THE CONSTRUCTIVIST CONCEPTION OF LANGUAGE AND ITS FUNCTIONS IN SCIENCE

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ABSTRACT

The main task of this thesis is to contrast two philosophical conceptions of 'language-and-its-functions' in science. The first, which I call the *dualist* conception, has dominated history and philosophy of science. There are two constituents of dualism: the disjunction of 'language' and 'world' (the latter term encompassing the supposed non-linguistic subject matter of science); and the assumption that the philosophically primary function of language in science is to be 'about' the world (thus language may be 'about' — or *fail* to be about — mind-dependent phenomena or mind-independent facts, depending on how a dualist conceives the world).

In Part 1 I examine a wide spectrum of formulations of the dualist conception and show that it has influenced philosophers of science of almost every persuasion. I argue that language/world dualism has been presupposed without justification, and (besides being implausible) has remained ill-developed as a philosophical thesis. At the end of Part 1, I look at a body of medical publications from the early nineteenth century that are evidence of a crisis in the use of the 'formal' language of medicine — here language appears to be plainly differentiated from the subject matter of science by scientists themselves. I use the case study to argue that crises of language in pre-consensual science are commonplace, but provide no support for the dualist conception.

The refutation of dualism is attempted in Part 2 where the *constructivist* conception of language is presented. I begin with an examination of recent scholarship in the history of science, laboratory studies, and the sociology of knowledge. In particular, I defend Gooding's 'theory' of the making of meaning in experiment, and argue that philosophy of science requires a much richer conception of language than that found in dualism. The conception I propose makes out language to be not a reference or symbolic device, but a resource of metaphysics, skills, and activities by way of which scientists communally make sense of their experimental experience. Functions of language considered basic by dualists (such as description and reference) are shown to be *made* functions, convention-bound, and historically contingent. Because language is an integral aspect of scientific knowledge from its most exploratory to its most accomplished levels, and because through uses of language human agency constitutes what natural knowledge and facts *are*, there can be no good argument for drawing a metaphysical distinction between language and that which it is 'about' in science, or for dissociating knowledge from that which it is knowledge 'of'. Consequently dualism is rejected.

Three secondary tasks of this thesis should be mentioned. First, I engage the realism/anti-realism debate to show that both sides rely heavily on the dualist conception. Constructivism about language amounts to neither realism nor anti-realism as traditionally understood. Second, I emphasise the *philosophical* value of a recent body of primarily *historical* research on 'literary' aspects of science. Scepticism about the relevance of history to philosophy of science needs to be contained even today. Third, I discuss new areas of research in the philosophy of science. Constructivism, being a philosophical conception of language much richer than dualism, brings into focus philosophical issues that have yet to be examined in the philosophy of science.

CONTENTS

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INTRODUCTION

A PHILOSOPHICAL NON-ISSUE: LANGUAGE AND ITS FUNCTIONS IN SCIENCE

> His is a philosophy founded upon reflections on language, and no such philosophy can teach anything positive about natural science.[1] (Hacking on Putnam)

Twentieth-century science, commonly regarded as a source of knowledge *par excellence*, has attracted the scholarly attention of historians, sociologists, anthropologists, and cultural critics, and has thereby become the focus of many relatively new industries in the humanities. Twentieth-century philosophy of science has also, of course, enjoyed a rapid growth, reflecting the growth in importance and status of science itself. Discussions of scientific rationality have been wide-ranging.

At the same time, competing philosophical notions of rationality have kept discussions from focusing on the study of actual scientific practice, with the result that philosophers of science in the analytic tradition have not comprehensively furthered our understanding of what scientific knowledge is or how it comes to be. Only in recent years, under the influence primarily of historians of science, have philosophers looked more closely at scientific practice and its history. This redirection has been at the root of much reworked and new philosophy.[2]

The thesis set out in these pages is about an aspect of scientific practice that has had little or no influence on contemporary philosophy. It is about language in science. Of course, asking after language in philosophy is hardly a novelty. It has been discussed extensively at least since Plato, and philosophy of language of one sort or another is part of every philosopher's basic education. Yet language has not figured prominently as a topic in philosophy of science. Given the centrality of language to philosophy throughout its history, it might come as a surprise to find it discussed sparingly in the considerable quantities of new philosophy that twentieth-century science has given rise to in its wake.

[1]Hacking, *Representing and intervening*, p. 92.

[2]For historical accounts of the development of the philosophy of science see Losee, *A historical introduction to the philosophy of science*; and idem, *The philosophy of science and historical enquiry*. On the relatively recent philosophical uses of history see, for example, Finocchiaro, 'The uses of history in the interpretation of science'.

Two objections to what I have just said, coming from two quite different directions, I acknowledge immediately. In the first case, the objection might be raised that we are not normally inclined to think of language as a distinct 'aspect' of science. 'Language as opposed to what?', one might ask. Scientific theories? — but theories are only ever encountered in a language of one kind or another, familiar or not. Laboratories? — but, again, laboratories contain conversing scientists, word processors, notebooks in continuous use, libraries and archives, document-inscribing instruments, labelled samples and labelled Petri dishes. Bacterial cultures? At first they might seem a possibility — yet the fact that no one would dream of calling a bacterial culture an 'item of language' does not normally lead anyone to think that bacterial cultures stand *opposed*, in some fashion, to language. Nor does it follow that everything in science either is or isn't an item of language. Bacterial cultures, for example, although obviously not items of language, would not be in the laboratory were they not contributing to the articulation of new facts, were they not an object of interest to a particular group of note-taking technicians and paper-writing scientists, were they not a part of the outlook of some research programme. Every characterisation ever given of a bacterial culture has, of course, been in terms of some language.

This 'common-sense' objection to the suggestion that language is a distinct 'aspect' of science springs from an entirely respectable instinct which I myself develop in opposition to the language/world distinction in the philosophy of science. However, it is also my view is that we *can* speak properly and innocently of language as an aspect of science, not in the course of contrasting it with something non-linguistic or physical, but in the course of highlighting and clarifying its role as a *knowledge-making* device. Language is of assistance to scientists not as a passive medium for the articulation of empirical knowledge, but as a complex resource by means of which a convincing understanding of the world may be built. Identifying language as an 'aspect' of science is useful just insofar as it helps us to gain a better understanding of the making and nature of scientific knowledge.

The second objection to my opening claim is that, in fact, it is quite possible to name philosophers of science in whose writings remarks about language occur in relatively high concentration (Rudolf Carnap, Moritz Schlick, and fellow logical positivists come to mind). My reply is that practically in all such cases the remarks demonstrably derive from preconceptions about what language in science *ideally ought to be like* and not from a close examination of what that language is or how it is used.[3] Such discus-

[3]Schlick believed that 'the languages employed in the sciences are designed to make possible the construction of unambiguous expressions that can be true or false' (Juhos, 'Moritz Schlick', p. 321).

sions, even if exhaustive, have had too narrow a focus. In a well-known review of the discipline, Larry Laudan and associates listed and categorised all major philosophical theses on scientific change and related topics proposed up until the early 1980s: of the hundreds of theses identified not one was about language and its functions.[4]

It is necessary, of course, to distinguish the rarity of philosophical *discussion* of language in science from the demonstrable presence of *assumptions* about it in philosophical writings on science. The drawing of this distinction enables the rather vague expression 'language in science' to become associated with concrete and clear philosophical issues. Once the distinction between overt acknowledgment (or the lack of it) and covert reliance on certain assumptions about language is emphasised, two questions arise. One concerns the assumptions themselves: what are they (how might

This is an idealisation — see Popper's remarks in the introduction to *The logic of scientific discovery*, where he states that 'the models of "the language of science" which [philosophers such as Schlick] construct have nothing to do with the language of modern science', but are driven rather by the 'spiritual consolations offered by the hope for knowledge that is "exact" or "precise" or "formalised"' (pp. 20-21). Popper offers an example elsewhere: 'By 1931 Carnap ... under Neurath's influence ... had adopted the thesis of physicalism, according to which there was *one* unified language which spoke about physical things and their movements in space and time. Everything was to be expressible in this language, or translatable into it' (*Conjectures and refutations*, p. 265). That unified *ideal* language was the preferred end of philosophers of science at the time, not the language of science in use. On the 'idealisations' of science by philosophers such as Hempel, Feigl, Frank, and others, see Toulmin, 'From form to function', p. 146. Danto's claim that 'the marked linguistic bias of [Henri Poincaré's] philosophising has been influential in directing philosophers' attention to the language in which scientific discoveries are expressed and theories formulated' ('Problems of the philosophy of science', p. 293), has remained, as I maintain in Part 1, largely untrue.

[4]See Laudan et al., 'Scientific change'. Hull's *Science as a process* (1988), whose enormous index contains not one reference to language, illustrates the neglect of language in contemporary works on the philosophy of science. I have mentioned above, and I argue in Part 2 of this thesis, that in all of the literature that has had science as its focus, language will be found observed and discussed in the writings not of philosophers but of historians and sociologists. Recent examples of this literature include Anderson, *Between the library and the laboratory*; Bazerman, *Shaping written knowledge*; articles in Dear (ed.), *The literary structure of scientific argument*; Gilbert and Mulkay, *Opening Pandora's box*; Gooding, *Experiment and the making of meaning*; Gross, *The rhetoric of science*; Latour, *Science in action*; Latour and Woolgar, *Laboratory life*; Lynch, *Art and artifact in laboratory science*; Myers, *Writing Biology*; and Shapin and Schaffer, *Leviathan and the air-pump*. For bibliographies of fundamental work in the sociology of science and scientific knowledge see Brown, 'The sociological turn', and Shapin, 'History of science and its sociological reconstructions'.

they be spelt out?) and what are their consequences for the ways in which philosophy of science has been written? The second concerns the adequacy of the assumptions: can they be justified? — have they *ever* been justified? — and what, if anything, can be said about language and its functions in addition to or instead of existing assumptions?

Guided by these questions, the purpose of the present thesis is two-fold. First, in Part 1, I attempt a survey and clarification of assumptions about language in science found in writings of contemporary philosophers of science. Second, in Part 2, I present a critique of these assumptions and discuss alternative assumptions that have either remained marginal in philosophy or have not been discussed at all or have been explored to some extent by historians, sociologists and anthropologists of scientific knowledge. The intended result is a revised and improved philosophical conception of scientific language and its functions.

At the end of Part 1 there is a brief section on medical history. It is, more accurately, a very compressed case study of some problems of language in the history of medicine. It gives an indication (and no more) of the kinds of problems that language has given rise to in the course of scientific inquiry by looking at a period in medical history when such problems were particularly exaggerated. Specifically, I have used a selection of examples from medical writings published in the early British medical periodicals (early in the nineteenth century) to illustrate the ways in which medical language had become a frustrating obstacle to those who chose to participate in the debates, raging at the time, on the nature of disease (in particular, epidemic disease). A remarkable feature of the nineteenth-century texts — and of other writings from 'pre-consensual' periods in science — is that the formal *language of medicine* (or, more specifically, the meanings of many words, the structures of arguments, the very act of writing, of 'putting it all into words', the printed text, the reflections that went into its preparation, and the ways in which contemporaries read it and responded to it) was fraught with difficulties, difficulties widely acknowledged and remarked upon at the time.

My initial response to these rather trivial findings was to think along the following lines: science (or medicine in this case) is about turning experience, practice, and methodical observations of nature into a methodical *discourse* about nature. Scientific inquiry, I supposed, converts 'the world' into 'the word', objectifying observations by translating them into language that is potentially intersubjective. I saw the difficulties present and expressed in the medical texts I have mentioned as arising both from attempted 'conversions' of diverse experiences into a shareable language (as it happened, authors of many of these texts inhabited quite dissimilar cultural and physical environments) and from attempts to establish one language as canonical and dispense with the rest (for only this could lead to shared knowledge and a more unified medicine). I also considered the possibility that an ever-changing language presented doctors — natural philosophers, scientists, or in any case its users — with challenges not unlike those more material and technical challenges repeatedly experienced in the history of science with the introduction of novel scientific instruments, the setting up of new experiments, the articulation of theories, the implementation of unorthodox measures against diseases, etc.[5] The problems of language I came across in nineteenth-century medical journals were, according to my initial reaction, a part — a neatly distinguishable part — of the general problem of determining 'what the world is like'. I thought, in other words, that the set of problems scientists faced at any particular time always included a subset of problems to do with language.

With these assumptions about language in science I turned to the better-known philosophical writings on science in search of analyses of both the 'world-to-word conversion' (as I called it) and the emergence of shared scientific expressions. But instead of analyses of these particular issues I found many mostly veiled assumptions about language in science resembling my own assumptions at the time, in particular the assumption that science converts methodical observations of natural facts into methodical discourse. All such assumptions, I realised, relied heavily on what I shall henceforth refer to as *the disjunction of 'language' and 'world'*. Otherwise, as I have already indicated, language was rarely an issue in mainstream philosophy of science.

Manifestations of the philosophical assumption that 'language' and 'world' (more precisely the *physical* world, the paramount subject matter of science) are disjointed, and that the primary function of language is to 'represent' or 'refer' to aspects of the world lying outside language, are surveyed in Part 1. In Part 2 I argue that the disjunction is unjustifiable: philosophical considerations and scientific history and practice unequivocally testify against it. Language has mistakenly been assumed to function in a relationship of correspondence to the world from which it is distinguishable at will, in practice or in principle. Close attention to the making of scientific knowledge leads to the identification of *rhetorical* disjunctions of language and world that are useful in practice, but renders metaphysical counterparts untenable. My aforesaid assumptions concerning the distinguishability of the language of science from the subject matter of science (or the discourse/object-of-observation distinction) were subsequently, as a consequence, quite drastically revised.

[5]On these problems see, respectively, Hackmann, 'Scientific instruments'; Hacking, *Representing and intervening*, part 2; Kuhn, *The structure of scientific revolutions*; and Riley, *The eighteenth-century campaign to avoid disease*.

The assumed disjunction of language and world is the core constituent of what I call the **dualist conception of language.** Other constituents of the conception vary somewhat. On the one hand, *realist philosophers* have argued that the physical world is mind-independent and naturally pre-structured, yet responsive to scientific inquiry and epistemically accessible. The philosophically primary function of language, they have assumed, is to be 'about' these structures and other such non-linguistic contents of the world. At the very least, language lends itself to the formulation of 'true statements' about the world's contents. *Anti-realist philosophers*, on the other hand, have denied that the non-linguistic physical world, if mind-independent, is epistemically accessible. The primary function of language according to them is to be about phenomena (appearances) and observations — these mind-dependent experiences constitute the genuine and epistemically accessible subject matter of science according to anti-realists.

Briefly put, both realists and anti-realists have assumed that the language/world disjunction is bridged by an 'aboutness' relationship of language to world. For both parties the primary function of language in science is to be about the world, even though realist and anti-realist notions of 'world' (and the subject matter of science) are very different. It will be seen that the core constituent of the dualist conception of language (namely the language/world disjunction assumption) is almost always accompanied by the aboutness assumption predicated on some notion of the world with respect to which language — in the hands of scientists — exercises its primary function.[6]

[6]The following unattributed philosophical statements, covering the spectrum from realism to anti-realism and idealism, all depend on the presuppositions of what I have called dualism:

(i) *There is a world*: there exists a physical world entirely independent of us (nothing about it hinges on how we perceive it, what we think is true of it or how we represent it) which we call 'reality'. Exact details about this world are accessible and often revealed to us in the course of scientific inquiry. Scientists aim to reveal the world as it is and in as much detail as possible.

(ii) *There is a world but we cannot know it*: there exists a physical world entirely independent of us (nothing about it hinges on how we perceive it, what we think is true of it or how represent it) but also *inaccessible* to us that we call 'reality'. Scientists construct and work within a 'scientific reality' of their own, although any correspondence between this and 'reality' is unknowable.

(iii) *There might be a world (for philosophers)*: 'reality' is always an interpretation of experience beyond which it is not the business of scientists to go, although it is open to philosophical speculation. 'Scientific reality' is constituted of those interpretations that seem unshakeable. That they are unshakeable is not a problem for scientists, but it is likely to be one for philosophers who would have to go beyond 'scientific reality' in order to explain their apparent certainty.

Karl Popper once wrote that 'language itself, like a bird's nest, is an unintended by-product of actions which were directed at other aims', adding that 'language, at first merely a means of communicating descriptions of prelinguistic objects, becomes an essential part of the scientific enterprise', essential, that is, to the formulation of theories.[7] But as I shall attempt to show in Part 1, besides communicating descriptions of prelinguistic objects and formulating theories, few other significant functions have accrued to language in the philosophy of science. One may wish for an indication, even at this early stage, of why that has come about.

Philosophers discover science largely through a series of texts. They do not often visit laboratories or practice science.[8] Scientific publications cited in philosophical writings conventionally represent a late stage in scientific inquiry, in the sense that a lot precedes and goes into a publication while even more is left out. In many ways publications are central to scientific practice, yet reading or drafting papers is not what scientists do most of the time, nor is it normally a task that draws on skills they value and are valued for most. One historian of science writes that the published narrative 'is the outcome of a complex process whereby an extended series of experiments is translated and condensed into prose'. It is a highly artificial product — the 'literary remains' of

(iv) *If there were a world we could not know it (there isn't one)*: 'reality' is always an interpretation of experience and scientists and philosophers are bound within that reality. It makes no sense to want to know anything whatever about the physical world stipulated by some. A number of scientific interpretations seem unshakeable, however. It makes no sense to ask why they are unshakeable, nevertheless they constitute reality.

The dualist conception of language is present in all four accounts of 'reality' despite the fact that they are so different. In (i) language is assumed to be transparent and the real world in principle visible. In (ii) the language used to construct 'scientific reality' cannot be known to access the real world. In (iii) reality has been reduced to an object of speculation, and language (which now constitutes 'reality') is distinguished from experience which it serves to interpret. In (iv) reality has been reduced to nonsense and language is again distinguished from experience as in (iii).

[7]Popper, *Objective knowledge*, pp. 117 and 136.

[8]Cf. Hacking, *Representing and intervening*, pp. 149-150. Popper occasionally writes as if science were nothing but a text: 'what is relevant for epistemology is the study of scientific problems and problem situations, of scientific conjectures, of scientific discussions, of critical arguments, and of the role played by evidence in arguments; and therefore of scientific journals and books, and of experiments and their evaluation in scientific arguments' (*Objective knowledge*, p. 111), which are all imagined to take a textual form, that is 'sentences' and 'statements', as I shall explain in Part 1.

experimental life.[9] Another observes that 'at its lapidary best, scientific prose is a most effective instrument of communication: it bears no relationship, however, to any form of speech that ever passed a scientist's lips'.[10]

Crucially, of course, publications *endure* and are relatively accessible. Philosophers experience science predominantly in print, and it is perhaps for this reason that no other product of science has had as much value attached to it in day-to-day philosophy as scientific publications and their epistemic content. That content has stood for completed or rational science, and has been contrasted with 'science in the making', which few philosophers have been concerned to characterise.[11] It is in the printed products of science that scientific arguments, debates, theories, experiments, observations, methodology, and rationality have been thought by many to rest in the most unclouded and accessible form. As I shall argue in Part 2, a peculiar rhetoric is operative in published science (the rhetoric of 'out-thereness'), which renders language transparent and unworthy of comment. My impression is that the traditional focus on *printed* science goes some way towards explaining why Popper and others have tended to regard scientific language as primarily an instrument for the communication of natural facts and the expression of theories (see Part 1.3.2 for more on the origins of that tendency).[12]

Three other factors may be put forth at this stage as likely explanations of the relative neglect of language in philosophy of science. First, many philosophers have trodden a separate path from historians of science. Genuine history and philosophy of science is only a very recent phenomenon. Most philosophers have understood the separation of paths in terms of the distinction between the context of discovery and the context of justification. They have *chosen* to leave matters of language to chroniclers of the

[9]Cantor, 'The rhetoric of experiment', pp. 159 and 160.

[10]Porter, 'Introduction', p. 8. On the 'official' view of scientific language see also Weininger, 'The evolution of literature and science as a discipline', pp. xv-xvi.

[11]Many philosophers still uphold a distinction between the so-called 'context of discovery' and the 'context of justification'. The context of discovery tells the history of a particular piece of knowledge, whereas the context of justification explains its 'content' and the reasons for accepting it. On these matters see especially Reichenbach, *The rise of scientific philosophy*, p. 231, and Popper, *The logic of scientific discovery*, p. 31. For a more recent exposition see Lakatos, 'History of science and its rational reconstructions', section 1E. Traditionally philosophers have avoided venturing into the context of discovery — although that is less true now than it was twenty years ago. For a strong criticism of the distinction see Feyerabend, *Against method*, pp. 152 f.

[12]See also Gooding, 'How to be a good empiricist', p. 421.

former context because they do not see them as pertaining to the 'rationality' of science. Alternatively they have seen them as too theoretically peripheral, historical, or social.[13]

Second, philosophers of science have traditionally written and read what is often referred to as 'history of ideas'. An assumption of this kind of history is that 'ideas' can be followed through history by following the language ('epidemic', 'evolution') in which they are expressed. That is, an idea maintains its identity through time by virtue of a standard set of terms that express it. The 'referents' of these terms change with time, but because the terms themselves do not change, the idea they represent outlasts many generations of scientists and survives different contexts of employment. Unlike philosophers, contemporary historians of science have shunned the history of ideas and questioned the de-contextualised and ahistorical view of language implicit in it.[14]

Third, some philosophers have argued that in the discipline of social science the nature of the observed object is such that social scientists are seldom free to *bestow* language (and meaning) on that which they observe because their observations take in situations and relations that are already meaningful to people and communities. This is supposedly in contrast to natural scientists who deal with events that have no linguistic character prior to it being bestowed on them.[15] The acceptance of this contrast in philosophy renders uses of language in *science* unproblematic.

Scientists create new language and technologies of language: the stream of novel scientific jargon, mathematical and other notations and the information channels and processing systems that underlie impressive scientific publications such as *Nature* or *The Lancet* testify to this. Of course, science, or natural philosophy, has always been a highly literate activity, an activity enmeshing literary and practical reasoning. Philosophers interested in the nature of knowledge acquisition need ways of taking the

[13]On the discovery/justification distinction see note 11 above. On the relation of history and philosophy of science, see Finocchiaro, 'The uses of history in the interpretation of science'; Giere, 'History and philosophy of science'; McMullin, 'History and philosophy of science'; Wartofsky, 'The relation between philosophy of science and history of science'; Burian, 'More than a marriage of convenience'; and Kuhn, *The essential tension*, pp. 3-20.

