.

Hacking, I. 1999. *The Social Construction of What?* Cambridge: Harvard University Press.

Henry, P. 1992. Wittgenstein and Contemporary Linguists, in *Wittgenstein and Contemporary Theories of Language*, Working Papers from the University of Bergen, No.5. edited by Henry, P. and Utaker. Bergen: Wittgensteinarkivet.

Hintikka, J. 1999. The Emperor's New Intuitions. *Journal of Philosophy*, 96(3),127–47.

Hamilton, R. *Understanding Winch, Understanding Ourselves*, (unpublished).

Hoyningen-Huene, P. 1993. *Reconstructing Scientific Revolutions: Thomas Kuhn's Philosophy of Science*, Chicago: University of Chicago Press.

Hutchinson, P. 2008. *Shame and Philosophy: An Ethical Investigation*. Basingstoke: Palgrave.

Hutchinson, P. 2010. Thinking and Understanding. In *Wittgenstein: Key Concepts*. K.D. Jolley. Durham: Acumen.

Hutchinson, P., Read, R. 2005. Whose Wittgenstein? *Philosophy*, 80 (3), 432–454.

Hutchinson, P., Read, R. and Sharrock, W. 2008. *There is No Such Thing as a Social Science: In Defence of Peter Winch.* London: Ashgate.

Janik, A. 2006. *Assembling Reminders*. Sweden: Santerus Academic Press.

Jaspers, K. 1963/1923. *General Psychopathology*. Manchester: Manchester University Press.
Jolley, K.D. (ed.) 2010. *Wittgenstein: Key Concepts*. Durham: Acumen.
Jutten T. 2010. Adorno on Kant, Freedom and Determinism. *European Journal of Philosophy*, 1–27
Kindi, V. 1995. Kuhn's The *Structure of Scientific Revolutions* revisited. *Journal for General Philosophy of Science*, 26(1), 75–92.
Koethe, J. 1996. *The Continuity of Wittgenstein's Thought*. Ithaca, NY: Cornell.
Kuhn, T.S. 1962/1996. *The Structure of Scientific Revolutions ("SSR" / "Structure")*, 3rd edition. Chicago: University of Chicago Press.
—— 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.
—— 2000. *The Road since Structure*, edited by Conant J and Haugeland, J. Chicago: University of Chicago Press.
Laing, R.D. 1965. *The Divided Self.* London: Penguin.
Lakatos, I., Musgrave, A., (eds) 1970. *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
Lakoff, G, Johnson, M. 1981. *Metaphors we live by*. Chicago: U. Chicago.
—— 1999 *Philosophy in the Flesh: The Embodied Mind and Its Challenge to Western Thought.* New York: Basic Books.
Leudar, I. 2000. *Voices of Reason*, *Voices of Insanity: Studies of Verbal Hallucinations*. London: Routledge.
Long, D. 1964. The Philosophical Concept of a Human Body. *Philosophical Review* (July), 321–37.
Loughlin, M. 2009a. The Search for Substance: A Quest for the Identity-conditions of Evidence-based Medicine and Some Comments on Djulbegovic et al. *Journal of Evaluation in Clinical Practice,* 15(6), 910–914.
Loughlin, M. 2009b. The Basis of Medical Knowledge: Judgement, Objectivity and the History of Ideas. *Journal of Evaluation in Clinical Practice*, 15 (6), 935–940.
Lyas, C. 1999. *Peter Winch*. Teddington: Acumen.
Lynch, M. 1993. *Scientific Practice and Ordinary Action*. Cambridge: Cambridge University Press.
Matson, W. 1966, Why Isn't the Mind–Body Problem Ancient? in *Mind, Matter and Method*, edited by Feyerabend, P. and Maxwell, G. Minneapolis: University of Minnesota Press.
McDowell, J. 1989. Scheme–Content Dualism, Experience and Subjectivity. Presented as part of *Two Dualisms: Scheme–Content and Fact*–Value, conference on the Philosophy of the Human Studies, Greater Philadelphia Philosophy Consortium, November 11
McGilchrist, I. 2009. *The Master and his Emissary*. New Haven: Yale.
McGinn, C. 1995. Consciousness and Space. *Journal of Consciousness* Studies, 2(3), 220–30.