[14]For an example in the history of ideas see McMullin, 'From matter to mass'. For a vitriolic attack on this kind of history, see Williams, 'Should philosophers be allowed to write history?'. See also Cantor, 'The rhetoric of experiment', especially p. 162; Fleck, *Genesis and development of a scientific fact*, pp. 38 f.; and Kelley, 'What is happening to the history of ideas?'. For a philosopher's criticism see Feyerabend, 'How to be a good empiricist', pp. 183 f.

[15]See, for example, Winch, *The idea of a social science*; and Mulkay, *Sociology of science*, p. xvi.

literary aspects of science into account. I argue in this thesis that sophisticated uses of language in science should have a counterpart in philosophical theory and characterisations of science. It is within the scope of philosophy of science to ask how the scientific article and its knowledge claims are put together — where do they come from? How does something that started off as an experiment, colour changes in a test tube, a series of instrument readings — an 'experience' even? — etc., end up as a handsome page in *Nature*.

It would be misguided, of course, to list everything one is prepared to identify as language and its functions in science and expect this to lead to a philosophically interesting result. Printed words in a scientific journal become philosophically interesting only after it is said of them that they are a 'means of representation',[16] an 'instrument' enabling us to communicate experience or 'express knowledge linguistically',[17] a way of 'making sense' of natural facts or experiments, a 'system of signs and symbols',[18] or that 'empirical scientific statements *speak of our experiences*',[19] that language records the 'objective contents of thought',[20] that 'terms in a mature scientific theory typically refer',[21] that 'terms carry with them an ontology',[22] that languages employed in the sciences 'are designed to make possible the construction of unambiguous expressions that can be true or false',[23] that a particular language might be '*appropriate* to the subject matter of science',[24] that 'within science observational vocabulary enjoys a certain ultimacy',[25] or some other claim of this order.[26] For it

[16]See, for instance, Feyerabend, *Farewell to reason*, p. 107.

[17]Juhos, 'Moritz Schlick', p. 321.

[18]Popper, *The logic of scientific discovery*, p. 59. See also idem, *Objective knowledge*, p. 235; and Kuhn, *The essential tension*, pp. 300-301 and 312.

[19]Popper, *The logic of scientific discovery*, p. 94, my emphasis.

[20]Popper, *Objective knowledge*, pp. 106-107

[21]Putnam, 'Language and reality', p. 290.

[22]McMullin, 'A case for scientific realism', p. 9.

[23]Juhos, 'Moritz Schlick', p. 321.

[24]Rudwick, 'The emergence of a visual language for geological science', p. 177.

[25]Danto, 'Problems of the philosophy of science', p. 297.

[26]For other examples see Popper, *The logic of scientific discovery*, p. 59; and Bazerman, *Shaping written knowledge*, pp. 13, 26, 32, and 188 (scientists engage in 'symbolic processing', p. 190). Sapir talks about 'the nature of language as a symbolic system, a method of referring to all possible types of experience' (Mandelbaum, *Selected writings of Edward Sapir*, p. 158). Note also Langer's claim (in *Philosophy in a new key*) that human beings are symbolising creatures, constantly engaged in the process of producing symbols as a means of categorising and organising their world.

then becomes an issue whether it is correct to say that a scientific publication that emerges out of scientific practice *represents*; that the experience of scientists is in need of an instrument to make it communicable; that natural facts or experiments exist prior to a language that makes sense of them; and so on. It is when language is presented as a distinguishable feature of scientific activity (distinguishable from scientific experience, facts, experimental procedures, the evaluation of procedures, manipulations of instruments and figures, the consideration of objections, the acquisition of skills, the extension of skills to new areas of research, the development of models, imagination, and thought), that room is created for philosophical discussion: for in all these cases the world is distinguished from the making of our conceptualisations of it.

In Parts 1 and 2 I argue there is no defensible version of the view that the world as constituted by scientific inquiry is independent of language and actions taken with language.[27] New scientific terms gain currency not by establishing a descriptive or referential relationship to a non-linguistic world, but through argument, negotiation, and skilled laboratory practice. Rather like a pure sample of thyrotropin in a test tube, language is better thought of not as 'symbolic representation', as many have claimed, but as a device on or by which science is *done*. It creates new ways of thinking about and viewing things. It creates new experience (or extends the old). If as Popper says it is a 'system of signs and symbols', it is so only in the most superficial sense (in the sense that all printed words are human artefacts, and most human artefacts can be viewed as signs and symbols).[28]

Some philosophers are bound to suspect that too much attention to language inevitably leads to the illusion that, in some sense, language is *all there is* — to a 'philosophy

[27]There is no presumption here that language ('words', 'meanings', whatever) functions in science in a philosophically unique way. There are no substantial differences between scientific and non-scientific practices of interpretation and reasoning. The issues this thesis concentrates on are discussed with reference to works by philosophers and historians of science. Similar issues have been discussed in relation to other disciplines. In recent literary criticism, for example, 'language', 'representation', and 'reality' are notions whose problematisation is of primary importance. But whereas in discussions about literature the representability of language is always problematical, a source of tension and metaphor, in writings about science it is supposedly a settled affair. Here the dualist picture is dominant (cf. Rorty, *Philosophy and the mirror of nature*, p. 333). It is in order to counteract this train of thought that I concentrate on language in science in this thesis.

[28]Popper, *The logic of scientific discovery*, p. 59.

which cares only about language, and not about the world'.[29] But they would be wrong: there's no reason to doubt the existence of the substance thyrotropin, or find interest in such a doubt. The interest lies elsewhere — for example, in how the existence of thyrotropin (from a point in time when it was not a substance but a hypothesis associated with a series of experiments, piles of printout alongside notebooks and draft graphs and papers on a bench in a particular laboratory) came to be regarded as a fact; how the meaning of 'thyrotropin' became fixed and public (independent of any laboratory, of any social context); and how the fact, the negotiations that led to it and all its uses have been dependent upon conventional uses of language and compositional traditions. Similarly, there's no reason to doubt the existence of a kind of physical world — what is interesting instead is that our understanding of and action upon that world appears possible in large measure because of our ability to use language (in speaking, thinking, writing, acting) in complex ways. To ask about our understanding of and action upon the physical world is at least in part to ask about the uses to which scientists put language — it is to ask about how mice and chemicals are transformed into words and graphs on paper, about the 'process of construction of sense'.[30] For historians and philosophers these questions are answerable without resort to the dualist conception of language, as I shall attempt to show in Part 2 of this thesis.

Before moving on to consider manifestations of dualism in Anglo-American philosophy of science, I would like to suggest the existence of *two worlds*. The postulation of these worlds will serve to sharpen my definition of dualism and help expose (in Part 1) the proponents of the dualist conception of language, both realists and anti-realists.

I distinguish between the physical world as understood and interacted with by scientists — which I call 'physical world A' (the history of science has been a succession of physical worlds of this kind) — and the world as it exists independently of scientific (or any) understanding and interaction — which I call 'physical world B'. By definition, I have no intellectual familiarity with world B, even though I believe in (or postulate) its existence.

[29]Russell, in foreword to Gellner, *Words and things*, p. xv. In a similar fashion, Popper is concerned that 'the study of the growth of knowledge can be replaced by the study of linguistic usages' (*The logic of scientific discovery*, p. 16). These fears have been given foundation by anti-realists for whom it is possible to debate whether understanding science entails anything *more than* analysing discourse. See, for example, Gregory, *Inventing reality*; and Gilbert and Mulkay, 'Experiments are the key'.
[30]Latour and Woolgar, *Laboratory life*, p. 32.

World A is best thought of as an *interpreted* physical world. It involves all the results of scientific investigations up to some point in time, results generally accepted as plausible or correct at the time. Presently, for example, world A contains atoms. From an alternative, complementary point of view, world A is the physical world as understood and interacted with by *individual* scientists — as it is expressed in their imaginations, conversations, written and published work, and as it is manifested to them in laboratories, observatories, and elsewhere. Scientists sharing the same speciality will generally have much of their world As in common, whereas scientists from different specialities will have less. (That is to say, their world As will be developed and overlap to a different extent. Practically all of them, however, will contain atoms of some degree of complexity.) Because the defining mark of physical world A is that it is *interpreted*, one may speak of world A without distinguishing individual from more comprehensive (collective, communal) levels of interpretation.

Although world A is a *physical* world, not everything that is part of it exists in the way atoms do (for example, hypothetical but as yet unconstructed instruments, black holes, thought experiments, etc., do not exist as atoms do). As a physical world, it is entirely unrelated to Popper's third world of 'objective contents of thought' (see Part 1.1.3, below). It is not reducible to 'concepts' or 'pictures' existing in scientists' minds, or to a world *view*. In world A lasers burn holes in the coats of forgetful experimenters, and when properly incorporated into polarising electron guns cause the release of polarised electrons.

Whereas physical world A involves scientists and their achievements, *world B* does not depend on scientists, or human beings in general. It is not a physical world *from* anyone's point of view or mode of interaction. If humankind were to meet its end tomorrow, world B would carry on unaffected. In particular, *there are no atoms (no planets, forces, orbits, etc.) in world B.* Atoms (and the rest) belong to world A. Unlike world B, world A would cease to exist were humankind to meet its end tomorrow, and so would atoms. Although this view may seem outrageous, it should be noted that at least in its consequences for atoms it differs both from philosophical realism and anti-realism. Atoms physically exist and do not exist, in my view, depending on the world in question. (Worlds A and B are both 'physical', but in different senses of the word: the former is physical in the stone-kicking sense; the latter is physical as opposed to 'interpreted' or 'mental'.)

Realist philosophers who find the *initial* terms (not including the last paragraph) of the distinction between the two worlds acceptable, are likely to say that much of what exists in world A exists in world B too — and that it exists in world A *because* it exists in world B. For example, they might say that atoms existing in world A are very much like atoms existing in world B, and that in this day and age scientists who manipulate atoms in world A can with near-certainty be said to be manipulating very similar (or the same) entities in world B. In other words, these philosophers will claim a *relation* between worlds A and B, the relation being one of similarity of world A to aspects of world B, the former physical world being an approximate (idealised) version of the latter. Realists such as Popper will defend the *sense* of that relation while rejecting the supposition that empirical knowledge can be certain.

Anti-realists (again, if they feel happy with the initial distinction) will deny the relation of similarity claimed by realists, either by denying that scientists intend world A to be like world B, or by denying that there is any sense in believing that world A is like world B, or by denying the existence of world B. Of course, anti-realists need not adopt the third, more radical, course; their claim might be only that world A (being for them a world of physical *phenomena*, perceptions, or sense data) is categorically different from world B, and the two worlds are not, therefore, comparable. At most, in their view, a relation exists between worlds A and B only insofar as the former 'saves the phenomena' as they appear to us from our situation within the latter.

Table 1. Language/world dualism from realist and anti-realist perspectives against the background of worlds A and B (the arrows represent an 'aboutness' relationship).

		metaphysical		
Realism	*Language*	——→	*world* **A**	*... which is the same as, or is the (with or without certainty) 'known' component of world B*
Anti-realism	*Language*	——→	*world* **A**	*... which is the world manifested in mind-dependent perception; world **B** is either beyond reach or does not exist*
		divide		

To summarise the dualist conception of language against the backdrop of my two worlds: for realists, language in science refers to a mind-independent reality; more precisely, it functions to refer to the physical constitution of world A, and thereby (in principle) the physical constitution of world B. For anti-realists, language in science refers to a mind-dependent reality; more precisely, it functions to refer to certain appearances in world A; it can say nothing that is sensible about features of world B.

See Table 1 for a summary representation of realist and anti-realist views on language as so far discussed.

As for what *scientists* might say about the two-world distinction, it is difficult to say. Perhaps it would be fair to attribute a position to them as follows. Along with almost everybody else, scientists believe in world B to the extent only that it denotes a physical world that does not depend on human existence; but for them there is very likely no relation and no distinction between worlds B and A. What I am calling world A for them is the sum of aspects of world B that have so far been investigated, whether successfully (DNA structure), approximately (viral actions), or speculatively (black holes).

According to the view I am advocating, however, world B is not the place where atoms do, or could conceivably exist. When I say that atoms exist in world A, I mean that full-blooded atoms exist, not just atoms in the mind or on paper. I have already stressed that world A is a *physical* world. Scientists, especially in the second half of our century, have employed (world A) atoms to get a variety of physical tasks done. That world A would cease to be — that it would 'pop out of existence' — were humankind to, does not make it less physical while it exists. As an *interpreted* physical world, world A's continued existence depends on the continued existence of a scientific community and perspective. Once these are gone, world A, like much else that is dependent on human agency and organisation, will disappear too. So will atoms. Only world B will remain.

It will be said: why can't atoms exist in world B? To which the short answer is: they were never *discovered* in world B. They were discovered in world A — in the physical world that has been constructed and reconstructed over the centuries by human beings pursuing scientific interests in world B. To put the answer differently: the ways we imagine atoms, the ways we characterise them and bring them into our calculations, our understanding of the practical uses we make of them, and so on, are all features of world A. We have no conception, as I have said, of atoms beyond that world. All our atomic conceptions are world-A conceptions, and 'atom' gets its *sense* in that world.

What, then, *does* exist in world B? The question has no answer. Not because we can't be sure about what exists there, but because world B is not an interpreted physical world — it has no ontology. To attempt to describe it, or interact with it as if it were a meaningful world, is to re-construct an interpreted world A (or A'). We cannot even properly say that world B is chaotic, or dishevelled, or unschematic, for that would be to impose a particular interpretation on it. Obviously, then, there is nothing to be said about the *contents* of world B that would not constitute an interpretation. It does not matter how certain we may feel about a process or thing existing in world A: that (or a similar) process or thing cannot sensibly be said to exist in world B (processes and things are world A notions). We cannot, without undermining our conceptual grasp on world B, attempt to think of it in terms similar to those we use to think of world A.[31]

Is world B, therefore, a mystery? It would be a mystery if it were an *interpreted*, ontology-endowed world, which was inaccessible to us. Once we see that the difference between world A (A', A", etc.) and world B is more than just a matter of access, it might be easier to come to terms with my characterisation — and indeed the existence — of world B. (An imperfect analogy: we may think of an unworked block of clay, and then of a row of pots made from it. To think that the latter existed in the former would be to add at least a touch of mystery to the block of clay.) World B is of no interest to us as scientists and of marginal interest to us as philosophers.

Yet, it will be said: world A *depends* on world B. This is true: in my view the former is the result of a special brand of human agency and organisation exercised in the latter. It depends, therefore, on the latter, as much as it depends on human beings. But if so, doesn't world B need to be *in some manner*? — how are we to explain our sense that the world A of modern science is not just an arbitrary world, replaceable in principle or at whim with a radically different world A', if world A cannot be said to be constrained by what world B *is like*? Physical world A certainly is not arbitrary; our sense of its non-arbitrariness arises from our recurrent and largely predictable interactions with it, not from what we believe *world B* to be like. We are normally able to explain predictability sufficiently well in world-A terms, without resorting to world B's supposed make-up. World A is self-contained in this respect. However, even though no world A has been constrained by what world B *is like*, every world A *has been constrained by world B* — although nothing can be said about the contents of world B, at the interface with world A it often appears stubborn and resistant.

The questions and issues raised above are addressed more fully in Part 2. Here is not the place to embark on a defence of the distinction between the interpreted and uninterpreted worlds. The framework they provide will become clearer as other arguments are developed. One final remark on the proper place of language in my bifurcated cosmos: the uninterpreted world B is not in any sense the object of language

[31]As Goodman notes, 'Talk of unstructured content or an unconceptualised given or a substratum without properties is self-defeating; for the talk imposes structure, conceptualises, ascribes properties' (*Ways of worldmaking*, p. 6).

(or knowledge). According to the conception of language I wish to defend, the interface of the interpreted and uninterpreted worlds is a space where language functions to produce knowledge, but the functions of language (and the nature of knowledge) are to be understood solely within the limits of an already interpreted world A. In other words, language functions within world A, and falls or rises with world A. To this extent language and world B *are* dissociated — *everything* that is part of world A is logically dissociated from world B. Within world A, however, there is no disjunction of language and the subject matter of science (unlike the two positions represented in Table 1) — or so I set out to argue.

PART 1

THE DUALIST CONCEPTION OF LANGUAGE AND ITS FUNCTIONS IN SCIENCE

The old language-world bipartition: the simplest, most trivial element at the basis of every sane account of the nature of knowledge.[1] (Coffa)

I think ... in terms of language and the world.[2] (Quine)

Philosophers and other writers on science have often made assumptions or included remarks about language in their writings while not engaging with issues of language — or rather without recognising that there is a set of philosophically relevant issues with which they have to some extent engaged. In order to discuss assumptions and remarks of this kind it would seem reasonable, as a first step, to assemble a cross-section of actual examples from writings in the philosophy of science. That is the primary task of Part 1. The secondary, though still important task, is to argue, first, that philosophers of science *do* regularly make metaphysical assumptions about language, and that these assumptions are as a rule left unclarified, under-developed, or unjustified; and, second, that their assumptions about language in science are not isolated occurrences, but significant components of philosophical doctrines they espouse.

1.1 Popper on language

To say that philosophers of science have engaged with a philosophically relevant set of issues about language but seldomly recognised that engagement might seem a bold claim. It is proper, then, to begin with a striking example of one such case.

1.1.1

In the preface to the first English edition (1959) of *The logic of scientific discovery*, Karl Popper undertakes to explain his attitude towards linguistic philosophy. He regards the work of ordinary language philosophers uninteresting because it holds no hope of teaching us anything new about the 'advance' of scientific knowledge; and it does not aspire to contribute to it.[3] He finds that the same criticism applies to those philosophers who from an analysis of the language of science construct 'miniature

[1]Coffa, *The semantic tradition from Kant to Carnap*, p. 357.

[2]Quine, *Theories and things*, p. 41.

[3]Popper, *The logic of scientific discovery*, p. 19.

model languages' for the purpose of expressing knowledge 'precisely'.[4] Both groups, he believes, miss what is most exciting about the theory of knowledge.

Popper states in the same preface that if there is one philosophical problem that is self-evidently interesting it is 'the problem of understanding the world — including ourselves, and our knowledge, as part of the world'. The central problem of epistemology, he adds, is the problem of the growth of knowledge.[5] The best way to solve it is to study the 'growth' of scientific knowledge. It is the 'logic' of growth that he intends to explore in the body of the book.

Written twenty-five years after *Logik der Forschung* first appeared, the attack on linguistic philosophy in the English preface exemplifies Popper's disapproval of the language-oriented efforts of positivists to discover a level of certainty in science, and his dislike for Wittgenstein's later philosophy. The attack naturally leads Popper's readers to expect that 'the language of science' is for him a relatively uninteresting object of study, and one that will not feature in his philosophical inquires.

In this respect, however, Popper's attack on linguistic philosophy is misleading. In *Logik der Forschung* and later books assumptions about the language of science (and about language in general) not only abound but can be shown to underpin some of Popper's central arguments and convictions. What is unusual is that he more than most philosophers of science makes his views on language and its functions explicit. He is typical, however, in abstaining from critical engagement with them.

Part 1 of *The logic of scientific discovery* opens with the following assertion:

> A scientist, whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience.[6]

Noteworthy here are, first, Popper's conception of experiment as theory-testing, and, second, his idea that scientists deal in *statements* or systems of statements. These statements, he says, add up to what are better known as hypotheses or systems of theories: 'Science', he emphasises again later, 'is ... a system of *statements*'.[7] Statements are the 'constructs' of scientists and their fate is to be tested against experience.

[4]Ibid., p. 21. He has in mind Carnap, among others.

[5]Ibid., p. 15.

[6]Ibid., p. 27.

[7]Ibid., p. 35.

So commonplace is this idea in Popper's writings (and in post-*Logik der Forschung* works in the philosophy of science generally) that it may seem almost unremarkable today.[8]

Popper has in mind nothing out of the ordinary when he talks about 'statements'. Here is an example of what he calls a *singular* statement: 'There is a raven in the space-time region *k*'.[9] We could just as well call this a *sentence* describing or asserting — or being used to describe or assert — a fact. Popper does not appear to have anything more arcane or restrictive in mind than this.

Let us return then to the claim that scientists put forward statements and test them against experience. Its position at the beginning of the book is not accidental: it is an assumption, an axiom, for which no argument is offered there or elsewhere. But what kind of assumption is it? Two readings suggest themselves: first, Popper is expressing an anthropological view about what scientists (theoreticians and experimentalists alike) actually do at their places of work. Second, he is making a philosophical claim that distinguishes between statements and experience, and holds that the former can be related to the latter by a procedure called 'testing'. In philosophical context, the second reading seems more relevant, although there is no reason to believe that Popper would expect a laboratory observer to notice that scientists do *not* in general test statements against experience.[10] Philosophy of science has never been free of references to actual or imaginary scientific practices, which are used to support philosophical claims. Popper's following remark is an example of such a reference:

[8]For similar claims about the role of statements see, for example, Hacking, *Why does language matter to philosophy?*, pp. 160-161, and 177 f.; and Hanson (who claims that 'knowledge of the world is a system of propositions'), *Patterns of discovery*, p. 26. Cf. Quine, *Theories and things*, pp. 24-25. Popper's student, Musgrave, defending objectivism (or 'impersonal', non-psychologistic knowledge), writes: 'Knowledge is always possessed by some person; the verb "know" requires a *subject*, the "knower" ... [it] also requires an *object*, the statement or proposition which is known' (*Impersonal knowledge*, p. 10). Also Tarski gives prominence to statements: 'Independent of whether a science is conceived merely as a system of statements or as a totality of certain statements and human activities, the study of scientific language constitutes an essential part of the methodological discussion of a science' ('The semantic conception of truth', p. 65).

[9]Popper, *The logic of scientific discovery*, p. 101. An example of a 'universal statement' is the following: 'All planets move in ellipses with an eccentricity other than zero' (p. 376).

[10]This is so even though Popper tries to distance his interests from those of a 'naturalistic methodology' (ibid., p. 52). He does, in any case, believe that the logic of scientific discovery he expounds often reflects actual scientific reasoning. See p. 107 for a perfectly clear statement of this.

> What is the general problem situation in which the scientist finds himself? He has before him a scientific problem: he wants to find a new theory capable of explaining certain experimental facts; facts which the earlier theories successfully explained; others which they could not explain; and some by which they were actually falsified.[11]

This kind of 'thought experiment' is used to lend credibility to philosophical considerations by tying them to a supposed scientific reality. Popper's assumption that scientists deal in statements, or that 'scientific theories are universal statements',[12] is early confirmation of my contention that assumptions about language and its functions in science play an important role in Popper's philosophy. Statements as we have seen are items of language. The formulation of hypotheses and the testing of such statements are supposedly significant *uses* of language in science. The claim that scientists as a matter of course construct and then test statements against experience suggests a kind of *function* for language in science. The claim presupposes a disjunction of the language used to construct statements and the experience against which they are ultimately tested.

The following quotation, in which Popper explains how a new theory is tested, provides further illustration of these points:

> Deductive testing of theories ... always proceeds on [these] lines ... With the help of other statements ... certain singular statements [predictions] are deduced from the theory; especially predictions that are easily testable or applicable. From among these statements, those are selected ... which the current theory contradicts. Next we seek a decision as regards these ... derived statements by comparing them with the results of practical applications and experiments.[13]

Because this is a *generalised* account of deductive testing (which itself is central to the logic of discovery, as Popper sees it), it is reasonable to conclude that the assumptions it contains about language are assumptions about important (if not the most important) functions of language in science: the construction of systems of statements, the derivation of other statements from them, and the comparison of these to 'the results of practical applications and experiments'. If we ask what the nature of such results might be (what do experimenters ultimately produce), we come across more talk about state-

[11]Popper, *Conjectures and refutations*, p. 241. For another example of imaginary practices attributed by Popper to scientists, see Hacking, *Representing and intervening*, pp. 243-244.

[12]Popper, *The logic of scientific discovery*, p. 59.

[13]Ibid., pp. 32 and 33.

ments ('basic statements'; and also: 'primitive propositions'[14]). We also come across assumptions about 'the world' that lies beyond statements ('observable events'), and so of 'facts' that have been split off from interpretations and conceptualisations. In other words, we come across a disjunction of language and world.

1.1.2

The language/world disjunction is emphasised in the course of a complication to which Popper turns his attention: if for the purposes of testing statements and 'the results of practical applications and experiments' are to be *compared* to be tested they have to be of a comparable nature. On some occasions Popper writes as if 'experience' or 'empirical tests' can bear directly on a 'scientific system' (which, of course, is a system of statements): 'it must be possible for an empirical scientific system to be refuted by experience ... [to] be singled out, by means of empirical tests, in a negative sense'.[15] Elsewhere he writes that in the process of testing 'it is ... basic statements which we try to compare with the "facts" and ... we choose these statements, and these facts, because they are most easily comparable'.[16]

How can experience refute statements? Popper is aware of Carnap's argument that in the 'logic of science' we must not say 'that sentences are tested by comparing them with states of affairs or with experiences: we may only say that they can be tested by comparing them with other *sentences*'.[17] Yet Popper does not want the requirement of falsifiability that he is promoting to be merely a logical relation among statements (the theory and the basic statements). 'Theories', after all, 'are nets cast to catch what we call "the world"'.[18] To overcome the problem, Popper expresses himself in what he calls the 'realistic mode of speech':

> In this 'realistic' mode of speech we can say that a singular statement (a basic statement) describes an *occurrence*. Instead of speaking of basic statements which are ruled out or prohibited by a theory, we can say that the theory rules out certain possible occurrences, and that it will be falsified if these possible occurrences do in fact occur ... It is easy enough to define [the use of

[14]In Popper, *Objective knowledge*, p. 124, the right hand side of the displayed table reads: 'Ideas, that is statements or propositions or theories, may be formulated in assertions, which may be true, and their truth may be reduced, by way of derivations, to that of *primitive propositions*' (my emphasis).

[15]Popper, *The logic of scientific discovery*, p. 41.

[16]Popper, *Conjectures and refutations*, p. 267.

[17]Popper, *The logic of scientific discovery*, p. 96; on the restriction that statements can be justified only by statements, see ibid., pp. 43, 93, and 95. See also, Lakatos, 'Falsification and the methodology of scientific research problems', p. 99.

[18]Popper, *The logic of scientific discovery*, p. 59.

> 'occurrence'] so that it is unobjectionable. For we may use it in such a way that whenever we speak of an occurrence, we could speak instead of some of the singular statements which *correspond* to it.[19]

The notion of correspondence plays a key role here: we may speak as if theories are checked against happenings in the world (occurrences) because what we mean in fact is that theories are checked by basic statements that *correspond* to happenings in the world. This relation of basic statements to the physical world is brought into sharper relief by the following requirement:

> a basic statement must also satisfy a material requirement — a requirement concerning the event which, as the basic statement tells us, is occurring at the place *k*. This event must be an 'observable' event; basic statements must be testable, inter-subjectively, by 'observation' ... Admittedly, it is possible to interpret the concept of an observable event in a psychologistic sense. But I am using it in such a sense that it might just as well be replaced by 'an event involving position and movement of macroscopic physical bodies' ... *Basic statements are therefore ... statements asserting that an observable event is occurring in a certain region of space and time.*[20]

This last sentence (with its non-psychologistic rendering of 'observable') presents us with a clear-cut disjunction of language and world.

In the *Logic of scientific discovery* we find Popper's 'epistemological theory of experiment': 'the theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions'.[21] But how is that answer 'elicited'? How does the experimenter move between experience and statement, or between question, 'practical application', and answer? Popper does not consider these questions. He objects instead to the aim of philosophers of the period to explain how statements are *justified* by observation (what he calls 'psychologism'), because one can rationally only *criticise theories*. 'The decision to accept a basic statement', Popper writes, 'is causally connected with our experiences — especially with our perceptual experiences. But we do not attempt to *justify* basic statements by these experiences'.[22] Apart from the passing reference to a 'causal connection', there is no explanation here or elsewhere of how statements and experiences are bridged in practice. That they *are* is taken for granted (and left to psychology to explain).[23] In the battle against psychologism in philosophy Popper

[19]Ibid., p. 88, final emphasis added.

[20]Ibid., pp. 102-103, emphasis added.

[21]Popper, *The logic of scientific discovery*, p. 107.

[22]Ibid., p. 105.

[23]Popper claims that 'Fries, and with him almost all epistemologists who wished to account for our empirical knowledge, opted for psychologism. In sense experience, he taught, we have "immediate

employs a dualist conception of language, with language and world — statements and physical occurrences — belonging to distinct realms, related, however, by description or correspondence.

1.1.3

So far I have wanted to argue that in *The logic of scientific discovery* assumptions about language are both apparent and important. The assumptions involve a disjunction of language and things external to it ('experience', 'occurrences', 'facts'). When Popper asserts that 'scientific theories are universal statements' and that 'like all linguistic representations they are systems of signs and symbols ... [they] are nets cast to catch what we call "the world"'[24] — he is committing himself to a threefold view of language in science: what it is, what it is not, and what it does. He sees proper scientific theories as being heavily dependent upon statements. The criterion of falsifiability is viable only if theories are fully expressible in language:

> a severe test of a system presupposes that it is at the time sufficiently definite and final in form to make it impossible for new assumptions to be smuggled in. In other words, the system must be formulated sufficiently clearly and definitely to make every new assumption easily recognisable for what it is.[25]

> [Knowledge] becomes objective when we *say* what we think; and even more so when we *write* it down, or *print* it.[26]

Thus for Popper an authentic scientific theory must be expressible clearly and definitely in some language. It does not exist in any significant sense in an extra-linguistic or language-independent realm. The view that scientific theories are exhausted in statements of a language (where the language is itself statement-like) is often encountered in philosophy. Quine, for example, says that a scientific theory 'is an idea, one might naturally say, or a complex of ideas. But the most practical way of coming to grips with ideas, and usually the only way, is by way of the words that

knowledge": by this immediate knowledge, we may justify our "mediate knowledge" — knowledge expressed in the symbolism of some language' (*The logic of scientific discovery*, p. 94; see also p. 98). What Popper dislikes about this chain of justification (from sense experience to basic statements to general statements) is that it is a chain of *justification*. For him it is just a causal chain with distinct links.

[24]Popper, *The logic of scientific discovery*, p. 59.

[25]Ibid., p. 71.

[26]Popper, *Objective knowledge*, p. 25. See also, pp. 31, 66, 70, 84, 96, 120, 263, and 266. This essentially anti-psychologistic requirement also applies to mathematics: 'mathematics ... grows through the criticism of guesses, and bold informal proofs. This presupposes the linguistic formulation of these guesses and proofs' (p. 136).

express them'.[27] This anti-psychologistic view is elaborated in Popper's later work where it is also evident that 'language' (a system of statements) is fundamental to his philosophy. In *Objective knowledge* Popper devotes the best part of a chapter (entitled 'Epistemology without a knowing subject') to explain his notion of a 'third world'. He begins with a disjunction of three worlds:

> first, the world of physical objects or of physical states; secondly, the world of states of consciousness, or of mental states, or perhaps of behavioural dispositions to act; and thirdly, the world of *objective contents of thought*, especially of scientific and poetic thoughts and of works of art.[28]

The third world includes 'theoretical systems', but 'inmates just as important are *problems* and *problem situations*'. The most important inmates of this world, according to Popper, 'are *critical arguments* ... and, of course, the contents of journals, books, and libraries'.[29] To the third world belongs scientific knowledge. It is the world, more analytically, 'of objective theories, objective problems, and objective arguments';[30] the world 'of scientific problems and problem situations, of scientific conjectures ... of scientific discussions, of critical arguments ... of experiments and their evaluation in scientific arguments', and so on;[31] it is 'a kind of Platonic (or Bolzanoesque) ... world of books in themselves, theories in themselves, problems in themselves, problem situations in themselves, arguments in themselves'.[32]

What does Popper imagine the nature of such 'objective contents of thought' to be? What do they exist *as*? The answer — present in what has been said already — is straightforward: with the exception of some 'works of art' (objective non-linguistic representations of emotions?), Popper appears to conceive of the third world as consisting entirely of items of *language*. Objective contents of thought, problems, the contents of books, and so on, exist in the third world *expressed in sentences of a*

[27]Quine, *Theories and things*, p. 24.

[28]Popper, *Objective knowledge*, p. 106.

[29]Ibid., p. 107; cf. p. 74.

[30]Ibid., p. 108.

[31]Ibid., p. 111.

[32]Ibid., p. 116. The roots of Popper's third world are to be found in the works of Bolzano and Frege (the latter also saw his arch-enemy in psychologism) — see Wedberg, *A history of philosophy*, p. 22. A three-way distinction similar to Popper's can be found in Ballard (1934): 'We therefore arrive at the general conclusion that as far as language is concerned, the three estates of the realm — the three agencies that make the process effective — are thoughts, words, and things; or, to put it with more scientific precision, thoughts, signs, and things signified' (*Thought and language*, p. 26).

language. This is stated unequivocally: 'Theories, or propositions, or statements are the most important third-world linguistic entities'.[33] Popper speculates that,

> without the development of an exosomatic descriptive language ... there can be no object for our critical discussion. But with the development of a descriptive language (and further, of a written language), a *linguistic third world* can emerge; and it is only in this way, and only in this third world, that the problems and standards of rational criticism can develop.[34]

The third world in Popper's scheme is but 'a by-product of human language'.[35]

It is not necessary for the purposes of this thesis to explore every detail of, or motivation for, the third world.[36] I wish only to comment on the sharp distinction Popper wishes to draw between language-expressed scientific knowledge (the third world) and the *first world* 'of physical objects or of physical states'. Noteworthy too is what the third world 'does', or *in what relations it stands*. It is the product of scientific inquiry (which involves the psychological processes belonging to the 'second world'): it is 'a human product'.[37] Yet despite its origins in the second world, the third world, because it is 'objective', because it is language-laden, and because language is characterised by Popper as being primarily 'descriptive' (see below), is bound in one direction only: in the direction of the first world. The third world of language is *about* 'physical objects' and 'physical states'.[38] It is, Popper suggests,

> possible to accept the reality or ... the autonomy of the third world, and at the same time to admit that the third world originates as a product of human activity. One can even admit that the third world is man-made and, in a very clear sense, superhuman at the same time.[39]

[33]Popper, *Objective knowledge*, p. 157.

[34]Ibid., p. 120, my emphasis. Cf.: 'it is an essential part of being human to learn a language and this means, essentially, to learn to grasp *objective thought contents*' (ibid., p. 156).

[35]Ibid., p. 117; see also pp. 165 and 240-241. As I mentioned in the Introduction, Popper wrote in *Objective Knowledge* that language, while 'at first merely a means of communicating descriptions of prelinguistic objects, becomes [as a result of its use in the formulation of hypotheses] an *essential part* of the scientific enterprise ... which in its turn becomes part of the third world' (p. 136).

[36]For a critical assessment of the third world theory that goes beyond my own concerns with language, see Toulmin, 'History, praxis and the "third world"', pp. 662 f.

[37]Ibid., p. 116; see also p. 117.

[38]Thus Popper assumes that if the human race was wiped off the face of the earth, Martians should be able to read our books and have access to our knowledge (ibid., p. 116).

[39]Ibid., p. 159.

It is superhuman in the sense that its *object* — that which it is 'about' — exists independently of everything human — or at least of everything human that falls within the psychologistic second world. This independence of the object bestows on the language that expresses it as 'knowledge' an objective, 'autonomous', non-psychological status.[40] The inmates of the third world are essentially *descriptions* of what falls within the first world.[41] It is description, according to Popper, that is one of the primary functions of language:

> The most important of human creations ... are the higher functions of human language; more especially the *descriptive function* ... With the descriptive function of human language, the regulative idea of *truth* emerges, that is, of a description which fits the facts.[42]

This assumption about language and its main function is quite fundamental to Popper's philosophy and is prior to the three-world theory:

> human language is essentially descriptive ... and an unambiguous description is always realistic: it is *of* something — of some state of affairs which may be real or imaginary. Thus if the state of affairs is imaginary, then the description is simply false.[43]

[40]Woolgar remarks: 'The rationalists ... are committed to the view that there exists a world of material things which do not depend for their existence on the fact that some mind is aware of them. Translated into more sociologically familiar terms, this means that the truth or otherwise of the matter is independent of the presence of social actors. In Popper, we find knowledge products preserved in the social equivalent of a hermetically sealed domain: the third world. Knowledge thus constituted is independent of the knowing subject' ('Discovery', p. 240).

[41]Not so for mathematics: Popper praises Brouwer for solving 'the problem [created by Kant's exclusion of discursive arguments from geometry and arithmetic] by making a sharp distinction between *mathematics as such* and *its linguistic expression and communication*. Mathematics itself he saw as an extra-linguistic activity, essentially an activity of mental construction on the basis of our pure intuition of time. By way of this construction we create in our intuition, in our mind, the objects of mathematics which afterwards — after their creation — we can try to describe and to convey to others' (*Objective knowledge*, p. 132). Popper agrees that in mathematical activity two things are involved: mental activity and its articulation in language. But the language in this case appears to have the second world (mental contents) as its object.

[42]Popper, *Objective knowledge*, pp. 119-120; cf.: 'description, including the description of conjectured states of affairs, which we formulate in the form of theories or hypotheses, is clearly an extremely important function of human language' (p. 236). On the descriptive function see also pp. 70, 84, 92-93, 122, 237-239, and 263; and idem, *Conjectures and refutations*, pp. 63, 134, and 295.

[43]Popper, *Objective knowledge*, p. 41.

The three-world theory could hardly arise in the absence of this conviction that the function of language is to relate the linguistic realm to the realm of objectively existing things (and ultimately relate mental states to the latter).[44] Yet nowhere does Popper explain what a descriptive language is, what the descriptive function amounts to beyond 'correspondence', what it is for a description to be *of* something or 'fit' some state of affairs.

The third world is presented as the repository of objective knowledge, but it is always language that expresses its knowledge items. Popper does not see the language-dependence of these as a possible source of philosophical problems. Nor does he express any concern about the descriptive function of language, for this is imagined to be an 'essential' (obvious?) function.[45] Moreover, he does not examine the emergence of the third world from the second, or rather its emergence from the interaction of the second with the first, which would naturally lead to questions about actual uses of language in scientific inquiry. For Popper, in what he calls a Platonic or Bolzanoesque kind of way, inmates of the third world *predate* their 'discovery' by the scientific mind: 'I assert', he writes, that even though the third world is a human product 'there are many theories in themselves and arguments in themselves and problem situations in themselves which [although inmates of the third world] have never been produced or understood by men'.[46] He does not find it necessary to examine the emergence of the third world from the second because it does not really emerge from the second world at all: it only *appears* to do so from our position within the second world, as we go about increasing our (linguistic) grasp on the third world. In reality the latter fully reflects the first world, no matter where we happen to be with our investigations.

Something more about Popper's assumptions about language and its functions in science may be gleaned from a look at remarks of his on the *aim* of science. I have mentioned already Popper's view that what scientists do is test statements against experience. But to what end? 'All work in science is work directed towards the

[44]Cf. Coffa, on propositions: 'Propositions are tools for conveying information about how things stand; they tell us something about "the world" in such a way that what they say agrees or fails to agree with the facts. With each proposition ... we must have two different factors: a fragment of reality and the association or mode of correlation between the statement and the reality in question' (*The semantic tradition*, p. 248).

[45]Popper *does say* in one instance that 'understanding the functions of our language is an important part of [the problem of understanding the world]' (*The logic of scientific discovery*, p. 15).

[46]Popper, *Objective knowledge*, p. 116; for 'discovery' see p. 74.

growth of objective knowledge',[47] he writes, which involves furthering scientists' access to the third world. When not elaborating the three-world theory, however, Popper also views *explanation* as an aim of science: 'what we attempt in science is to describe and ... explain reality'.[48] He says elsewhere that the aim of science is to find satisfactory explanations, and that 'by an *explanation* ... is meant a set of statements by which one describes the state of affairs to be explained'.[49] (We see here that Popper's version of language/world dualism entails that a good explanation is little more than an accurate description.)

Also noteworthy here is a particular use of the indefinite and definite articles. '*A* set of statements' by which one describes '*the* state of affairs'. The guiding assumption behind this variation is that a 'physical object or physical state' is a definite pre-determined pre-existing matter of fact, whereas hypotheses about it (framed, of course, in language) may vary indefinitely. We find an identical choice of articles in one of Popper's endorsements of Tarski's theory of truth:

> All this was changed by Tarski's theory of truth and of the correspondence of *a* statement with *the* facts. Tarski's greatest achievement ... is that he rehabilitated the correspondence theory of absolute or objective truth ... the intuitive idea of truth as correspondence to the facts.[50]

The dualist distinction between language and world is thus found combined with the belief that states of affairs or facts in the world are targeted by linguistic formulations of science that either successfully correspond or fail to correspond to their targets. Popper's assumption that that is the case is evidently also an assumption about how language is used in scientific practice: language is used to produce those statements that correspond to the facts, with testing eliminating those that do not.[51]

1.1.4

In what has gone so far I have made reference to assumptions about language in science as they exist in Popper's writings. They may be summarised as follows. *What language is*: (i) statements or systems of statements are the significant forms that

[47]Ibid., p. 121.

[48]Ibid., p. 40.

[49]Ibid., p. 191.

[50]Popper, *Conjectures and refutations*, p. 223, my emphasis. See also idem, *Objective knowledge*, p. 44.

[51]Cf. Hesse: 'In the network model [which she defends], all sentences of a theoretical system have truth value in a sense which has been defined as the correspondence with the world of statements expressed in a given descriptive language' (*The structure of scientific inference*, p. 293).

language takes in science; (ii) statements, and therefore language, are firmly distinguished from: experience, observations, experiments, physical objects, physical states of affairs, facts, and the first world of physical reality (language is 'about' all of the above). *Functions and uses of language*: (i) scientific language is about the physical world; statements correspond or fail to correspond to facts; (ii) one of the most important functions of language is the descriptive function; scientists use language to propose descriptions of physical reality and to test them against experience; (iii) language or statements that express scientific knowledge occupy a distinct position (the third world) between the worlds of physical objects and states of consciousness. Also operative is an assumption about *the physical world*: although the workings of the world are not knowable with certainty, the world consists of objective facts, with which scientists are becoming increasingly familiar.

In this list of assumptions are found the elements of the dualistic conception of language. First, language and world are disjointed (this being the core of dualism, as I have defined it). Second, the primary function of language is to be about the world. A third element, that the world to which language reaches out has a mind-independent structure, makes this the version of dualism typically expressed by realists. Because language in Popper's scheme functions merely to describe, or be about, the contents of the first world of physical objects and physical states, the scientific determination of those contents prior to their description must be, in a fundamental sense, an extra-linguistic activity. Thus the role of language in the exploratory stages of scientific inquiry is deflated.

Popper advises that we keep in mind the distinction between 'problems connected with our *personal contributions* to the production of scientific knowledge on the one hand' and 'problems connected with the *structure* of the various products, such as scientific theories or scientific arguments, on the other'.[52] Language, Popper repeatedly states, is the means of expression of *all* scientific knowledge (theories and arguments alike). It follows from these two statements that language is connected with the 'structure' of scientific knowledge. But is language not also a 'personal contribution' of ours to the production of scientific knowledge? Popper does not say. He does, however, note the following: 'I do admit that in order to belong to the third world of objective knowledge, a book should be capable of being grasped (or deciphered, or understood, or 'known') by somebody'.[53] This admission puts pressure on the boundary Popper is keen to set up between states of consciousness and objective knowledge. Understanding a book

[52]Popper, *Objective Knowledge*, p. 114.

[53]Ibid., p. 116.

means understanding the language in which it is written. Yet how we understand that language depends as much on *us* as it does on any 'objective' (first-world-derived) meanings the language, and therefore the book, may have. 'A man who reads a book with understanding', writes Popper, 'is a rare creature'.[54]

1.2 Kuhn on language

Without being nearly as open as Popper with his core assumptions about language, the equally influential Thomas Kuhn nevertheless treats issues in the history and philosophy of science in such a way so as to suggest that important assumptions about language are operative.