—— 1999. Can We Ever Understand Consciousness? *New York Review of Books*, 10, June 1999.
McManus, D. 2006. *The Enchantment of Words*. Oxford: OUP.
Mirowski, P. 1989. *More Heat than Light*. Cambridge: CUP.
Mulhall, S. 1990. *On Being in the World: Wittgenstein and Heidegger on Seeing Aspects*. London: Routledge.
Nietzsche, F. 1968. *The Anti-Christ*. Harmondsworth: Penguin.
Nietzsche, F. 1974. *The Gay Science.* New York: Vintage.
Nelson, J. 2006. *Economics for Humans*. Chicago: University of Chicago Press.
Pleasants, N. 1999. *Wittgenstein and the Idea of a Critical Social Theory*. London: Routledge.
—— 2000. Winch and Wittgenstein on Understanding ourselves Critically: Descriptive, not Metaphysical. *Inquiry*, 43(3), 289 – 317.
Popper, K. 1994. *The Myth of the Framework: In Defence of Science and Rationality*. Routledge: London.
Preston, J. 1997. *Paul Feyerabend*. Oxford: Polity
Rapaport, J. 1990. *The Boy Who Couldn't Stop Washing*. London: Harper Collins.
Read, R. 1995. The Unstatability of Kripkian Scepticisms. *Philosophical Papers*, XXIV: 1.
—— 1997. The Career of Internal Relations in Wittgenstein's Thought. *Wittgenstein Studies*(2).
—— 1998. There is No Good Reason to Think that Philosophical Counselling can Cure Schizophrenia: a Reply to Gibbs. *Contemporary Philosophy*, *XX*, 59–62.
—— 2000a. What 'There Can Be No Such Thing as Meaning anything by any Word' Could Possibly Mean, in Crary and Read (2000).
—— 2000b. Is There a Legitimate Way to Raise Doubts about the Immediate Future from the Perspective of a Doubted Immediate Past?: The Case of Kripke, in *Wittgenstein Jahrbuch*, edited by Wilhelm Lüttersfeld, Andreas Roser and Richard Ratzsch. Frankfurt: Peter Lang, 89–112.
—— 2000c. In Closing: the New Hume's New Antagonists: On the Relevance of Goodman and Wittgenstein to the New Hume debate, in *The New Hume Debate*, edited by Read, R and Richman, K. London: Routledge.
—— 2001a. On Approaching Schizophrenia through Wittgenstein. *Philosophical Psychology*, 14(4), 449–475.
—— 2001b. On Wanting to Say: "All we need is a Paradigm". *The Harvard Review of Philosophy* XI, 88-105.
—— 2001c. What Does "Signify" Signify? *Philosophical Psychology*, 14(4), 499–514.
—— 2002a. Understanding Kuhnian Incommensurability: Some Analogies from Wittgenstein. *Wittgenstein Studies,* 151-172.
—— 2002b. Marx and Wittgenstein on Vampires and Parasites: A Critique of Capital and Metaphysics, in *Marx and Wittgenstein: Knowledge, Morality and Politics*, edited by Kitching, N. and Pleasants G. London: Routledge, 254–281.

—— 2002c. Logicism and Anti-Logicism Alike are both Bankrupt and Unnecessary, in *Wittgenstein and the Future of Philosophy*: *Proceedings of the 2001 Ludwig Wittgenstein Symposium*, edited by Puchberg, Haller and Puhl. Vienna: Holder Pichler.

—— 2003. On Delusions of Sense: A Response to Coetzee and Sass. *Philosophy, Psychiatry, & Psychology*, 10(2), 135–141.

—— 2004a. Kuhn: A Wittgenstein of the Sciences? *UEA Papers in Philosophy*, (15).

—— 2004b. Wittgenstein and Faulkner's Benjy: Reflections on and of Derangement, in *The Literary Wittgenstein*, edited by Gibson, J. and Huemer, W. London: Routledge. 267–288.

—— 2005. Book Review: How and How not to Write on a 'Legendary' Philosopher. *Philosophy of the Social Sciences*, 35(3), 369–387.

—— 2007a. *Applying Wittgenstein*. London: Continuum

—— 2007b. Economics is Philosophy: Economics is not Science. *International Journal of Green Economics* 1:3, 307–325.

—— 2008. The 'Hard' Problem of Consciousness Is Continually Reproduced and Made Harder by All Attempts to Solve It. *Theory, Culture and Society*, 25:2, 55–86.

—— 2009a. Extreme Aversive Emotions: A Wittgensteinian Approach to Dread, in *Wittgensteinian Perspectives: Emotions and Understanding,* edited by Gustaffson *et al*. London: Palgrave Macmillan, 221–235.

—— 2009b. Towards a Green Philosophy of Money. *Eco-Politics*, 3, 3–26.

—— 2011a. A Strengthened Ethical Version of Moore's Paradox: Lived Paradoxes of Self-loathing in Psychosis and Neurosis. Forthcoming in *Philosophical Psychology.*

—— 2011b. The Difference Principle is Not Action-guiding. *Critical Review of Social and Political Philosophy* 14 (4):487-503.

Read, R and Richman, K. (eds). 2000. *The New Hume Debate*. London: Routledge.

Read, Rupert and Gregory, M. 2007. Book Review: Persons and Passions: Essays in Honour of Annette Baier. *Mind*, 116, 173–176.

Read, Rupert and Lavery, Matt (eds). 2011. *Beyond the Tractatus Wars*. New York: Routledge.

Reid, L. 1988. Wittgenstein's Ladder: The *Tractatus* and Nonsense. *Philosophical Investigations*, 21, 97–151.

Rhees, R (ed.) 1984. *Recollections of Wittgenstein*. Oxford: Oxford University Press.

Richter, D. 2009. Book Review: *Applying Wittgenstein*. In *Philosophical Investigations*, 32(1).

Roszak, T. 1991. *The Voice of the Earth*. New York: Simon and Schuster.

Samuelson, P. 1963. Discussion: "Problems of methodology". *American Economic Review Papers and Proceedings*, 53, 231–236.