1.2.1

A different set of interests is, of course, apparent in Kuhn's writings, the consideration of scientific *communities* and cognitive *commitments* being of special importance. In a collection of essays entitled *The essential tension*, Kuhn outlines a role for language in relation to these two:

> One thing that binds the members of any scientific community together and simultaneously differentiates them from the members of other apparently similar groups is their possession of a common language or special dialect ... in learning such a language, as they must to participate in their community's work, new members acquire a set of cognitive commitments ... Such commitments are a consequence of the ways in which the terms, phrases, and sentences of the language are applied to nature.[55]

In the context of Kuhn's work, the references to language in this instance can be properly understood only in relation to his various articulations of the notion of a paradigm and *its* functions in science. This is because Kuhn never explains what he means by 'language' (or indeed by 'community'). Instead it becomes clear that in one sense language (a 'special dialect') along with scientific communities, cognitive commitments, theories, and paradigms are just different aspects of one and the same thing — what he calls 'normal science'.

Thus, for example, in the space of a page, the 'scientific community' in the excerpt above is replaced by 'proponents' of theories: 'Proponents of different theories ... speak different languages — languages expressing different cognitive commitments, suitable for different worlds'.[56] And a paradigm, characterised by Kuhn in one

[54]Ibid., p. 115.

[55]Kuhn, *The essential tension*, p. xxii.

[56]Ibid., pp. xxii-xxiii.

instance as those 'shared elements [that] account for the relatively unproblematic character of professional *communication* and for the relative unanimity of professional judgement',[57] clearly encompasses language in a rich sense. Language, in this sense, is itself like a paradigm. But Kuhn also imagines language as *sets of sentences* (or as that in which theories, thought of as sets of sentences, can be expressed), as is evident in the following exposition of 'incommensurable' normal-scientific traditions: '[my] claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss'.[58] I have already quoted Kuhn as saying that language consists of '*terms, phrases, and sentences*' that are 'applied to nature'.

Explicit references to language in science are almost completely absent from *The structure of scientific revolutions* (which predates most of the articles in *The essential tension*). But in this classic work much of what Kuhn writes about scientific traditions and scientific change relies on supposed uses, properties, and typical functions of language.

For example, throughout the book Kuhn employs the expression 'paradigm articulation', or 'articulation of theory', to describe what in his view is the main preoccupation of scientists during periods of normal science. Just 'three classes of problems', he writes, namely 'determination of significant fact, matching of facts with theory, and articulation of theory', 'exhaust ... the literature of normal science, both empirical and theoretical'.[59] Evidently, the processes of articulation he has in mind are carried out largely, although perhaps not exclusively, by means of the scientific language (or the 'competing' languages) that are current at any particular time. The processes of articulation directly influence the language in question: 'the construction of elaborate equipment, the development of an esoteric vocabulary and skills, and a refinement of concepts' are the essential features of paradigm articulation, according to Kuhn.[60] Clearly at least two of these involve modifications of language.

Additional examples of the importance that language assumes in Kuhn's scheme may be gleaned from his account of how scientists are brought into the fold of normal science. 'Textbooks' here play a crucial role:

[57]Ibid., p. 297, my emphasis.

[58]Kuhn, 'Commensurability, comparability, communicability', p. 670.

[59]Kuhn, *The structure of scientific revolutions*, p. 34; for some other instances of the expression see pp. 23, 35, 79, 83, and 91.

[60]Ibid., p. 64.

> [They exhibit] concrete problem solutions that the profession has come to accept as paradigms, and they then ask the student ... to solve for himself problems very closely related in both method and substance to those through which the textbook or the accompanying lecture has led him.[61]

> Textbooks ... aim to communicate the vocabulary and syntax of a contemporary scientific language.[62]

Of course, the concrete problem solutions textbooks exhibit are exhibited in language, albeit a special one. The student's first paradigms (where 'paradigm' is used here in the narrow sense of 'exemplar') are thus almost exclusively expressed in the language of the text or the lecture. The effect of repeated exposure to exemplars instils in the student basic *skills*: the practice of normal science 'depends on the ability, acquired from exemplars, to group objects and situations into similarity sets which are primitive in the sense that the grouping is done without an answer to the question, "Similar with respect to what?"'.[63] The exercise of these skills involves uses of language that imitate or extend the language of the exemplars, although as Kuhn is keen to point out they are not skills at definition: science students are not taught definitions but standard ways to solve selected problems in which crucial terms such as 'force' or 'compound' figure.[64] Concepts, laws, and theories — which Kuhn calls 'intellectual tools' — 'are from the start encountered in an historically and pedagogically prior unit that displays them with and through their applications'[65] — that is, with and through actual and significant linguistic uses. Only then do scientists go on to 'model their own ... research on them [i.e. the exemplars]'.[66]

When Kuhn characterises the transition of a field of study from 'its prehistory as a science' to 'its history proper', it is the emergence of a special kind of textual cult that he has in mind: 'brief articles' make an appearance, 'addressed only to professional colleagues, the men whose knowledge of a shared paradigm can be assumed and who prove to be the only ones able to read the papers addressed to them'.[67] But more generally and extensively, Kuhn relies upon linguistic features to characterise the revolutionary transition from one period of normal science to another:

[61]Kuhn, *The essential tension*, p. 229.

[62]Kuhn, *The structure of scientific revolutions*, p. 136.

[63]Ibid., p. 200; cf. Kuhn, *The essential tension*, p. 306.

[64]Ibid., p. xix; cf. Kuhn, *The structure of scientific revolutions*, p. 201.

[65]Ibid., p. 46; see also p. 47.

[66]Kuhn, *The essential tension*, p. xix.

[67]Kuhn, *The structure of scientific revolutions*, p. 20.

After a scientific revolution many old measurements and manipulations become irrelevant and are replaced by others instead ... But changes of this sort are never total. Whatever he may then see, the scientist after a revolution is still looking at the same world ... much of his language and most of his laboratory instruments are still the same as they were before.[68]

Similarly, a few pages on:

Since new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed. But they seldom employ these borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other.[69]

Kuhn is arguing that from one period of normal science to next the language of science is put to radically different uses (even though superficially much of it remains the same), and scientists see the world in radically different ways. Because 'different languages impose different structures on the world',[70] it is the language of a paradigm that determines how the world is seen. To displace this, is to displace a way of seeing (perceiving, thinking):

This need to change the meaning of established and familiar concepts [like space, time, and mass] is central to the revolutionary impact of Einstein's theory ... Just because it did not involve the introduction of additional objects or concepts, the transition from Newtonian to Einsteinian mechanics illustrates with particular clarity the scientific revolution as a displacement of the conceptual network through which scientists view the world.[71]

Elsewhere Kuhn suggests that a paradigm be thought of as a *lexical* network or 'lexicon' whose multi-dimensional structure 'mirrors aspects of the structure of the world which the lexicon can be used to describe'. This same structure 'simultaneously limits the phenomena that can be described with the lexicon's aid'.[72] A change in paradigm would thus be tantamount to a change in lexical structure.

However, Kuhn nowhere explains how such a change is effected, how 'old terms, concepts, and experiments', in the course of the relatively brief period of a revolution, 'fall into new relationships one with the other', how concepts are related to experiments, how the meaning of 'established and familiar concepts' is changed. He

[68]Ibid., pp. 129-130.

[69]Ibid., p. 149.

[70]Kuhn, 'Commensurability, comparability, communicability', p. 682.

[71]Kuhn, *The structure of scientific revolutions*, p. 102.

[72]Kuhn, 'Commensurability, comparability, communicability', pp. 682-683. For a similar suggestion, see also idem, 'Dubbing and redubbing', p. 300.

writes that the transition from one scientific paradigm to another results in scientific practice conducted 'within a different universe of discourse',[73] but does not explain what it takes to bring about so much change in the language and linguistic habits of scientists.[74] He implies that changes in practice follow changes in 'discourse', and that language places significant constraints on scientific practice (a paradigm can 'insulate the community from those ... problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies'[75]), but does not explain why language (or discourse) and scientific practice are *distinguishable* — especially after they have been intricately related during the uptake of exemplars. Kuhn avoids such issues arising from claims of his own, suspecting that they demand 'the competence of the psychologist'.[76] Like Popper, Kuhn places language and its cognitive consequences beyond the scope of the philosophy of science.

In addition to remarks cited so far, Kuhn refers to the 'adjustment' of 'conceptual categories',[77] the 'process of conceptual assimilation',[78] 'rules and standards for scientific practice',[79] standards that determine what should count as 'an admissible problem',[80] 'rules that limit ... the nature of acceptable solutions' to problems of normal science,[81] paradigms as sources of methods and 'standards of solution',[82] the acquisition of 'cognitive commitments' through language,[83] etc. Although he does not

[73]Kuhn, *The structure of scientific revolutions*, pp. 85-86.

[74]Rorty writes that Kuhn and Feyerabend were 'concerned to show that the meanings of lots of statements in the language, including lots of 'observation' statements, got changed when a new theory came along; or, at least, that granting that such change took place made more sense of the facts of the history of science than the standard textbook view which kept meanings constant and let only beliefs change' (*Philosophy and the mirror of nature*, p. 270). The implication, of course, is that Kuhn and Feyerabend merely postulated changes without investigating the ways in which changes come about.

[75]Kuhn, *The structure of scientific revolutions*, p. 37; see also p. 58.

[76]Ibid., p. 86. Yet Kuhn's philosophy of science often reads like a kind of psychology. See, for example, his article 'Logic of discovery or psychology of research?' (in whose title a characterisation of Popper's philosophy is contrasted with a characterisation of his).

[77]Kuhn, *The structure of scientific revolutions*, p. 64.

[78]Ibid., p. 56.

[79]Ibid., p. 11.

[80]Ibid., p. 6.

[81]Ibid., p. 38.

[82]Ibid., p. 103.

[83]Kuhn, *The essential tension*, p. xxii.

explain how concepts are assimilated, where rules and standards are found and how they are applied, how language on its own could give rise to cognitive commitments, etc., Kuhn imagines that all these things (categories, processes, commitments, rules, standards, and so on) are expressible, subsist, or are enforceable in language. They presuppose and are about possible usages of language, or are processes dependent upon language. The extent to which a postulated and underexplored notion of language facilitates Kuhn's characterisations of pre-paradigmatic science, normal science, paradigms, etc., is hard to exaggerate.

My view, then, is that suppositions about the uses and significance of language are central to Kuhn's writings despite his inclination not to treat them explicitly (that is to say, not to treat them as requiring clarification within his scheme). The suppositions are important to my argument not only because they endow language with peculiar powers and functions, but also because of *contrasts* they draw. In what has gone already we have seen Kuhn contrast 'terms, phrases and sentences' with 'nature', 'terms and concepts' with 'experiments', 'conceptual' with 'manipulative' apparatus, and scientific discourse with scientific practice.[84] He has also said that language imposes a structure on the world (or constraints on how the world is viewed), and that a lexicon (a paradigm) has a structure which mirrors that of the world. Such contrasts bring us to a set of related issues that Kuhn likewise does not openly engage with.

1.2.2

In the early pages of *The structure of scientific revolutions*, Kuhn presents us with an image of pre-consensual science:

> in the early stages of the development of any science different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways. What is surprising ... is that such initial divergences should ever largely disappear.[85]

Appearing to be a generalisation about the early stages of every science, this supposition is also a kind of thought-experiment, effectively introducing the uncommitted reader to a dualist conception of language. Historically fundamental to all science, writes Kuhn, is the stage at which descriptions and interpretations of the same range of phenomena differ. The distinction between descriptions and interpretations on the one hand, and the objective range of phenomena on the other, is the surface distinction here

[84]Also, for a distinction between 'mental equipment' and 'world', see ibid., p. 261; see also Kuhn, *The structure of scientific revolutions*, p. 263. For a distinction between 'conceptual apparatus' and 'nature', see ibid., pp. 264-265.

[85]Ibid., p. 17.

— but at the same time, because the descriptions and interpretations belong to the domain of language and the phenomena are a part of the world, Kuhn is also expressing a disjunction of language and world. The implication of this manifestation of dualism is that a *mature* science is characterised by a language that describes and interprets the world of phenomena to the satisfaction of the majority of scientists.

However, Kuhn is not a naive, unquestioning language/world dualist. The passage cited above is, with its stark contrasts, an aberration. Later in the book Kuhn reasons that 'what occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable data ... the data are not unequivocally stable'.[86] Later still he returns to ask:

> is sensory experience fixed and neutral? Are theories simply man-made interpretations of given data? The epistemological viewpoint that has most often guided Western philosophy for three centuries [has it so]. In the absence of a developed alternative, I find it impossible to relinquish entirely that viewpoint. Yet it no longer functions effectively, and the attempts to make it do so through the introduction of a neutral language of observations now seem to me hopeless.[87]

Dualism, as I have said, involves the disjunction of 'language' and 'world'. Occasionally — and the epistemological viewpoint Kuhn refers to is such an occasion — the world is seen as constituted by 'the fixed and neutral data of sensory experience' (viz. phenomena), either because it is thought that they are the outer limits of what we can sensibly talk about, or because it is thought that they give us direct access to the world, or merely because it is thought that such data play a role in the deliberations of scientists. The disjunction of language and the world-thus-understood represents a version of dualism that is often encountered in the works of anti-realists.

By not relinquishing entirely the epistemological viewpoint in question, Kuhn (while not necessarily espousing phenomenalism) is *ipso facto* not relinquishing a version of the disjunction of language and world. He stands by a similar viewpoint in an article published many years later:

> [some philosophers] remark that empirical reference enters ... theories from the bottom up, moving from an empirically meaningful basic vocabulary into the

[86]Ibid., p. 121. There is, however, an ambiguity in his use of 'data'. It is likely he is referring to what is *already* described and interpreted. See my comments on his reference to 'facts', below.

[87]Ibid., p. 126. Cf.: 'the view of science-as-cumulation is entangled with a dominant epistemology that takes knowledge to be a construction placed directly upon raw sense data by the mind' (p. 96). See also p. 201.

> theoretical terms. Despite the well-known difficulties that cluster about the notion of a basic vocabulary, I cannot doubt the importance of that route.[88]

This metaphor, which calls on us to imagine empirical reference as a movement that arises beyond the bounds of basic vocabulary, following a route which 'enters' the vocabulary and continues up to the level of theoretical terms, is yet another expression of dualism. The language of science (in this case, basic vocabulary and theoretical terms) fulfils its function (empirical reference) by way of a kind of intercourse (an unspecified relation) with the world (which lies *beyond* basic vocabulary). Kuhn, as we have seen, has rejected as hopeless the search for a neutral observation language. Yet in the end, despite reservations, he endorses a view whose expression relies on the disjunction of language and world.

From a dualistic perspective the subject matter of science lies beyond the language of science — beyond its vicissitudes; it is this assumption that underlies Kuhn's following statement (a variant of which I have already mentioned):

> the proponents of competing paradigms practice their trades in different worlds ... That is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other.[89]

How can Kuhn know that 'both are looking at the world, and what they look at has not changed'? Judgements of similarity and difference (or identity) presuppose a point of view. What is Kuhn's point of view when judging in this instance that the world has not changed (or that it is one and the same world)? Obviously Kuhn cannot be restricted to what the various proponents 'see' for he would be at a loss to judge. So he claims access to 'what they look at' as well. Such access does not presuppose superhuman vision on his part. It is allowed for and enabled by dualist assumptions. From the belief that language and the world are disjointed and that in a scientific context a function of the former is to secure reference to the latter (Kuhn talks about the 'physical referents' of Einsteinian and Newtonian concepts[90]), it is a short step to the

[88]Kuhn, *The essential tension*, p. 300.

[89]Kuhn, *The structure of scientific revolutions*, p. 150; a similar remark, quoted above, appears on pp. 129-130. Kuhn imagines an occasion on which a physicist and a chemist offer conflicting answers to the question 'Is a single atom of helium a molecule?' Kuhn comments: 'Presumably both men were talking of the same particle' (pp. 50-51).

[90]Ibid., p. 102.

belief that the world, or the phenomena it furnishes, are always basically the same, and only human interpretations and descriptions of them differ.[91] When Kuhn asserts that:

> theories do not evolve piecemeal to fit facts that were there all the time. Rather, they emerge together with the facts they fit from a revolutionary reformulation of the preceding scientific tradition, a tradition within which the knowledge-mediated relationship between the scientist and nature was not quite the same,[92]

he is not saying, as it might seem, that the world itself undergoes reconstruction after each revolution. The 'facts' he mentions are not constituents of *that* world. Rather, they are 'descriptions' and 'interpretations' — constructs of the language of science. The phenomenal/physical facts of the world remain unchanged. Carefully read there is no radical new departure in Kuhn's claim — it mirrors the language/world dualism espoused, for example, by Popper (only here the ready-made first world is phenomenal rather than physical).

1.2.3

In an article published twelve years after *The structure of scientific revolutions*, Kuhn exclaims in a footnote that 'it is ... remarkable how little attention philosophers of science have paid to the language-nature link'.[93] The so-called problem of the link between language and nature, although not an explicit object of analysis in *The structure of scientific revolutions*, nevertheless seems to find expression there: 'there are seldom many areas in which a scientific theory ... can be directly compared with nature'.[94] Indeed, 'immense difficulties [are] often encountered in developing points of contact between a theory and nature'.[95] In these isolated remarks Kuhn could be interpreted as saying that it is immensely difficult to set up and perform useful experiments. Alternatively, he could be saying that the process of articulating theory to yield predictions, which like Popper he sees as one of the tasks of normal science, is an extremely arduous one. But Kuhn could also be interpreted as saying that there is *a problem about how words link up with the world.*

[91]Cf. Dirac: 'In atomic theory we have fields and we have particles. The fields and particles are not two different things. They are two different ways of describing the same thing — two different points of view. We use one or the other according to convenience' (*Lectures on quantum field theory*, p. 1).

[92]Kuhn, *The structure of scientific revolutions*, p. 141.

[93]Kuhn, *The essential tension*, p. 303, n. 13. For an earlier mention of the language-nature link, see p. xxii.

[94]Kuhn, *The structure of scientific revolutions*, p. 26.

[95]Ibid., p. 30; cf. p. 31

Dualism (at whose core is a metaphysical assumption about the distinguishability and dissociation of language and world) naturally leads one to ask how the two link up. That Kuhn's language-nature link problem is really a word-world link problem (recall his concern with empirical reference, briefly mentioned earlier), is supported by a question Kuhn pursues in the article of 1974: 'how do scientists attach symbolic expressions to nature?'[96] — a question he tries to answer in part by way of an explanation of how *young children* learn to apply symbolic expressions to nature.[97] But he asserts too:

> Since the abandonment of hope for a sense-datum language, the usual answer to this question has been in terms of correspondence rules. These have ordinarily been taken to be either operational definitions of scientific terms or else a set of necessary and sufficient conditions for the terms' applicability. I do not myself doubt that the examination of a given scientific community would disclose a number of such rules shared by its members.[98]

The assumption in the final sentence, like Popper's assumption that what scientists in fact do is test statements against experience, is not actually investigated (not even by reference to Kuhn's own past experience as a scientist). Kuhn here simply *assumes* that a scientific community shares 'correspondence rules'.[99] Later he mentions that very few such rules are to be found in science texts or science teaching and 'that scientists regularly deny their relevance'. Ultimately, however, he does not doubt their existence. Instead he seeks to complement them: 'One begins to wonder whether more than a few such rules are deployed in community practice, whether there is not some alternate way in which scientists correlate their symbolic expressions with nature'.[100] Mere mastery of rules cannot account for everything that underpins scientific practice, which is why Kuhn's eventual proposal is that 'shared examples of successful practice could ... provide what the group lacked in rules'.[101]

Of course, mention of the importance of 'shared examples' (paradigms or exemplars) has already been made in *The structure of scientific revolutions*, long before the appearance of the article I have been referring to. (In the book, as I have said, Kuhn states that exposure to exemplars develops skills in students, skills that are assumed to be overwhelmingly language-induced and language-dependent.) In the article, the role

[96]Kuhn, *The essential tension*, p. 301.

[97]Ibid., pp. 309 f.

[98]Ibid., p. 302.

[99]For a contrary opinion, based on the first-hand experience of an active scientist, see Senior, 'The vernacular of the laboratory'.

[100]Kuhn, *The essential tension*, p. 305.

[101]Ibid., p. 318.

of shared examples is called upon to answer the question 'how do scientists attach symbolic expressions to nature?'. This approach is unpromising and developed only half-heartedly by Kuhn. His shared examples of successful practice can at most explain how scientists *learn* the use of their 'symbolic expressions' (I have already quoted Kuhn as saying that what scientists learn from exemplars is 'to group objects and situations into similarity sets'[102] — not to bridge the supposed language-world divide). The shared examples are not sufficient to resolve the philosophical question about the supposed mode of *attachment to nature* of symbolic expressions. Kuhn never actually solves what he calls the problem of the 'language-nature link'.

In another article, published twenty-one years after *The structure of scientific revolutions*, Kuhn regrets not having devoted more space to language in his philosophy: 'If I were now rewriting *The structure of scientific revolutions*, I would emphasise language change more and the normal/revolutionary distinction less'.[103] Yet, as I have tried to show, a significant conception of language and its functions is already present in his early work. Like Popper, though less explicitly, Kuhn makes assumptions both about what language in science is and about what it does. Language is, first of all, 'symbolic expression'. It expresses (or enables) facts of observation, theories, rules, and standards. Mastery of it is synonymous with the acquisition of the cognitive commitments necessary for normal science. Moreover, language is distinguishable from scientific practice (even though a student is not taught them separately). Kuhn contrasts language with that to which it refers, namely the world or nature. Reference of this kind is supposedly what legitimates language as scientific.

For the purposes of my argument, four elements of the above summary of Kuhn's conception of language are important. First, the conception is rather limited or stilted. Second, it is not peripheral to Kuhn's philosophy: many of his familiar views depend on it. Third, the conception comprises assumptions (some obviously philosophical) that are not defended or substantiated by argument. And fourth, a subset of these assumptions (comprising the language/world disjunction and the primarily referential/descriptive function of language discussed in relation to the possibility of epistemic access to the world), identifies Kuhn as a dualist.

1.2.4

My remarks so far have taken into account Kuhn's most commonly cited works up until the early '80s. In a recent paper about 'The vulnerability of rigid designation'

[102]Kuhn, *The structure of scientific revolutions*, p. 200.

[103]Kuhn, 'Commensurability, comparability, communicability', pp. 715-716.

(1990), 'problems of language' have been elevated by Kuhn to a position of central importance.[104] His assumptions about what language in science is and what it does, as I have summarised them above, are rendered relatively explicit without significant alteration. Kuhn sets out to explain incommensurability of successive scientific theories by stipulating the existence of a lexicon peculiar to each theory, systematically different from the lexicons of older or newer theories. A lexicon cannot be translated into another, Kuhn argues, even though there is no principled obstacle to *understanding* unfamiliar lexicons. Here, in contrast with earlier work, issues relating to the stipulation of a lexicon are examined closely. Noteworthy is the language Kuhn favours (indicative perhaps of a general shift of interest): the scientific community is a *speech* community; scientists are *participants in discourse*; scientific *communication* is thus of great interest; scientific novices are *language learners* who are put through a *lexical acquisition process*; scientists are *bound by language* (a redescription of their 'cognitive commitments'); theory-change is the *transition to a new vocabulary*; and so on.[105]

Kuhn considers how a lexicon is *learned*. Terms not available in the student's 'antecedent vocabulary' (terms like 'force', 'mass', and 'weight', if the student is being introduced to Newtonian mechanics) are learned in use. They are first encountered, Kuhn maintains, in authoritative statements about the world by someone who already knows how to use them. The lexical acquisition process interrelates a set of new terms by juxtaposing 'statements involving the terms to be learned with situations drawn directly or indirectly from nature'.[106] A lexicon that embodies such interrelationships between terms is said thus to embody 'necessarily' 'knowledge of the world those terms can be used to describe'.[107] The possession of such knowledge underpins and simultaneously constrains what members of the scientific community can express. The learning process ensures that at least some physical laws are 'built into the lexicon'.[108] Scientists are therefore bound by language to preserve certain laws.[109]

The examples introduced during the lexical acquisition process can be modified in the light of new observations, when that is deemed necessary. Modified examples will maintain the stability of the lexicon, 'keeping in place a set of quasi necessities

[104]Kuhn, 'Dubbing and redubbing'.

[105]These and similar phrases occur frequently throughout the article.

[106]Kuhn, 'Dubbing and redubbing', p. 304.

[107]Ibid., p. 315.

[108]Ibid., p. 306.

[109]Ibid., p. 307.

equivalent to those initially induced by language learning'.[110] If too many examples require modification the lexicon in use will not be able to accommodate the changes: 'it is no longer individual laws or generalisations that are at stake, but the very vocabulary in which they are stated'.[111] Because theoretical and practical knowledge is embodied in the vocabulary during lexical acquisition, a threat to that vocabulary is also a threat to the theory or laws essential to its acquisition and use. Altered laws introduce terms that make translations between lexicons impossible. A new lexicon, according to Kuhn, gives access to its own set of possible worlds. Once consolidated it will display the same sorts of limitations as its predecessor. It cannot, he argues, be used to provide coherent descriptions of some aspects of the world described by its predecessor: 'here and there the old and new lexicons embod[y] differently structured, nonhomologous taxonomies, and statements involving terms from the regions where the two differ [are] not translatable between them'.[112]

Kuhn's article is primarily about the incommensurability of successive scientific theories, but its contents are relevant to my exposition of the dualist conception of language. Despite Kuhn's new emphasis on scientific language-learning, linguistic communities, linguistic constraints, etc. — and despite his remarkable (though unelaborated) suggestion that 'the substance and the vocabulary of science' are in a certain sense inseparable[113] — much of his language continues to point to underlying dualist assumptions. To give some examples: 'theories' are said to be expressible as systems of statements (this usually facilitates the further claim that the statements are *about* the world);[114] science students are said to need a vocabulary 'adequate to refer to physical objects';[115] words are distinguished from 'the world' in which they function (even though, again, a student is said to acquire knowledge of both simultaneously);[116] actual examples/exemplars are said to 'anchor the terms of the lexicon to the world';[117] after the adoption of a new lexicon, 'one can write down strings of symbols that ... attach to nature differently from the corresponding symbols in the old';[118] lexicons can be used to *describe* a varied set of worlds (description is of course a primary function

[110]Ibid., p. 306.

[111]Ibid.

[112]Ibid., p. 315.

[113]Ibid., p. 303.

[114]Ibid., p. 299.

[115]Ibid., p. 302.

[116]Ibid.

[117]Ibid., p. 314.

[118]Ibid., pp. 307-308.

of language in science for dualists);[119] terms of a lexicon 'designate rigidly', as long as the lexicon endures;[120] and so on. Most of these assertions are contained in one form or another, as I have tried to show, in Kuhn's earlier work. Here, as there, they are not merely loose metaphors, but are related to a core dualist disjunction of language and world.

1.3 Other formulations of dualism

'The events of the physical world are language-free. Human language is necessary for our apprehension and description of events beyond the human'.[121] This statement clearly embodies the dualist conception of language: it involves a disjunction of language and world and is also a specification of primary functions for language (namely, apprehension and description). The dualist conception has been expressed in a variety of ways, and some of that variety was evident in the previous sections on Popper and Kuhn.

Gillian Beer, author of the statement just quoted, proceeds to qualify the statement in a way that reveals the extent to which her conception of language differs from Popper's and Kuhn's: 'Yet at the same time language is anthropocentric, persistently drawing the human back to the centre of meaning'.[122] Whereas at first her remark would have appeared as yet another statement of the thesis that language conveys 'descriptions' of a mind-independent world, with the qualification it becomes apparent that language for Beer always reflects human interest in 'events beyond the human'. The disjunction of language and world is preserved, but the function of language is to achieve more than mere description.

Most philosophers of science, both realists and anti-realists, share Beer's view that 'the events of the physical world' are language-free. In Part 2 I argue that the 'events' of the physical world are not language-free if what are referred to are physical facts that *scientists* identify and try to comprehend (viz., 'events' that are the subject matter of science). For the remainder of Part 1 I confine myself to a survey of the various expressions that the dualist conception of language has received outside of the works of Popper and Kuhn. I hope in this way to show that dualism in one form or another has

[119]Ibid., p. 300.

[120]Ibid., p. 315.

[121]Beer, 'Problems of description in the language of discovery', p. 41.

[122]Ibid.

held an important place in meta-scientific discourse generally. Its presence as an assumption, although widespread, has never been justified or defended, and has often led to unclear views in the philosophy of science. Its refutation and replacement with a new conception of language becomes all the more urgent as the extent of its influence is appreciated.

1.3.1

The apparently mutually exclusive metaphysical categories of 'language' and 'world' are fundamental to the philosophies of Popper and Kuhn. Some philosophers (supportive of idealism) have wanted to jettison the notion of 'world', while others (supportive of hard-nosed empiricism) have sought to diminish the importance of 'language'. Both groups, however, have merely expounded extreme cases of dualism. The abandonment of the world in favour of language, and the repudiation of language in favour of the world, set the outer limits for philosophical views that the dualist conception can underpin. So, for example, at one extreme, Susanne Langer asserts that 'the fact is that our primary world of reality is a verbal one'.[123] And Roger Jones, in his book *Physics as metaphor*, argues that it is the language of science which — like poetry — creates the physical world. He claims to offer:

> an idealistic reevaluation of the physical world. I reject the myth of reality as external to the human mind, and I acknowledge consciousness as a source of the cosmos. It's mind that we see reflected in matter. Physical science is a metaphor within which the scientist, like the poet, creates and extends meaning and value in the quest for understanding and purpose.[124]

At the other extreme, language, far from being the sole source of the physical world and reality, is at best a very *poor* means of understanding what is really there. Maurice Merleau-Ponty took himself to be speaking on the behalf of many when he declared that 'we all secretly venerate the ideal of a language which in the last analysis would deliver us from language by delivering us to things'.[125] In the history of science some familiar expressions of scepticism about language date from the first half of the nineteenth century (as do lesser known and more pernicious expressions),[126] although even earlier examples are easily found: 'I think it fitter to alter a terme of art than reject a new

[123]Langer, *Philosophy in a new key*, p. 126.

[124]Jones, *Physics as metaphor*, p. ix.

[125]Merleau-Ponty, *The prose of the world*, p. 4.

[126]For the most extreme expressions of pro-observation, pro-fact, anti-theory, and anti-language sentiments see the views of surgeons such as P.-J.-G. Cabanis, as discussed in Ackerknecht's *Medicine at the Paris hospital, 1794-1848.*

truth', declared Robert Boyle.[127] Thomas Sprat, early advocate of the Royal Society, diagnosed a general perversion of language and misapplication of rhetoric in science.[128] Francis Bacon attacked language for merely perpetuating age-old categories untested by reference to reality.[129] The chemist Antoine Lavoisier often advised that 'we must trust nothing but facts', insofar as they are revealed to us by experiment and observation.[130] In every instance, he believed, we should submit our reasoning to facts, not words.

One of the most vocal advocates of facts was yet another chemist, Michael Faraday, whom a modern biographer sees as the perpetrator of a 'cult of facts'.[131] In a letter to Whewell in 1834, Faraday drew a distinction between facts and the medium of language, asserting that the latter distorted the former.[132] Facts and only facts, he believed, are the basic signs of nature, and as signs they can, in principle, be read straight off nature.[133] In practice, however, an artefactual human language is necessary. Faraday hoped for a purification of ideas and words, in order that (in Geoffrey Cantor's words) 'the link between experiment and language [is rendered] complete and facts could be expressed and communicated without distortion by the linguistic medium'.[134]

Most contemporary expressions of dualism, explicit or implicit, are not the extreme formulations of the kind enunciated by the likes of Jones and Faraday: they are not normally part of an argument or wish to dispense with language or the world. Contemporary expressions are, moreover, mostly encountered as assumptions (the 'monism' of Jones and Faraday is so self-conscious that it can hardly be called an assumption). For example, the anti-positivist Michael Polanyi bases his analysis of

[127]Cited in Crosland, *Historical studies in the language of chemistry*, p. 114, n. 179.

[128]See Vickers, 'The Royal Society and English prose style', p. 6.

[129]Ibid., p. 12. Bacon disliked definition 'since the definitions themselves consist of words, and those words beget others: *so that it is necessary to recur to individual instances ...*' (ibid., my emphasis).

[130]Quoted in Hackmann, 'The relationship between concepts and instrument design', p. 211.

[131]Cantor, *Michael Faraday*, p. 201.

[132]The letter is quoted in ibid., p. 215.

[133]Ibid., p. 200.

[134]Ibid., p. 214; see also Schaffer, 'The history and geography of the intellectual world', pp. 226-230. For a modern parallel see Quine: 'Science, though it seeks traits of reality independent of language, can neither get on without language nor aspire to linguistic neutrality. To some degree, nevertheless, the scientist can enhance objectivity and *diminish the interference of language*, by his very choice of language' ('The scope and language of science', p. 7, my emphasis).

scientific *concept acquisition* on the dualist conception: language is 'words', he assumes, and through training words are associated with 'things'. The following passage, echoing some of Kuhn's claims,[135] highlights the 'passive' role that language is accorded in many dualist accounts:

> Think of a medical student attending a course in the X-ray diagnosis of pulmonary diseases. He watches in a darkened room shadowy traces on a fluorescent screen placed against a patient's chest, and hears the radiologist commenting to his assistants, in technical language, on the significant features of these shadows. At first the student is completely puzzled ... Then as he goes on listening for a few weeks, looking carefully at ever new pictures of different cases, a tentative understanding will dawn on him ... And eventually, if he perseveres intelligently, a rich panorama of significant details will be revealed to him ... He still sees only a fraction of what the experts can see, but the pictures are definitely making sense now and so do most of the comments made on them ... Thus, at the very moment when he has learned the language of pulmonary radiology, the student will also have learned to understand pulmonary radiograms. The two can only happen together ... by discovering a conception which comprises a joint understanding of both the words and the things.[136]

The disjointed realms of words and objects are brought together in scientific intuition. Polanyi's scientist, free from a positivistic preoccupation with the rationality of scientific procedures, is constrained nonetheless by the language/world duality.

Rom Harré employs the metaphysics of dualism to express his realist position that the aim of science is to reveal the ultimate structure of the world. An assumed disjunction of theoretical 'terms' and natural 'things' enables him to answer a larger question about the true aim of science:

> Science is actually interested in discovering the structure and inner constitution of natural things and their relations in the cosmos, in virtue of which phenomena display the regularities and irregularities they do. The use of theoretical terms is precisely the best way of achieving science's real aim, for they are just what lead to existential hypotheses about the unobserved.[137]

Assuming, as Harré does, that it is clear what is meant by 'natural things' and 'theoretical terms', and how they are to be differentiated, the aim of science is to use the terms to *access* the things.[138] In other words, in the course of specifying the aim of science, and differentiating between terms and things, Harré also indicates a particular *function* for language. Joseph Margolis, in a similar vein, writes that 'the use of language [assists] the discernment of the real structures of the world'.[139]

[135]See p. 46, above.

[136]Polanyi, *Personal knowledge*, p. 101.

[137]Harré, *The principles of scientific thinking*, p. 21; cf. p. 66.

[138]See, ibid., p. 260.

[139]Margolis, *Texts without referents*, p. xiii.

Metaphysical assumptions about the function of language in science almost always follow upon the disjunction of language and world. They are not insights resulting from actual investigations of how language functions in science, or from any empirical investigation of scientific practice. Assumed functions are purely philosophical or *a priori* products, resulting from a perceived need to specify a relationship between the realms of language and world. Popper, as I mentioned, believes that the primary function of language in science is the construction of statements that may then be tested against experience. It is a belief that arises from within Popper's philosophy — it is not, and does not purport to be, an *observed* fact of scientific practice, even though subsequently it is treated as if it were a correct description of what scientists do.

Bruce Gregory, an idealist, asserts that 'implicit in the way we use language is the notion that language points to a world beyond itself'.[140] But is that notion *really* implicit in the way we use language? The suggested function of language as a pointer to another world should, of course, be treated cautiously in the absence of supporting arguments. Conceivably the suggested function is a fabrication, appearing genuine to Gregory only as a result of his acceptance of the disjunction of language and world. Implicit there, in *that* disjunction, it could be said, and not in anything 'we' do, is a function for language. The function which Gregory calls implicit need be little more than an *a priori* projection onto language. When the linguist/philosopher Jay Rosenberg asks, 'how is it possible for language to represent the world?', his spontaneous answer, to which he is at the same time firmly committed, demonstrates the close connection between language-function and the language/world disjunction: '*The question posits two structures* — one linguistic and one extra-linguistic'.[141] Grover Maxwell posits a 'framework', which amounts to much the same thing:

> in order to understand considerations concerning the existence of any kind of entities one must understand the meanings of the linguistic expressions (sentences and terms) referring to them — and that *such expressions have no meaning unless they are given a place in a linguistic framework which 'talks about the world'*.[142]

[140]Gregory, *Inventing reality*, p. 195; cf. pp. 22 and 200. Similarly, Bloor writes that our everyday understanding of truth is in terms of correspondence: when we talk of truth we suppose that 'some belief, judgement or affirmation corresponds to reality and that it captures and portrays how things stand in the world' (*Knowledge and social imagery*, p. 32).

[141]Rosenberg, *Linguistic representation*, p. 95, emphasis added. Cf. Quine, 'The scope and language of science', p. 3.

[142]Maxwell, 'The ontological status of theoretical entities', p. 22, my emphasis. Quine asks: 'What does it mean to assume external objects? ... If we turn our attention to the words, then what had been a

Although the primary metaphysical function variously assigned to language in science is *not* at the core of dualism (see p. 10), an assignment of such a function follows almost as a matter of necessity from the disjunction of language and world (or else they would remain disjointed). In Part 2 I argue that the empirical study of scientific language-use refutes the view that there is a metaphysical bridging function for language in science. That is to say, it is from an examination of actual language *function* that I proceed to the refutation of the core dualist disjunction.

The function usually assigned to language in philosophy of science is either referential or representational (usually in the sense of descriptive). 'The [scientific] text is a linguistic object that takes on the overriding task of the representation of nature',[143] writes the anti-realist Charles Bazerman. In Popper's work, an 'aboutness' function of one kind or another is frequently in evidence ('it is interesting to analyse ... the function of language ... as an instrument ... [for] we use ... language in order to talk about the world').[144] The aboutness at the heart of both representation and reference harmonises nicely with the assumption that language and the subject matter of science are disjointed. The connections are relatively clear in the following remarks by the realist Ronald Giere:

> My general view is that scientific theories should be regarded as continuous with the representations studied in the cognitive sciences ... Scientific theories are more often described using *written* words or *mathematical* symbols than are the mental models of the lay person. But fundamentally they are the same sort of thing ... the only feature of representations I wish to remark is that they are just that — *representations* ... they are like internal maps of the external world.[145]

In Giere's view, in other words, scientific theories (words and symbols) are a special kind of language that functions to represent the world. The language/world disjunction in this case is upheld by reference to an image of the body: language inhabits the mind inside — world the space outside. Although Giere presents his view as merely an extension of an alleged guiding assumption of cognitive science (namely, 'that humans ... have internal representations of the environment'[146]), in reality it involves an

question of assuming objects becomes a question of verbal *reference* to objects ... We refer by using words, and these we learn through more or less devious association with stimulations of our sensory receptors' (*Theories and things*, p. 2).

[143]Bazerman, *Shaping written knowledge*, p. 220.

[144]Popper, *Conjectures and refutations*, p. 63.

[145]Giere, 'The cognitive study of science', p. 143.

[146]Ibid.

assumption of his own. This is evident in Giere's presentation of his key notion of a 'model'. On this account (designed to circumvent direct language-world correspondence and reminiscent of Plato's tactic), there is 'no direct relationship between sets of statements and the real world. The relationship is indirect through the intermediary of a theoretical model'.[147] Here the familiar disjunction of language and world has little to do with the existence of internal representations, which Giere calls the guiding assumption of cognitive science. Dualism is not an entailment of *that* assumption. It is an entailment of Giere's own assumption, namely that language is dissociated from the world and functions to refer to elements of an (internal) model of the world.

Some philosophers have suggested that specific conditioning during childhood determines the primarily representative or referential function of language in science. Quine supposes that the perceived disjunction of language and world follows naturally upon the learning of a language. Scientists have merely come to develop a more robust understanding of that disjunction:

> At the very beginning of one's learning of language ... words are learned in relation to such likenesses and contrasts as are already appreciated without benefit of words. No wonder we attribute those likenesses and contrasts to real stuff, and think of language as a superimposed apparatus for talking *about* the real ... The notion of reality independent of language is carried over by the scientist from his earliest impressions.[148]

But while Quine supposes that we think of language as an apparatus superimposed upon reality, Paul Feyerabend, who also depends on generalisations about childhood learning, denies that we ordinarily think that way. In the following instance Feyerabend refers to what he calls 'natural interpretations'. He has in mind statements such as, 'the stone is falling straight down':

> There are not two acts — one, noticing a phenomenon; the other, expressing it with the help of the appropriate statement — *but only one* ... We may, of course, abstractly subdivide this process into parts ... But under normal circumstances such a division does not occur; describing a familiar situation is,

[147]Giere, *Explaining science*, p. 82. See also Fig. 3.8 on p. 83 and other expressions of the language/world disjunction on pp. 19, 75, 80, and 93.

[148]Quine, 'The scope and language of science', pp. 4-5. Quine insists that notions like 'reality' and 'evidence' are intelligible to us, and have been derived from the testimony of our senses: 'We cannot significantly question the reality of the external world, or deny that there is evidence of external objects in the testimony of our senses; for, to do so is simply to dissociate the terms 'reality' and 'evidence' from the very applications which originally did most to invest those terms with whatever intelligibility they may have for us' (ibid., p. 2).

> for the speaker, an event in which statement and phenomenon are firmly glued together.[149]

This intimate relationship originates in childhood:

> From our very early days we learn to react to situations with the appropriate responses, linguistic or otherwise. The teaching procedures both *shape* the 'appearance', or 'phenomenon', and establish a firm *connection* with words, so that finally the phenomena seem to speak for themselves ... They *are* what the associated statements assert them to be ... [the statements] seem to emerge from the things themselves.[150]

Feyerabend claims that what is instilled in us during childhood is a strong *psychological* connection between words and things — so strong that in many situations we are quite unable to distinguish between words that constitute our interpretations and things themselves. Feyerabend does not reject the dissociation of language and world.[151] He merely says that we are not aware of it. But he does not say is how he knows these things — what, for example, is his source of facts about childhood.

Many philosophical claims about language invite a similar question. Ostensive definition, about which much has been said in philosophy, is a case in point. Whereas normally it would be understood as a kind of naming by pointing (involving linguistically competent persons), in philosophy it can be subjected to arcane metaphysical analyses. The conventionalist Barry Barnes writes:

> Imagine *T* conveying the usage of the term '*x*' by repeatedly pointing to some particular and simultaneously saying '*x*'. As a result, *L* acquires a series of memories of particulars, all associated with '*x*' ... Ostention is an essential element in all verbally mediated learning. It is the ingredient which knots terms to the environment itself. It shows directly the things to which terms properly apply. No account of language learning can omit ostention ...[152]

[149]Feyerabend, *Against method*, p. 58.

[150]Ibid.

[151]In his development elsewhere of so-called 'pragmatic conditions for observation languages', Feyerabend writes: 'they stipulate what is to be the relation between the (verbal or sensory) behaviour of human beings of a class C (the observers) and a set of physical situations S (the situations observed)' (*Realism, rationalism, and scientific method*, p. 18). The assumed disjunction of language and world is quite obvious.

[152]Barnes, *Scientific knowledge and sociological theory*, p. 22. Bloor argues that words are connected to the world by *training* — a conception he claims to derive from Wittgenstein: 'But, insists Wittgenstein, "words are not a translation of something else that was there before they were there" ... The point is not that there is nothing in the world but words. The point is that words are ultimately connected to the world by training, not by translation' (*Wittgenstein*, pp. 27-28).