Sass, L. 1992. *Madness and Modernism*. New York: Harper Collins.

—— 1994a. *The Paradoxes of Delusion*. New York: Cornell University Press.

—— 1994b. Civilized Madness: Schizophrenia, Self-consciousness and the Modern Mind. *History of the Human Sciences*, 7, 83–120.

—— 1997. The Consciousness Machine: Self and Subjectivity in Schizophrenia and Modern Vulture, in *The Conceptual Self in Context*, edited by Neisser & Jopling. Cambridge: Cambridge University Press.

—— 2000. Furtive Abductions: Schizophrenia, the Lived Body, and Dispossession of Self. *Arobase: Journal des Lettres et Sciences Humaines*, *4*.

—— 2007. Contradictions of Emotion in Schizophrenia. *Cognition & Emotion,* 21(2), 351–390.

Sharrock, W. 1989. Ethnomethodology. *British Journal of Sociology*, 40(4), 657–77.

Sharrock, W. and Anderson, R.J. 1989. *Working for Profits*. Aldershot: Avebury.

Sharrock, W. and Coulter. J. 2004. Theory of Mind (ToM): A Commentary. *Theory and Psychology*, 14(5), 579–600.

Sharrock, W and Leudar, I. 2003. Changing the Past? *History of the Human Sciences*, 16, 105–121.

Sharrock, W and Read, R. 2002. *Kuhn: Philosopher of Scientific Revolution*. Cambridge: Polity Press.

Sharrock, W and Read, R. 2003. Does Thomas Kuhn Have a 'Model of Science? *Social Epistemology* 17(2), 293–296.

Schatzki, T. 1983. The Prescription is Description, in *The Need for Interpretation*, edited by Mitchell and Rosen. London: Athlone.

—— 1991. Elements of a Wittgensteinian Philosophy of the Social Sciences. *Synthese*, 87, 311–329.

—— 2000. Wittgenstein and the Social Context of an Individual Life. *History of the Human Sciences*, 13:1, 93–107.

Searle, J. 1992. *The Rediscovery of the Mind*. Cambridge, MA: MIT Press.

Shotter, J. 1998. The Dialogical Nature of Our Inner Lives. *Philosophical Explorations*, 1(3), 185–200.

Scheman, N. 1996. Forms of Life: Mapping the Rough Ground, in *The Cambridge Companion to Wittgenstein*, edited by Sluga and Stern. Cambridge: Cambridge University Press.

Sechehaye, M. 1951. *Autobiography of a Schizophrenic Girl: The True Story of* '*Renee*'. New York: Meridian.

Stone, M. 2000. Wittgenstein on Deconstruction, in *The New Wittgenstein*, edited by Crary and Read, 83–117.

Tallis, R. 1999. *On the Edge of Certainty*. London: Macmillan.

Turner, S. 2010. Webs of Belief or Practices: The Problem of Understanding. *European Journal of Sociology*, 51, 403–427.

Williams, D. 1995. *Somebody Somewhere*. London: Doubleday.

Wilson, F. (ed.) 1970. *Rationality*. Oxford: Blackwell.

Waller, B. 1982. Mentalistic Problems in Cicourel's Cognitive Sociology. *Journal for the Theory of Social Behaviour* 12(2): 177–99.

Watson, R. 1998. Ethnomethodology, Consciousness and Self. *Journal of Consciousness Studies* 5(2), 202–23.

Williams, M. 1999. *Wittgenstein, Mind and Meaning: Toward a Social Conception of Mind*. London: Routledge.

Winch, P. 1964. Understanding a Primitive Society. *American Philosophical Quarterly* 1(4), 307–24.

—— 1958/1990. *The Idea of a Social Science and its Relation to Philosophy* ("*ISS*") (2nd ed). London: Routledge.

—— 1969. The Unity of Wittgenstein's Philosophy. *Studies in the Philosophy of Wittgenstein,* edited by Winch, P. London: Routledge.

—— 1987. Winch, P. *Trying to Make Sense*. Oxford: Basil Blackwell.

—— 1992. Persuasion. *Midwest Studies in Philosophy*, XVII, 123–137

—— 1922. Wittgenstein, L. 1922. *Tractatus Logico-Philosophicus (TL-P)*. C.K. Ogden (trans.). London: Routledge & Kegan Paul.

—— 1953/1958. *Philosophical Investigations (PI)*. G.E.M. Anscombe and R. Rhees (eds), G.E.M. Anscombe (trans.), Oxford: Blackwell.

—— 1964. *Philosophical Remarks*. R. Rhees (ed.), R. Hargreaves and R. White (trans.). Oxford: Blackwell.

—— 1969. *On Certainty*, G.E.M. Anscombe and G.H. von Wright (eds). G.E.M. Anscombe and D. Paul (trans.). Oxford: Blackwell.

—— 1978. *Remarks on Frazer's Golden Bough*. Doncaster: Brynmill Press.

—— 1980. *Culture and Value*. G.H. von Wright (ed.), P. Winch (trans.). Oxford: Blackwell

—— 1998. *Culture and Value* (revised edition). Oxford: Blackwell.

Index