For all that Barnes' readers know, this may be pure speculation. It is, in any case, very unlikely that the '*usage*' (not just the meaning) of the term '*x*' is imparted by 'repeatedly pointing to some particular and simultaneously saying "*x*".' Indeed, Barnes leaves the supposed imparting of usage by this method swiftly behind, simultaneously elevating ostention to 'the ingredient which knots terms to the environment itself'. Ostention thus facilitates the desired function of language, which is for its terms to apply properly to things.

Of course, dualists have also given straightforwardly philosophical accounts of the function of language. In the following explication of 'reference', Hilary Putnam proceeds directly from a disjunction of 'words and the world', without psycho-historical speculation:

> Reference is a relation between words and the world; this is just a fancy way of saying that the extension of the relation 'refers to' is a class of *ordered pairs of terms and things* ... Any relation which ... maps words onto things is a *words-world relation.* Reference is a words-world relation.[153]

So far I have wanted to draw attention to the fact that the dualist conception of language is usually found associated with a philosophically outstanding function that language is thought to have in science. Grounds for belief in that function either appear bogus or are simply absent.

1.3.2

In philosophical reflections on science, reference, representation, correspondence, and description are the functions commonly assumed to characterise its language. Plausible grounds for supposing that these functions must be given prominence in philosophical analyses of science are hard to come by. Is there perhaps an *historical* explanation to be given for the popularity of the metaphysics of dualism? It might be of interest to list a handful of (quite different) summary observations that would contribute to such a history if properly elaborated.

[153]Putnam, 'Language and reality', p. 283, my emphasis. Putnam's referential model of *meaning* also springs from dualistic assumptions — Hacking remarks: 'He says that meaning is a vector ... First comes the syntactic marker (part of speech); next the semantic marker (general category of thing signified by the word); then the stereotype (... standard examples of its use ...). Finally, there is the actual referent of the word, the very stuff, or thing, it denotes if it denotes anything' ('Experimentation and scientific realism', p. 157).

(a) Rorty argues that 'mirror-metaphors' (variations on the theme that scientific language represents physical things as they really are) played a central ideological role in the Enlightenment in separating the natural sciences off from theology and theological conceptions of knowledge. The metaphors protected the rise of modern science by taking into account sensitivities of the religious authorities of the day.[154] The 'technologizing of the word' (as the medievalist Walter Ong put it) under the authority of the Church depended on a case being made for disinterestedness and objectivity in matters physical.[155] Natural philosophical language was distanced from Scripture by its intentional, *rhetorical* reduction to a (mere) mirror of nature. Rorty argues that that ideology has been enormously influential, but that now it is functionless and simply lingers on. Along similar lines, Nicholas Jardine traces a tradition characterised by what I would call the dualist conception of language to Kepler's *Apologia*, while Steven Shapin argues that an epistemological rhetoric of 'solitude' — conducive of dualism — has dominated modern science.[156]

(b) Historians of literacy have argued that an early form of what I would call the language/world disjunction has its roots in the impact of systems of writing on oral societies. The impact of printing on literate societies in early-modern times is said by these historians to have reinforced the disjunction. For example, Brian Stock writes about the earlier period:

> Texts ... when introduced into a largely oral society, not only created a contrast between two different ways of looking at the world. They also raised the possibility that reality could be understood as a series of relationships, such as outer versus inner, independent object as opposed to reflecting subject, or abstract sets of rules in contrast to a coherent texture of facts and meanings. Experience in other words became separable, if not always separated, from ratiocination about it.[157]

(c) Moving closer to the present, Menachem Fisch refers to an 1831 notebook in which Whewell schematically outlined in table form his view of the inductive method. The scheme, Fisch remarks, was orthodox Bacon. This is Whewell's table:

[154]Rorty, *Philosophy and the mirror of nature*, pp. 328-333. Cf. Shapin, 'History of science and its sociological reconstructions', pp. 170 f.

[155]See Ong, *Orality and literacy*, and idem, *Ramus, method, and the decay of dialogue*.

[156]Jardine, *The birth of history and philosophy of science*, pp. 289 f.; and Shapin, 'The mind is its own place'.

[157]Stock, *The implications of literacy*, p. 531. See also Eisenstein, *The printing press as an agent of change*, ch. 5.

	Sciences of observation	*of experiments*
1.	Common observation	ditto
2.	Decomposition of phenomena	ditto
3.	Classification and nomenclature	Insulation of facts and terminology
4.	Systematic observation and technical description of facts	Systematic experiment and measurement
5.	Induction 1. Propositions concerning classes	Ind.1. Laws of phenomena
6.	Ind.2. Causes of laws	Ind.2. ditto.[158]

Note, to begin with, that language is envisaged to have no function at all at levels 1 and 2. Only after 'observation' has been completed, and only during (or perhaps after) the performance of mental acts of classification, is language called upon to provide terms of nomenclature (level 3). Even at higher levels language remains in a passive or servile role: it assists in the description of facts and the formulation of laws. It is, if we can imagine this, a scientist's in-built amanuensis. Whewell's dualist conception of language is evident in his writings. Fisch quotes him *circa* 1832-33: 'Observation necessarily supposes the powers of perception and *the language* by which its results are *retained and conveyed* cannot exist without nouns and verbs. Hence the power of perception and the faculty of conceiving things *so that nouns and verbs are applicable to them*, are essential to our capacity of knowledge'.[159] Fisch comments on these statements:

> Knowledge ... begins with observation of the external world and is thereafter retained by words — the words being the mind's exclusive contribution ... The *content* of scientific knowledge, [Whewell] maintained, hinges wholly on the input of the senses, whereas the role of mind, in contributing its linguistic setting, confines itself to the capturing and the codification — in a word, to the *representation* — of that which is first found in the facts.[160]

In this image of the mental inner and physical outer, the function of language that preoccupies the mind is the labelling or representational function — the facts, or factual input, are presented to the mind on a platter. In Part 2 I defend the polar opposite of this view: in the pursuit of physical content and structure scientists 'knead' language into an amorphous world — without language, content and structure would not simply dissolve, they would not be possible in the first place.

[158]As reproduced in Fisch, 'A philosopher's coming of age', p. 54.

[159]Quoted in ibid., p. 55, emphasis added.

[160]Ibid.

My brief excursion into Whewell (and by association the Lockean legacy, which is still evident in Kuhn[161]), provides an example of the *philosophical* roots of the dualist conception of language, and complements historical examples of the kind suggested earlier by Rorty and Stock. Although in contemporary philosophy of *language* the philosophical roots of dualism have been severed, they have survived in philosophy of science. Thus in Ernst Cassirer's *Philosophy of symbolic forms* (originally published in 1923), we find a distinctly Lockean conception of language, put to rest in most respects by philosophers of language in the fifties and sixties, yet with certain modifications alive and well among realists and non-realists today. Jan Golinski writes of Cassirer:

> [In his view] philosophy cannot penetrate behind language; it can only discern the way in which language works, namely by attaching labels to sense-data, so that objects are brought within the range of understanding. Linguistic symbols denote objects, and thereby appropriate them within the structure of the mind through which the world is apprehended.[162]

(d) Philosophical and broader historical roots of many ideas are, of course, intertwined. This brings me to another possible source of the language/world disjunction, namely the history of the distinction between primary and secondary properties. In his article 'The dematerialization of matter', Norwood Hanson argues that from the seventeenth century onwards there was an intimate connection between the growth of atomism in science and scientists' dependence on some version of the philosophical distinction between primary and secondary properties.[163] Berkeley's challenge to the distinction did not discredit it in scientific circles because whereas his challenge was, and was seen to be, directed at supposed *real* properties of matter, scientists were still able to maintain the distinction in relation to *physical* properties. It was to the latter properties that the language of science was aimed, at least according to the metaphysics operative in science at the time. But with the rise of contemporary matter theory (by which Hanson means quantum mechanics), metaphysics changed, and any correspondences between the properties now attributed to things like electrons and the classical primary properties which scientists defended in the past became at best analogical. Language, from the new metaphysical viewpoint, could no longer be taken to refer to physical properties as they existed 'unperceived', because contemporary matter theory no longer had room for such properties. Hanson imagines how a critic wishing to preserve the old distinction might respond:

[161]See Locke, *An essay concerning human understanding*, III.2. On Kuhn's phenomenalist version of word/world dualism, see Part 1.2 of this thesis.

[162]Golinski, 'Language, discourse and science', pp. 112-113.

[163]Hanson, 'The dematerialization of matter', pp. 27-38.

> Granted, physics has changed the values appropriate for the property variables *a*, *b*, *c*, *d*, ... still, the primary-secondary distinction remains viable so long as there are good reasons for claiming that fundamental particles *do* have *a*, *b*, *c*, *d*, ... etc. This remains true so long as some properties of aggregates and some properties of components-of-aggregates are distinguishable in that the former result from observer-interaction whereas the latter, however unfamiliar, are such that we have good theoretical reasons for thinking them observer-independent. A theory of the electron is a theory about the properties electrons *have*, not a theory describing what bubbles up out of electron-observer interactions.[164]

Hanson argues that the contrast suggested here by the critic between certain properties particles may be said to have when harnessed to a detector, and certain other properties these particles have when free and unharnessed to any detector, does not exist in contemporary physical theory. There is no room, in other words, for the notion of completely objectifiable properties of particles: 'quantum-theoretic information is always about particles-and-their-detectors-in-combination. Dissolve this combination and you destroy any possible knowledge of the particle'.[165] Of course, different philosophical glosses have been put on quantum mechanics, and I do not mean to side with Hanson's particular interpretation.[166] Nevertheless, the historical content of his article offers an historical perspective from which to view the language/world disjunction, a perspective to be found in the history of the natural-philosophical adoption and subsequent fate of the metaphysics of primary and secondary properties. The dualist word/world disjunction effortlessly complemented that metaphysics, while it was current. The suggested historical contingency of the disjunction of language and world raises (as in Rorty's argument) the possibility that to defend the disjunction today is to defend the defunct but lingering metaphysics of an out-of-date science.

1.3.3

Is science a 'practice of representing'?[167] As I have tried to show, many philosophers of science believe that that is what the language of science is there to do. One of its main functions is to be *about* the world (whether physical or phenomenal), in such a

[164]Ibid., pp. 35-36.

[165]Ibid., p. 27. Cf. Eddington: 'The statement often made, that in modern theory the electron is not a particle but a wave, is misleading. The "wave" represents our knowledge of the electron. The statements is, however, an inexact way of emphasising that the knowledge, not the entity itself, is the direct object of our study; and it may perhaps be excused by the fact that the terminology of quantum theory is now in such an utter confusion that it is well-nigh impossible to make clear statements in it' (*The philosophy of physical science*, p. 51).

[166]On this matter see, for example, Heisenberg, 'The representation of nature in contemporary physics', p. 99; and Honner, *The description of nature*, ch. 2.

[167]Hacking, *Representing and intervening*, p. 136.

way that the world is represented in it.[168] Expressing a variation of this view, Dudley Shapere writes that 'vocabulary' does not merely have the function of encapsulating scientific knowledge in a convenient form to facilitate memory. It is indispensable for a much more important reason. One of the primary aims of science, he writes, is the organisation of knowledge by a suitable language:

> in attempting to 'organise' knowledge, science aims at formulating that knowledge in ways that represent, in a perspicuous way, the world as it is claimed to be. And such representation is accomplished through the vocabulary of science: the propositions of science are to be formulated in terms of *a vocabulary that is fundamental in the sense that it reflects the objects, processes, and behavior* under investigation.[169]

By contrast, Quine writes that 'the general task which science sets itself is that of ... delineating the structure of reality as distinct from the structure of one or another traditional language' — drawing a firm distinction between the structures of reality and language.[170] There is confusion on this issue. I mentioned earlier Kuhn's contention that facts about the world can be 'built into the lexicon'. Rosenberg, advocating a particular linguistic structure for the proper representation of the structure of the world, writes that 'the vehicle of linguistic representation of states of affairs is the (declarative) sentence'.[171] But the underlying issue is never directly confronted: how can the world, from which language is assumed to be disjointed, ever be represented in it? How can a world of objects, processes, and behaviour be 'reflected' in the vocabulary? What does it mean to be hopeful that the language of science 'mirrors the world', or that in this day and age the universe is being represented better than ever before?[172]

Answers to these questions tend to exploit, too briefly, a variety of metaphors (key images are italicised in the quotations that follow). 'Science *attaches* clear and precise significations to fixed signs', writes Merleau-Ponty — 'it *fixes* a certain number of transparent relations and, to represent them, it establishes symbols'.[173] Martin Carrier writes that science, by means of language, 'manages to *induce* the right classification or

[168]The 'concept of reality', Hacking writes, is *entirely dependent upon* the representative function of language (ibid.).

[169]Shapere, 'The influence of knowledge', p. 294, emphasis added.

[170]Quine, 'The scope and language of science', p. 5.

[171]Rosenberg, *Linguistic representation*, p. 2.

[172]On the mirror metaphor, see Rorty, *Philosophy and the mirror of nature*, p. 298 — (on language as a 'public' mirror of nature, see p. 211).

[173]Merleau-Ponty, *The prose of the world*, p. 4, my emphasis.

taxonomy among the phenomena'.[174] Alan Gross argues that because mind-independent reality is non-linguistic, it can only be '*incorporated* directly into knowledge by reference' — 'reference is the only possibility'.[175] Putnam writes that, 'as language develops, the causal and noncausal *links* between bits of language and aspects of the world become more complex and more various'.[176] Here we find, according to Rorty's interpretation, the idea that 'if the world *reaches up and hooks language* in factual (e.g., causal) relationships', we shall always be 'in touch with the world'.[177] Putnam, this time using mathematical imagery, writes: 'the essence of the relation [between language and reality] is that language and thought *do asymptotically correspond to reality*, to some extent at least'.[178]

Other philosophers view language as a way of constructing partial or tentative *pictures* of what there is;[179] or as being 'primarily useful for conveying *generalisations*' of what there is.[180] Giere (relying on a distinction between 'models' and the language used to characterise them, on the one hand, and the world, on the other hand), believes that the relation is one of *similarity*: 'the primary relationship between models and the world is not truth, or correspondence, or even isomorphism, but similarity'.[181] It is, of course, the *image* (or 'model') construed by language (not the language itself) that bears a relation of similarity to the world.[182] Ernan McMullin adopts a variant of this view.[183] Richard Boyd argues that scientific language increasingly provides categories that *cut*

[174]Carrier, 'Establishing a taxonomy of natural kinds', p. 391, my emphasis.

[175]Gross, *The rhetoric of science*, p. 203, my emphasis.

[176]Putnam, 'Language and reality', p. 290, my emphasis.

[177]Rorty, *Philosophy and the mirror of nature*, p. 289; my emphasis. See idem, 'Texts and lumps', p. 3. For another instance of the 'hooking' metaphor, see Gregory, *Inventing reality*, p. 3.

[178]Putnam, 'Language and reality', p. 290, my emphasis. On the *referential* role of scientific terms, see p. 280.

[179]Murphy, 'Scientific realism and postmodern philosophy', p. 296.

[180]Lewis, *Mind and the world-order*, p. 118.

[181]Giere, *Explaining science*, p. 93. I have already quoted Giere as saying that representations are like 'internal maps of the external world'. Would we normally say that maps are 'similar' to that which they are maps of?

[182]For logical empiricists this distinction is less clear-cut: the language of physics is a universal language for two reasons: (i) it best describes what is, and therefore is best-suited to describe the universe as a whole; and (ii) by best describing what is it comes close to *reflecting* what is — of being the universe's own language.

[183]McMullin, 'A case for scientific realism', pp. 14-15.

the world at its joints — a common metaphor.[184] Elsewhere Boyd writes that a linguistically mediated epistemic access to reality necessarily involves modification of linguistic usage by means of which language is *accommodated* to newly discovered features of the world.[185] Echoing the second of Boyd's metaphors, Andrew Pickering writes that the language of science is shaped by 'material resistances': 'scientific knowledge is articulated *in accommodation to resistances* arising in the material world'.[186] Other expressions allude to description and discovery (metaphors in their own right in this context). Clifford Hooker considers the view that the language in which the theories of science are expressed is intended as a *literal description* of the physical world.[187] Popper and Putnam imagine that language describes (much as it was used by Captain Cook to map — and possess — the Pacific) things that scientists *discover*.[188] Quine assumes that the world has 'a single true physical description', although not one that scientists *can* discover.[189]

Many more characterisations of language and its relation to the world can be found in philosophical writings.[190] But there is little beyond such metaphor-like expressions to

[184]Boyd, 'Scientific realism and naturalistic epistemology', pp. 615.

[185]Boyd, 'Metaphor and theory change', pp. 384-385. Cf. Putnam who describes truth, the relationship between language-expressed facts and reality, as '*ultimate goodness of fit*' (*Reason, truth and history*, p. 64).

[186]Pickering, 'Living in the material world', p. 279.

[187]Hooker, *A realistic theory of science*, p. 7.

[188]Popper, *Objective knowledge*, p. 203; Putnam, *Reason, truth and history*, p. 54.

[189]See Sacks, *The world we found*, p. 96. For the distinction between correspondence-as-correlation and correspondence-as-congruence see Kirkham, *Theories of truth*, p. 119.

[190]I have already made the point that claims about the *relation* of language and world usually follow upon *distinctions* made between the two. For such claims see, for example: Bazerman, *Shaping written knowledge*, p. 213, and *passim* (for 'symbolic representation'); Cartwright, *Nature's capacities and their measurement*, pp. 193-194 (for 'simplicity of representation'); Eddington, *The nature of the physical world*, pp. 264-281; Elkana, 'Experiment as a second-order concept', p. 189; Fine, 'The natural ontological attitude', p. 98, for 'stability of reference'; Goldman, *Empirical knowledge*, pp. 3, 211, and 213-214; Gooding, 'How do scientists reach agreement about novel observations?', p. 206 (for the 'translation' metaphor); and idem, 'Magnetic curves', p. 192 (for 'representation'); Hacking, *Why does language matter to philosophy?*, p. 187; Hanson, *Patterns of discovery*, pp. 27-28 (on 'referring' and the representation metaphor); Jardine, *The birth of history and philosophy of science*, pp. 292-293 (for Kepler's 'portrayal of the form of the world' metaphor); Lewis, *Mind and the world-order*, p. 38; Linsky, *Referring*, *passim*; Margolis, *Texts without referents*, p. 262; Nagel, *The view from nowhere*, p. 91; Polanyi, *Personal knowledge*, pp. 114-116; Putnam, 'Poets, scientists, and critics', p.

explain how the supposedly disjointed realms of language and world are related in scientific practice. Metaphors not only cannot *explain* the function of language in science, they give rise to altogether unclear views. Consider, for example, the following argument by Peter Dear:

> the meaning of an account of an experimental event — that which makes it an account of an experimental event rather than a series of marks on paper — is provided by its implicit reference to a spatiotemporally defined region of clinking glassware or grooved pieces of wood being manipulated by a human agent. The meaning of that spatiotemporal region itself — what makes it discernable as an experimental event — is conferred, reciprocally, by the account of an experimental event. In other words, there cannot be an account of an experimental event without reference to the spatiotemporally defined region, while the spatiotemporally defined region cannot *be* an experimental event without its constitution as such in the account.[191]

We see here that in addition to the distinction between the experimental event and the account which 'implicitly refers' to it, Dear also believes that there are two distinct *meanings* — one of the experimental event and one of the account.[192] His expression 'spatiotemporally defined region' is just a technical way of referring to a particular bit of that which other dualists call the world. But the expression is confusing. A spatiotemporally defined region is, according to Dear, distinguishable from any account of it. Yet its name implies that it is also a *defined* region. But who defines it if not the experimental account? Presumably what Dear is wanting to say is that the average experimental account does not reside solely in the imagination of scientists. It is tied

17 (for the view that the texts scientists produce are 'about lumps'); Quine, *Theories and things*, pp. 24-25; Radford, 'Must knowledge — or "knowledge" — be socially constructed?', p. 32 (for the view that language 'stands in an extraneous relation' to the world); Rescher, *Scientific realism*, ch. 4; Rorty, *Philosophy and the mirror of nature*, pp. 3, 267, and 298 (p. 12 for the 'great mirror' metaphor); idem, 'Texts and lumps' (for the distinction between 'texts' and 'lumps'); Russell, *Our knowledge of the external world*, pp. 106 ff.; Salmon, *Scientific explanation and the causal structure of the world*, p. 230; Shapere, 'The influence of knowledge on the description of facts' (for the view that language in science functions to describe and 'perspicuously express relationships'); Skinner, *Verbal behaviour*, ch. 18; Tyler, 'Review of Grace', *passim*; van Fraassen, 'The semantic approach to scientific theories', p. 122; Wittgenstein, *Tractatus logico-philosophicus*, *passim* (prop. 4.01: 'The proposition is a picture of reality. The proposition is a model of reality as we think it is.'); Worrall, 'Fresnel, Poisson and the white spot', p. 155 (for the 'reflection' metaphor); and Ziman, *Public knowledge*, p. 35. See also Whorf's peculiar brand of dualism ('we dissect nature along lines laid down by our native languages'), critically assessed by Cook in 'Whorf's linguistic relativism' — (Whorf's remark is cited in Part 2 of that article, p. 5).

[191]Dear, 'Narratives, anecdotes, and experiments', pp. 136-137.

[192]Dear does not present arguments for these distinctions. See Zahar, 'Review of Dear', p. 101.

down to events that take place in the laboratory and in interactions with nature. But his dualism (which includes a referential function for language) adds a touch of obscurantism to a claim that is incontrovertible.

In many respects Dear's writings are representative of the very latest fashions in the history of science. Dear would fully concur with the thesis that language in science is put to many different non-representational uses — or that its supposed representational function is merely rhetorical. Yet at a philosophical level his analysis is traditional. He conceives of language or discourse ('the account') as an instrument of representation. In line with what Woolgar calls 'the Anglo-Saxon empiricist commitment to a realist epistemology', Dear's analysis assumes that 'on the one hand, there is discourse, and on the other is a separate body of entities to which discourse is addressed and upon which it reports'.[193]

1.3.4

I indicated in the Introduction that the backdrop of *two worlds* (A and B) is useful for understanding dualism, and for developing a refutation of dualism. Both realists and anti-realists postulate a separation, within the interpreted physical world A, of the language of science from the 'body of entities' to which it is addressed. Realists believe that language addresses (interpreted) entities in world A that approximate or are identical with (pre-interpreted) entities in world B. In effect, as scientific ontology is progressively refined, language can be supposed to refer to the 'ready-made' ontology of world B more and more. Anti-realists also believe that language addresses entities in world A. However, these entities are always mind-dependent, and do not even begin to approximate or correspond to the (unknowable) entities of world B (see Table 1).

If we were to employ metaphor to contrast the dualist conception of language with the constructivist conception to be developed in Part 2, we might imagine language under the former conception as *ink* dripping onto the blotting-paper of world B. For dualists, world A is the resulting and ever-expanding *blot*: ink (or language) attaches itself to the pre-existing structure in the blotting-paper. On the (forthcoming) constructivist conception, by contrast, language might be imagined as an ingredient of the *rain* (cultural resources) which falls upon the desert of world B. For constructivists, world A is the resulting and ever-expanding *oasis*: its plants are joint products of the water and the soil

[193]Woolgar, 'Discovery', p. 312.

(language is an ingredient of these products but it is not 'attached' to anything, let alone to a pre-existing structure).[194]

Earlier I argued that unelaborated metaphors bridge the language/world disjunction in philosophical accounts of science. In both realist and anti-realist accounts vagueness also cloaks that body of entities from which language has been divorced. Consider, for example, a reference by Alan Chalmers to 'the nature of the world':

> While the details of an experimental set-up will depend on the theory-guided judgement of the experimenter, as will the significance attached to the results, once the apparatus is activated it is the nature of the world that determines the position of a pointer on a scale, the clicks of the geiger counter, the flashes on the screen, and so on ... It is the fact that experimental outcomes are determined by the workings of the world rather than by the theoretical views of experimenters that provides the possibility of testing theories against the world.[195]

Chalmers is making the very point I attributed to Dear above, namely that the outcome of an experiment as a rule does not spring from the imagination of scientists. Rather, it springs from physical arrangements and events that take place in the laboratory (which includes scientists and their beliefs).

However, Chalmers' appeal to the nature of the world serves only to obscure this point. For at face value Chalmers is saying that the experimental outcome is dependent not only upon the experimental set-up (the environment of the laboratory), but also upon something quite different, not directly accessible to experimenters, and fully differentiable from their 'theoretical views'. That something he calls the nature of the world. But what is the nature of the world that can determine the position of a pointer on a scale, the clicks of the geiger counter, etc., *independently* of the judgement and theoretical views of scientists? Chalmers does not say. He could, of course, respond by saying that an experimental setup allows for a range of outcomes, and that the actual outcome is determined by the nature of the world. This response, however, preserves the unexplained differentiation between laboratory contents (apparatus, material changes, experimenters, theoretical views), and the nature of the world. It fails to elucidate the latter expression in terms that are independent of the former. And it raises the question of the grounds Chalmers has for claiming the distinction.

[194]Obviously, this metaphor for constructivism is deficient: world B, unlike a desert, has no structure or ontology (see pp. 19-21).

[195]Chalmers, *Science and its fabrication*, pp. 71-72.

Of course, in the passage above, Chalmers is not attempting to elucidate 'the nature of the world'. But there is also no other place in the book to which the passage belongs where he attempts to do so. The distinction between the setting up of the experiment and its 'activation' — at which point the experimenters stand back and the nature of the world takes over — a distinction echoed by Bazerman, who writes that 'once the experimenter sets up the conditions of the experiment, what turns up is beyond his control'[196] — raises the issue whether 'what turns up' depends on *the expression the result is given*, and the significance attached to it, by the experimenter.

Some references to 'the world' are not only mysterious, they seem spurious. Writes Hacking: 'the *reference* of a natural kind term is the natural kind in question ... The reference of 'water' is a certain kind of stuff, namely H_2O'.[197] Relying on the disjunction of language and the subject matter of science, Hacking is stating what a natural kind *term* refers to. In the case of 'water' (as in the case of other natural kind terms) it refers to a certain kind of stuff. Specifically, the reference of 'water' is H_2O. But this, according to Hacking's mode of expression, is like saying that the reference of 'water' is the reference of the scientific term 'H_2O'. In attempting to say that a piece of 'language' is *about* something in 'the world', Hacking resorts to another piece of language ('H_2O') to specify what that something is. 'A certain kind of stuff' — elsewhere: 'the actual referent of the word, the very stuff, or thing, it denotes if it denotes anything'[198] — is quickly substituted with 'the reference of "H_2O"'. Hacking does not shed light on the supposed constituents of the world (the structure of the blotting-paper) that stand dissociated from language, other than by providing alternative linguistic characterisations of those constituents.[199]

It may seem a tall order to expect a philosopher to demonstrate the language-independence of the world without using language to characterise its contents. However, at stake is not a detailed characterisation of the *contents* of the world, but a demonstration of its language-independence. When dualists make reference to the

[196]Bazerman, *Shaping written knowledge*, p. 208.

[197]Hacking, *Representing and intervening*, p. 80. For a criticism of this thesis, see Kuhn, 'Dubbing and redubbing', pp. 309-313. Cf. Shapere, 'Reason, reference, and the quest for knowledge', pp. 3 f.

[198]Hacking, 'Experimentation and scientific realism', p. 157.

[199]Cf. Gregory: 'The sentence in the metalanguage tells us the word *electron* refers to something in the world called a subatomic particle. But notice that reference is a relationship between the word *electron* and the words *subatomic particle*, not between words and something that is not words. The observations with which physicists compare their predictions are not some mute expression of the world' (*Inventing reality*, p. 191); and Gross, *The rhetoric of science*, p. 11.

'stuff' of the world, they invariably proceed quickly to cloak and itemise that stuff in language. They afford no insight into its language-free state. Nothing by way of a *philosophical* clarification is given of the world to which language is addressed.[200]

Pickering, in the course of putting forth a view he calls 'pragmatic realism', commits himself to the existence of what he calls 'the material world'.[201] His aim, he says, is to look into 'the relation between articulated scientific knowledge and its object, the material world' — or alternatively, the relation 'between performances in the material world and our understanding of them'.[202] *What does Pickering mean by the material world?* He claims that, 'a glance into any scientific laboratory reveals a range of engagements with the material world' — scientists here are 'continually handling material objects'.[203] Perhaps all that he means by this is that scientists handle test tubes, chemicals, substances whose compositions and properties are unknown, etc.

Yet what Pickering means by 'material world' cannot in fact be reduced to such things. He implies that the material world lies beyond all superficial appearances. In his scheme the material world gives rise to 'resistances' that are brought to the attention of scientists in the course of 'material procedures' (namely, 'experimental action in the material world' and, notably, '*nonverbal* interaction with the material world'[204]), and are 'translated' with the help of 'instrumental models' into 'phenomenal models' (which 'endow experimental findings with meaning and significance' and, of course, language).[205] Scientific knowledge, Pickering writes, 'is articulated in accommodation to resistances arising in the material world'.[206] But while the 'material world' is all-important in his scheme (the expression saturates the text), all that is said about it is that it is the object of scientific knowledge and language, and the source of ill-defined 'resistances'.

[200]See, for example, Giere, *Explaining science*, ch. 5; Franklin, *The neglect of experiment*, *passim*. Examples of this kind abound.

[201]Pickering, 'Living in the material world', *passim*.

[202]Ibid., p. 275.

[203]Ibid., p. 276. One would like to ask, handling material as opposed to *what* objects?

[204]Ibid., p. 285, emphasis added.

[205]Ibid., pp. 276-277, and footnote 4. Within this scheme lies Pickering's espousal of dualism: scientists observe signs of resistance in the material world and then express them in language to give them meaning.

[206]Ibid., p. 279.

Like Dear and Chalmers, perhaps all that Pickering is wanting to say is that scientists modify their experiments and their views about them in the light of what happens in the course of (cognitively compelling) experiments. But in order to say *that*, all three philosophers have fitted scientists' actions around a philosophical notion of 'world' about which they have very little to say by way of introduction or justification. The main problem for Pickering is his dissociation of language from the 'material world' to which it supposedly refers, not the idea (which is defended later in this thesis) that scientists engage an uninterpreted physical world and construct meaning in the course of such engagements.[207]

In realist versions of dualism the disjunction of language and world goes hand-in-hand with the claim that features (structures, properties, etc.) of the world remain just what they are *whether or not scientific language reaches out to represent them*, that is to say, independently of what scientific language purports to say of them.[208] Consider, for example, the following passage by Roy Bhaskar:

> We can easily imagine a world similar to ours, containing the same intransitive objects of scientific knowledge, but without any science to produce knowledge of them ... In such a world, which has occurred and may come again, *reality would be unspoken for* and yet things would not cease to act and interact in all kinds of ways. In such a world ... the tides would still turn and metals conduct electricity in the way that they do, without a Newton ... to produce our knowledge of them. The Widemann-Franz law would continue to hold although there would be no-one to formulate, experimentally establish or deduce it. Two atoms of hydrogen would continue to combine with one atom of oxygen and in favourable circumstances osmosis would continue to occur.[209]

According to Bhaskar, in our world, in which reality is as a matter of fact *spoken for*, language is about the world in the sense that it corresponds to actual features of the world. We can easily imagine a world in which reality is unspoken for, Bhaskar says — but is it really so easy? Instead of demonstrating the possibility of an imaginary world in which reality is unspoken for, Bhaskar merely projects high-school facts about our actual world (electricity, atoms, etc.) onto a world that is not, as a result, difficult for our imaginations to grasp (all it lacks is people and language: add these to Bhaskar's ready-made world B and you get a world A that differs little from the

[207]Pickering's notion of 'accommodation to resistances' is freed of dualist connotations in Part 2.2.6, below.

[208]'Scientific realism ... is the view that if a scientific theory is in fact true then there is in the world exactly those entities which the theory says there is, having exactly those *characteristics* which the terms of the theory describe them as having' (Hooker, *A realistic theory of science*, p. 7, emphasis added).

[209]Bhaskar, *A realist theory of science*, p. 10, emphasis added.

original — it simply mirrors it in words). Bhaskar intends to introduce a reality that is unspoken for, but quite clearly and unequivocally *speaks for* a particular reality.[210]

Another realist, Rom Harré, unexpected author of a preface to a book by the social constructivist Karin Knorr-Cetina (subtitled *An essay on the constructivist and contextual nature of science*), warns readers: 'Of course, science only makes sense as a realist enterprize, an attempt, using the means at hand, to truly represent physical reality as it is'.[211] He writes elsewhere that most scientific instruments 'take up a definite state when acted upon by the world *which is also in a definite state*'.[212] According to Harré there is just one way in which language can *truly* represent reality, and that is to represent it as it is, in its definite state. But once again how *is* it over and above how it is represented? Concurring with Harré, Clifford Hooker defines his realist view as one in which 'the intended and proper sense of the theories of science is as literal descriptions of the physical world, as saying what there is and how it behaves'.[213] Of course, what Harré's and Hooker's remarks tell us about the world beyond language (but this is *all* that they tell us) is that that world has a definite 'pre-linguistic make-up', of which scientific accounts (descriptions in language) may be given, of greater or lesser accuracy.

The paramount criterion for the correct representation of the world in language according to realists is to be found not in language but in the world.[214] It is to the constituents of a pre-given and ontologically delineated world of natural facts that the linguistic constructions of scientists refer, when they refer, and it is to these constituents that language is 'accommodated'.

[210]Cf. Latour and Woolgar, *Laboratory life*, p. 178, who make the same point.

[211]Knorr-Cetina, *The manufacture of knowledge*, p. viii. Cf. Nagel: 'the world is in a strong sense independent of our possible representations ... [objectivity's] aim and sole rationale is to increase our grasp of reality, but this makes no sense *unless the idea of reality is not merely the idea of what can be grasped by those methods*. In other words, I want to resist the natural tendency to identify the idea of the world as it really is with the idea of what can be revealed' (*The view from nowhere*, p. 91); and Bhaskar, *Scientific realism and human emancipation*, p. 5.

[212]Harré, *Great scientific experiments*, pp. 26-27, my emphasis.

[213]Hooker, *A realistic theory of science*, p. 7.

[214]Herbert writes: 'I do not wish to get specific about what might be meant by 'reality itself' lest we hamper our search with needless preconceptions', however, 'deep reality will no doubt carry its own validation: we'll know it when we see it' (*Quantum reality*, p. 5).

In the Introduction to this thesis I set out my conception of world B, a world that has no ontology and cannot (therefore) be known. Unlike world B, which lies beyond the boundaries of knowledge and is not (therefore) inaccessible or mysterious in a strict sense, dualist conceptions of 'the nature of the world', 'the actual referent of the word',[215] 'natural reality',[216] 'the material world', 'mind-independent reality',[217] 'deep reality',[218] 'brute nature',[219] 'the final ultimates of reality',[220] 'the concrete actuality of the empirical world',[221] 'the world of natural phenomena',[222] and, in general, 'a certain kind of stuff' beyond language, appear destined to remain inaccessible and mysterious in both realist and anti-realist metaphysics: they set up a realm that is independent of language, but because this realm can only ever be known *through* language, its independence cannot be convincingly imagined, explained, or illustrated.

In this and earlier sections I have attempted to show that assumptions about language/ world dualism in writings about science are widespread. The assumptions have lead many philosophers of science to express a very narrow conception of language and its functions in science (the dualist conception), and a confident but essentially empty account of 'the world' beyond language.

1.4 Dualism in pre-consensual uses of language

The history of science furnishes evidence of a fundamental disjunction between language and world, and thus may seem to speak for the validity of the fundamental component of the dualist conception of language. This evidence is especially plentiful from periods of low consensus on how to talk about (and how to handle) natural phenomena of great scientific concern. In this section I discuss briefly a period in British history when an important chunk of the formal language of medicine was regarded as problematic and the disjunction of language and world appears to have been favoured by many.

[215]Hacking, 'Experimentation and scientific realism', p. 157.

[216]Livingston, *Literary knowledge*, p. 35.

[217]Gross, *The rhetoric of science*, p. 203.

[218]Herbert, *Quantum reality*, p. 5.

[219]Bazerman, *Shaping written knowledge*, p. 201.

[220]Harré, *The principles of scientific thinking*, p. 260.

[221]Rickert, *The limits of concept formation in natural science*, p. 36.

[222]Pickering, 'Living in the material world', p. 277.

1.4.1

> The controversy upon the contagiousness or non-contagiousness of the yellow fever of the West Indies is as remarkable as any known in the history of medicine for jarring opinion and irreconcilable evidence: for the numbers who fight for victory with obstinate and intemperate zeal, and the few who with moderation and firmness contend for truth.[223]

Thus remarked the editor of the recently established periodical *The London medical repository* in 1819, referring to what was at the time widely acknowledged to be 'one of the most interesting problems that can occupy the consideration of the medical philosopher'.[224] He was being over-optimistic about the accomplishments of 'the few', as in 1819 there was precious little agreement about who they were, and disagreement about the cause and mode of transmission of yellow fever would last for decades.

In a battle often portrayed as one between 'contagionists' and 'anti-contagionists' — but there were also 'contingent contagionists' and many other varieties — yellow fever was just one terrible battleground. (In the United States, for example, there were yellow fever outbreaks every single year between 1800 and 1879.[225]) Battles were also fought over typhus fever. And of course there was cholera — in the period 1831-32 alone Asiatic cholera spread through Britain killing more than 30,000 people[226] — and the dreaded plague of the Levant, which had devastated Europe in earlier times. The highly controversial British quarantine system, a disruptive form of government intervention in commerce developed to defend against epidemics, polarised opinion in the medical world. This and other 'unifying' measures, leading to the treatment of all epidemic diseases as essentially alike, meant that a doctor's opinion on the method of propagation of, say, yellow fever, was likely to coincide with his opinion on the method of propagation of plague. At stake was an understanding of the nature of fever itself, a single explanatory framework for all communicable diseases. Consensus on

[223]Blane REVIEW (1819), p. 225. In this section, dated citations refer to entries in Part B of the Bibliography, which is arranged in chronological rather than alphabetical order. 'REVIEW' signifies a review of a work (usually a book, but occasionally an article), and comes immediately after the name of the person whose work is reviewed (when a large number of works is reviewed the name of the subject matter is given instead; 'NOTICE' refers to an unsigned announcement on a particular disease). The reviewer's name is not given because reviews at the time generally appeared as unsigned articles. In many cases they were written by the editors of the medical journals themselves. First names of contributors (and reviewees) were not always printed. The title 'Dr' often appeared instead.

[224]Bancroft REVIEW (1812), p. 324.

[225]Winslow, *The conquest of epidemic disease*, p. 193.

[226]Durey, 'Medical elites', p. 257.

this matter was to begin to emerge only in the 1860s and seventies. Meanwhile, in 1819, and for the first half of the nineteenth century, opinions varied greatly.[227]

In respect of the variety of opinions the situation early in the nineteenth century was not unlike what it had been half a century or a century earlier. But in another respect a new factor had already made an important difference. *Medical periodicals* began to appear regularly in the closing decades of the eighteenth century, and the number of titles increased greatly in the opening decades of the nineteenth.[228] The periodicals provided physicians and others interested in illness and disease ('medical profession' is too presumptuous a term for this time) with unprecedented opportunities to publish their views. And publish they did, documenting as never before the diversity of opinions prevailing in most areas of medical practice.[229]

[227]For background to the early nineteenth-century medical world, see, among others, Ackerknecht, *A short history of medicine*, pp. 145 ff. ; Cartwright, *A social history of medicine*; Morrell, 'Individualism and the structure of British science in 1830'; Peterson, *The medical profession in mid-Victorian London*; and Shapin, 'Nibbling at the teats of science'. The classic account of contagionism vs. anti-contagionism in the first half of the nineteenth-century is Ackerknecht, 'Anticontagionism between 1821 and 1867'; see also, Parsons, 'The British medical profession and contagion theory'; for an earlier history of these debates see Singer and Singer, 'The development of the doctrine of contagium vivum, 1500-1750'; and Nutton's two definitive articles, 'The seeds of disease', and 'The reception of Fracastoro's theory of contagion'. A classic book on the history of epidemics is Winslow, *The conquest of epidemic disease*. See also, Ackerknecht, 'Hygiene in France, 1815-1848'; Delaporte, *Disease and civilization*; Hudson, *Disease and its control*; Pelling, *Cholera, fever and English medicine 1825-1865*; and Smith, 'Gerhard's distinction between typhoid and typhus'. For an account of the theoretical background to the nineteenth century, see Riley, *The eighteenth-century campaign to avoid disease*. On quarantine see McDonald, 'The history of quarantine in Britain during the 19th century'; and Mullett, 'A century of English quarantine'. On the beginnings of a consensus on the causes of disease and epidemics late in the nineteenth century, see Carter, 'Translator's introduction'; idem, 'The Koch-Pasteur dispute'; Reiser, *Medicine and the reign of technology*, pp. 83 f.; and Winslow, *The conquest of epidemic disease*, pp. 267 f.

[228]For a clear view of this dramatic growth, see Lefanu, *British periodicals of medicine*.

[229]Publishing an 'article' was naturally much easier than publishing a book. The job was made even easier by the fact that editors would accept and publish 'long letters', which meant that the views of physicians with a knowledge of written English and access to a postal system could now be published from any part of the world.

The new medical journals revealed afresh the powers of the printing press.[230] That which for centuries had been difficult to achieve through treatises and personal correspondence was made relatively easy within a few years of the rise of the medical serial press. Large audiences were soon addressed from a variety of medical viewpoints, as many viewpoints were compounded into single journal volumes.[231] Editors of journals took pleasure in pointing out that they had gained the cutting edge in medical communication:

> There are few diseases which require systematic treatises to be written on them, and not many persons would take the time and trouble of reading them if they were written, especially at the present period, when every information may be conveyed and acquired with such facility through the medium of the periodical press.[232]

Their scorn for many books, whose contents they saw as politically and medically conservative, was often expressed in stark terms.[233] Besides 'original communica-

[230]The effects of periodical publication on the early nineteenth-century medical world (and on its language) have yet to be studied closely. A general (non-medical) framework for approaching this complex issue has already been set out in Eisenstein, *The printing press as an agent of change*, and applied more specifically by Bazerman, *Shaping written knowledge*, chs. 3-5. A recent study by Bartrip (*The mirror of medicine*) unfortunately takes off quite late in the nineteenth century. Porter's 'Laymen, doctors and medical knowledge in the eighteenth century' concerns an earlier period and a different set of problems; his 'The early Royal Society and the spread of medical knowledge' goes back to the seventeenth century when the Society's *Transactions* first began to appear. For the 'journalisation' of another branch of science at around the time I am concerned with here, see Broman, 'Reil and the "journalization" of physiology'.

[231]Hays notes that, by 1850, 'periodicals edited and published in London increasingly dominated the diffusion of scientific ideas to the literate of the country' ('The London lecturing empire, 1800-50', p. 91). That is not to say that treatises ceased to be important. One of the most important American publications on yellow fever was a two-volume treatise by R. LaRoche (*Yellow fever, considered in its historical, pathological, etiological and therapeutical relations* [1855]), discussed in Wilson, *The conquest of epidemic disease*, ch. XI. Books continued to be important until the twentieth century. Nevertheless *printed debate* over books was the domain of the periodical press, and so was multilateral debate in general. The smooth presentations contained in books stood in contrast with the contents of periodicals, whose language was a better approximation to the language (and the state of the language) in use.

[232]Boggie REVIEW (1828), p. 18.

[233]Reviewers would damn books with faint praise: 'We are positively driven to notice the work before us, for want of a better performance ... among the four or five other works ... that have appeared within

tions' and 'observations' (which came from as far away as India, or were penned on board ships calling at ports in the tropics), transcripts of lectures, and other brief reports, the journals published critical book reviews and retrospectives of 'medical progress', and sustained (and sometimes encouraged) debates among their contributors. Opportunities and demands created by the new communication technology, especially in the context of widespread concern with epidemic fevers, put a great strain on medical language. The strain was noticed and commented upon by contributors and editors alike.

1.4.2

Signs that medical language was a source of difficulties in the British medical world are evident from early on in the life of the periodical media. Referring to articles on the question of the contagiousness or non-contagiousness of yellow fever Thomas Dancer remarked in an 1805 issue of *The medical and physical journal*:

> A science like medicine does not always admit of definite language; this is apparent on many occasions, and is partly so, if not particularly so, with respect to the terms, Infection and Contagion, from the confounding of which, or from assigning to each, new and not well defined significations, proceeds in a great measure the obscurity that rests on the question under discussion.[234]

Dr Rodgers, in an interview with a committee of the Board of Health of New York, expressed a similar sentiment:

> As the term malignant fever appeared to us of ambiguous import, we next inquired if [Dr Rodgers] considered the fever referred to as the yellow fever: to this question he answered, he disliked the term yellow fever, considering it an improper one, but admitted that the disease was such as would by many persons be denominated yellow fever.[235]

A reviewer for *The London medical and physical journal* warned that terms used loosely were a curse on everyone. He advised those who asserted that variola is communicable from person to person to be 'careful in their language'. For they who argue thus, he continued, 'apply to words in common use uncommon significations: a plan admirably calculated to mislead their hearers and confound themselves'.[236] Words and their usage, the point seems to have been, did not always do justice to

the last year ... Dr Chisholm's volume is the least reprehensible' (Chisholm REVIEW [1822], pp. 406-407).

[234]Dancer (1805), pp. 385-386.

[235]Board of Health (1812), p. 169. Cf.: 'Dr Bancroft animadverts on the want of precision with which the term Yellow-fever has been used, and to which he attributes much of the ambiguity that has prevailed respecting the nature of the disease' (Bancroft REVIEW [1812], p. 326).

[236]Aiton REVIEW (1832), p. 488.

reality. Dr Ranken's decision to name the fever that struck parts of northern India in the summer of 1836 the 'Pali plague', was swiftly condemned in Edinburgh:

> The term Plague has been indeed applied so generally to every disease of which the approach is sudden, the symptoms intense, the progress rapid, and the mortality considerable, that it is not wonderful that, in the loose and vague manner in which various professional persons not unfrequently think and express themselves, this epidemic has also been designated plague.[237]

It was said, moreover, that the diversity of observations of 'local circumstances or particular symptoms' made its way into the press through 'appellations' that were 'unscientific and absurd':

> Thus, in India there is a 'Hill fever', from its occurring in the mountains; and a 'Jungle fever', from its occurring in the woods; and, in England, a 'Fen fever', from its occurring in the marshes: and this nomenclature we often find formally employed in medical dissertations![238]

Joseph Arnold thought it scarcely worth his while to enter into an argument on the nature of typhus for it was bound to degenerate into 'a war of words'.[239] Key words in this area had ceased to designate, and to engage with them was pointless. 'I have long ago been well convinced of the futility of all medical controversy', declared Colin Chisholm in disgust.[240] Having pondered long over a work by Mr Fraser, a reviewer wrote in despair: 'it is impossible to say what Mr Fraser's notions on the subject of contagion certainly are'.[241] It was common practice for one party to declare the views of another 'unintelligible'.[242] A reviewer of a book by Sir Gilbert Blane quipped about one of its chapters: 'the chapter is entitled, "on the Ambiguity of Language"; but in reality it is a dissertation on the yellow fever'.[243] The French were said to be experts at rendering words meaningless. 'The next paper', wrote a reviewer for *The Edinburgh medical and surgical journal* in reference to an excerpt on the subject of yellow fever from a French medical dictionary, 'shows that, if we often accuse our countrymen of confused terms and want of arrangement, they are complete philologists and logicians, compared with the French pathologists'.[244]

[237]Ranken REVIEW (1839), p. 247.

[238]Maclean REVIEW (1818), p. 58.

[239]Arnold (1809), p. 17.

[240]Chisholm (1813), p. 413.

[241]Fraser REVIEW (1829), p. 154.

[242]For example, Hawker (1805), pp. 337-338.

[243]Blane REVIEW (1819), p. 225.

[244]Yellow Fever NOTICE (1817), p. 38.

Words had to be brought into line with the facts, it was said. Dr Roberton, in a report from Edinburgh in 1808, urged English speakers to lay aside

> those mere names that have in every age obstructed scientific researches, and on which all reasoning upon these subjects was founded ... and no terms made use of in the proof of a fact but such as can be more easily explained than the fact itself.[245]

Whether as a result of such advice, or simply because they were anxious to get their views across, a great number of contributors to the British journals began their contributions with *a list of definitions*. This, indeed, is one of the most striking features of early periodical writing. An alternative to the silence advocated by the likes of Dr Arnold, it was an obvious remedy for uncertainty about significations and the supposedly collapsing symbolic system, hinted at in remarks above. As Dr James Veitch put it in 1818, it had become important to 'commence [an article] by an *exposition of the terms of designation* ... it is of much moment to attach precise and distinctive notions to their special bearings'.[246]

Some took the opportunity on such occasions to sound self-righteous. Dr Larkin required more than three generous pages to explain 'a few of the various and opposite significations in which the words *contagion* and *infection* have been used':

> There is such a total want of precision in the meaning attached to them, that, unless I clear away verbal difficulties, and define the sense in which I shall use this word, contagion, we shall be involved in an interminable dispute ... I enlarge the signification of infection, and make it embrace every thing and circumstance that has a tendency to vitiate the atmosphere, whether confined or free.[247]

It would seem that 'infection' was so problematic a term that Larkin could simply define it or redefine it as he liked. Sir Thomas Maitland, writing in the same year, prefaced his own definitions with a justification almost identical to Larkin's. Before answering the question whether the plague is acquired by contagion or infection, he wrote:

> it is absolutely necessary that we should come to a perfect and clear understanding of the meaning of these two words. In many publications they are not defined at all; in some they are most strangely confounded; and in not a few they are altered exactly as suits the argument of the individual at the moment.[248]

[245]Roberton (1808), p. 365.

[246]Veitch REVIEW (1818), p. 489, emphasis added.

[247]Larkin (1825), pp. 265-267.

[248]Maitland (1825), p. 118.

Other definitions were phrased to sound like mere reminders. David Hosack, introducing his article on 'the typhoid state of fever', modestly noted:

> Fever, in the opinion of the writer of these remarks, is a disease of the whole system; it appears no less in all the faculties of the mind than in all the functions of the body; it shows itself in every organ of our frame, and affects every nerve and fibre of our system.[249]

Such highly controversial definitions could be arranged to sound like commonplace beliefs. In the following we find a definition of the epidemic potential of the atmosphere (another highly controversial matter) masquerading as a well-worn truth:

> It is certain, and *an axiom in natural philosophy*, that nothing in the whole of nature has a more general, sudden, uniform, and greater influence upon all classes of mankind, than the atmosphere.[250]

But the greater number of definitions were set out matter-of-factly, for this had quickly become the convention, as in the case of Dr Adams' opening words in an 1832 article on the causes of epidemics:

> An *epidemical* disease is a disease which seizes upon a number of persons at a time, or during a particular period: an *endemical* disease is a disease to which the inhabitants of any district or country are subject: a *sporadic* disease is one which is confined to individuals ... [and so on].[251]

Of course, definitions themselves could be, and were regularly, misunderstood. Many contributors began with the good intention of at least keeping the order they themselves had imposed on their words, but then failed to abide by their definitions, or ignored them altogether. Attempted clarity through definitions soon failed to appease the ever-watchful journal editors, who had become increasingly cynical anyway. One reminded future contributors:

> Before authors discuss a definition of fever, it should be settled what is meant by the term. Does it mean any given state; or any given disease; or any given stage of a disease? — because the definition must vary accordingly.[252]

Charles Maclean, the great 'anti-contagionist' who spent a year at the Greek Pest Hospital at Instanbul demonstrating to his satisfaction that the disease could not be propagated through contact with the sick, did not believe that definitions could correct an unreliable, unworkable language. He alluded to the terms 'epidemic', 'pestilential', 'contagious', and 'infectious':

[249]Hosack (1816), p. 354.

[250]Domeier (1805), p. 105, emphasis added.

[251]Adams (1832), p. 182.

[252]Smith REVIEW (1830), p. 235.

> I deem it necessary previously to state what are the precise ideas which I affix to certain terms frequently employed in the course of the discussions which have arisen on this subject, and which have been variously interpreted; not that I entertain the vain expectation of being able, by that means, to obviate being misunderstood, misinterpreted, or misrepresented; but that I may be able to justify, when necessary, my own conclusions upon my own principles.[253]

A few resorted to etymology as a method of dealing with the uncertainty afflicting the use of medical terms, even though most projects grounding language in more language drew immediate criticism: 'The etymological import [of 'infection' and 'contagion'], so critically ascertained in a late ingenious Thesis, by Dr. Bayley, is of little moment', concluded Dr Dancer typically.[254]

1.4.3

Remarks of the kind assembled above occurred plentifully in the new press. It is not usual in the history of science to find problems of language aired with such strong feelings by so many authors over such a long period of time (roughly 1780-1850). Does that wave of emotion reveal a belief in the disjunction of 'language' and 'world'? Does it express a conception of the proper function of language as being one of correspondence with the world? It certainly seems that participants in the debate on disease propagation had lost faith in the ability of language to describe or refer to the world. With the bridging function of language in disrepair, they were left, it would seem, with a gaping language/world disjunction.[255]

[253]Maclean (1819), p. 116.

[254]Dancer (1805), p. 386; for other 'philological' researches see Chisholm (1810), pp. 404 f. For various expressions of frustration with language not mentioned above, see Harris (1803), pp. 25 and 26; 'Inquirer' (1805), p. 429; and Blane REVIEW (1819), *passim*. On the definition of terms, see also Blackburne REVIEW (1803), p. 464; Patterson (1803), p. 108; Blane (1807), p. 388; Miller (1807), pp. 280-281 and 289; Bancroft REVIEW (1812), p. 335; Blane (1816), p. 23; Dickinson (1817), pp. 462 f.; Dickson (1817), pp. 35 f.; Veitch REVIEW (1818), pp. 489-490; Dickinson REVIEW (1819), pp. 481-482; Nicholl (1821), *passim*; Coventry (1822), p. 182; Jackson REVIEW (1822), pp. 28-29; McGhie (1822), p. 370; Ferrari (1823), p. 368; Foderé REVIEW (1823), pp. 153-155 and 246; Chambers (1828), p. 321; Wilson REVIEW (1828), pp. 190-195; Fraser REVIEW (1829), p. 154; Lawrence (1829), p. 33; Barry (1831), p. 479; Fraser (1831), p. 210; Guyon (1831), p. 289; Fergusson (1832), pp. 68-69; Gregory (1832), *passim*; and Alison REVIEW (1840), p. 206.

[255]An argument to this effect might run as follows. Whereas in times of 'normal' science, people (minus philosophers) don't notice the difference between the two, in times of crisis the distinction between language and world becomes obvious because it is not clear in those times what referents scientific terms have. The example from medical history shows that doctors were highly aware of the fact that language had lost its primary function of being *about* aspects of the world.

This conclusion loses all plausibility, however, when semantic problems with the language of fever are considered in their proper context, textual and social. The linguistic preoccupations of doctors and surgeons do not betray awareness of a language/world dichotomy, nor do they justify such a metaphysical arrangement. A partial reconstruction of the *textual* context of their concerns is presented below.[256]

Journals quickly became the locus of a highly charged debate on disease propagation. Dr Chisholm, a contagionist and defender of the quarantine system, launched merciless attacks on the non-contagionist Dr Bancroft and 'the opposition':

> [They] are reduced to the necessity of railing, because we do not choose to adopt their law, and, with their systematic good opinion of themselves, assure everybody that differs from them, that they are a parcel of blockheads, and understand nothing of the matter'.[257]

Bancroft's essay, Chisholm would claim, is 'as coarse in its language, as it is furious in its matter'.[258] Bancroft retorted that the contagionist's work was nothing less than 'a climax of contradiction and absurdity'.[259] Chisholm's ability to reason had been doubted some years earlier by another critic: '[Chisholm] has detailed many of his facts in such a loose, incoherent, and slovenly manner, and has viewed objects so much through the vague and unsettled medium of *theory* ... that he has greatly diminished the weight of his authority'.[260]

From an adjacent battleground Mr Royston, a reviewer for *The medical and physical journal*, had this to say about an article expounding the 'animalcular' hypothesis:

> Dr John Crawford ... endeavours by a long train of reasoning to prove ... [that all] febrile infections, are consequences of animalcular action upon human bodies. This, the wildest of philosophical vagaries, has taken full possession of Dr Crawford.[261]

[256]As I mentioned in footnote 230 above, a book-length history of this period of uncertainty is lacking.

[257]Chisholm (1813), p. 413.

[258]Ibid.

[259]Bancroft (1814), p. 329; cf. p. 326.

[260]Anonymous (1802), p. 314.

[261]Royston (1810), p. 23.

Such language was common.[262] The emotion it expressed was largely frustration with the apparent complexity of the diseases themselves, the widespread disagreement over fundamentals and observables, and the complete lack of any progress towards consensus.[263] The periodical press, by capturing in print and making more accessible the variety of opinions and practices, aggravated the frustration of interested readers. Already by 1822 the problem of yellow fever had occasioned views and attitudes of such diversity that a reviewer for *The London medical repository* was convinced that the disease:

> possessed so many characters, — some of them of a nature so opposite and so mutable, according as it was met with in an epidemic or endemic form, as it arose from causes of greater or less intensity or complication, and as it was witnessed within or without the tropics, or in the eastern or western hemisphere. These circumstances, with the accidental occurrence of a crowded population, — a confined and frequently breathed atmosphere, — foul air arising from the ill-ventilated apartments of the sick, — and from other sources of impurity, whether proceeding from the soil, or from any other cause, — and the varying disposition of the subjects who become exposed to these influences,

could not fail to provide arguments for practically any view.[264] Knowledge of cures was in no better state. Dr Faulkner, who had been following the plague in Malta, noted that whereas 'laws' and 'cures' were plentiful, none enjoyed anything approaching generality:

> Every attempt to accommodate the phenomena of plague to the operation of general laws, or to discover any thing approaching to a successful method of cure, either by experiment or speculation, has shared the same unfortunate fate as in all former ages.[265]

'Considering that all the functions are so violently affected in continued fever', remarked Dr Peaal, 'it is not to be wondered at that we are at a loss to form proper indications of cure — the manner in which the causes operate are hid from our view'.[266] Two factors, diversity of opinion and helplessness in the face of disease, took a serious toll on morale. These factors are an important part of the context of the remarks on language presented earlier.

[262]For other examples, see Adler (1807), p. 505; Burnett (1816), p. 441; and Yellow Fever NOTICE (1817), pp. 45-46.

[263]On the variety of views see, second hand, Latour, *The pasteurization of France*, pp. 20 f. On the lack of progress see, first hand, Townsend REVIEW (1824), p. 339.

[264]Jackson REVIEW (1822), p. 21. See the three-part table listing contagionists, anticontagionists, and 'contingent contagionists' by name, ibid., pp. 22-24.

[265]Faulkner (1814), p. 137.

[266]Peaal (1802), p. 322.

A reviewer of a long list of works on yellow fever expressed himself in a manner common to those involved with the periodical press, when he concluded :

> Any one acquainted with the process of induction employed in other sciences would imagine, that parties maintaining opinions so diametrically opposite must necessarily have deduced their conclusions from different facts, or at least different kinds of facts.[267]

Sarcasm aside, there was a grain of truth in the conclusion. The new periodical press gave voice to observers and physicians in many parts of the world, observing and doctoring different diseases, the manifestations and physical and cultural environments of which were in most cases very different. Because the consensus existing on how epidemics should be understood (explained, prevented, alleviated, etc.) was insignificant, local conditions and theorising about local conditions prevailed in the description of the fevers.[268] This was quite apparent to commentators in London:

> Another source of error and of difficulty respecting a knowledge of the aetiology of yellow fever, arose from *the situation in which those were placed who described its phenomena.* There were few from among the numerous writers upon the subject, who derived their knowledge from personal observation in both hemispheres. The experience of many was confined to its appearance in one country, and during the prevalence of a single epidemy.[269]

Error and difficulty were the results of accident of situation because, in the first place, one's assessment of the facts did not, as a matter of course, benefit from a second opinion (correspondents such as Dr Maclean at the Instanbul Pest Hospital and — even more so — ship surgeons had little opportunity to share actual experiences with colleagues). Secondly, the correspondent engaged in dispute would stand firmly by his version of the evidence, for any evidence that was presented in support of a contrary view could either be dismissed or discounted on the grounds that it was the product of very different environmental/observational circumstances. (Obviously, epidemics were not observed in laboratories. When a town was affected, the conscientious physician would take to the streets knocking on doors, he would visit the port and markets, look for stagnant water in the vicinity, take notes on the weather, etc.) Thus peculiarities of 'situation' in the absence of consensus-inspiring team work and efficient channels of

[267]Chevrin REVIEW (1831), p. 366.

[268]Gooding would describe this situation as one in which the 'construals' of observers and physicians never made a successful ascent from their concrete, chaotic, and particular world to become 'publically observable phenomena, or facts' (*Experiment and the making of meaning*, p. 66). Gooding's views are discussed in Part 2.2.

[269]Jackson REVIEW (1822), pp. 21-22, emphasis added.

communication sustained diversity of opinion and uncompromising and frustrating confrontation in the periodical press.[270]

Another source of frustration was the general absence of what today we would call 'standards' or 'conventions' of good practice. For example, not only were conventions for the *presentation* of evidence few in the early decades of medical journalism, a conventional understanding of what counted *as* evidence was largely missing too. It would be a while before canons of evidence crystallised and became internalised in medical practice.[271] Contagionists, non-contagionists, and the rest could publish practically any 'fact' which they regarded as supportive of their views. The anti-contagionist Dr Larkin implied as much when he berated an opponent:

> Dr Mead tells us that in the year 1726, an English ship took in goods at Grand Cairo, whilst the plague was raging there, and carried them to Alexandria. Upon opening one of these bales in a field, two Turks employed in the work were immediately killed; and some birds, which happened to fly over the place, dropped down dead! *Credat judœus!* These are the *facts* upon which the doctrine of contagion is built![272]

Yet what Larkin himself believed were miasmatic (non-contagionist) facts undoubtedly produced incredulity in other quarters. One man's evidence was in the opinion of the next man a self-serving speculation. Dr Miller, another anti-contagionist, exclaimed:

[270]On the extent of disagreement, see also Royston (1808), p. 34; Hosack (1816), p. 353; Smith and Tweedie REVIEW (1830), pp. 234-235; Smith REVIEW (1830), pp. 232-233; Chevrin REVIEW (1831), pp. 365 and 380; Cholera NOTICE (1832), *passim*; Fergusson (1832), p. 86; Chevrin REVIEW (1833), p. 397; and Cholera REVIEW (1849), p. 201. On expressions of dismay over the state of disagreement, see Dancer (1805), p. 385; Caldwell (1807), pp. 111 and 125; Burnett (1816), p. 464; Bancroft REVIEW (1817), p. 401; Aiton REVIEW (1832), pp. 487-488; and O'Shaughnessy REVIEW (1832), p. 389.

[271]On attempts later in the century to introduce to medicine standards and conventions that mimicked those of the 'sciences', see Lawrence, 'Incommunicable knowledge'; and Warner, 'Ideals of science and their discontents', especially pp. 454-455. On the lack of standards and conventions in the sciences, see Kuhn on optics before Newton: 'Being able to take no common body of belief for granted, each writer on physical optics felt forced to build his field anew from its foundations ... there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. Under these circumstances, the dialogue of the resulting books was often directed as much to the members of other schools as it was to nature' (*The structure of scientific revolutions*, p. 13; also pp. 14, 17, and 47-48). See also, idem, *The essential tension*, p. 261. On the presuppositions of organised 'textual communities' more generally, see Stock, *The implications of literacy*.

[272]Larkin (1825), p. 268; see also pp. 277 and 279.

> In order to explain the scattered, remote, and unconnected occurrence of cases, the advocates of contagion are obliged to resort to the extravagant supposition of the contagion being diffused through an extensive range of atmosphere, or, to use their own singular phrase, of an *inoculation of the atmosphere* by the effluvia of the sick![273]

Besides the exclusivity of facts reported to the personal experience of the reporter,[274] another problem, as some recognised, was that both 'contagionist' and 'miasmatic' views on disease propagation, and all compromises, variations, qualifications, and alternatives to basic expressions of those views, postulated *things* ('agents of contagion', 'animalculae'), *states* ('corrupt atmosphere'), and other goings-on ('miasmatic influences', 'predisposing causes'), that had not the slightest chance of being demonstrated true or false by means of the available technology and organisation of the medical community at the time. As Dr Bancroft noted in a rare effort to rise above the disputing sides:

> Contagion and marsh miasmata being alike *imperceptible by the senses*, it must have been impossible for Mr Mackenzie to know, that a single particle of febrile contagion then existed at Grenada, or that an abundance of what are called marsh miasmata, did not exist there.[275]

Evidence was not only doubtful, doubtable, or unconstrained by conventions. In the eyes of some, the evidence required to make a view convincing was often simply unattainable or beyond comprehension. Whereas, for example, corruption of the atmosphere was said by non-contagionists to be a result of putrefying matter, subterranean exhalations, heat acting on marshes, etc., the origin of the supposed contagions was left unaccounted for by contagionists. As one editor remarked, 'the origins of contagious diseases have been commonly deemed so obscure, that any attempt to elucidate the subject would appear more curious than useful'.[276]

[273]Miller (1807), p. 283.

[274]For further examples of the intensely personal flavour of many of the accounts, see Dancer (1805), p. 387; Thomas (1805), *passim*; Noble (1806), *passim*; Chisholm REVIEW (1807), p. 130; Burnett (1816), p. 444; Calvert (1816), pp. 326-327; Doughty REVIEW (1816), pp. 142-143; Pym (1816), p. 209; Bancroft REVIEW (1817), p. 413; Dickson (1817), pp. 36 f.; Fergusson (1817), p. 149; Veitch REVIEW (1818), p. 495; Maclean (1819), p. 218; Townsend REVIEW (1824), p. 348; Christie REVIEW (1829), p. 166; Observer (1830), *passim*; Chevrin (1831), *passim*; Anonymous (1832), *passim*; Webster REVIEW (1832), *passim*; Latham (1835), *passim*; Fergusson (1838), *passim*; Simpson (1838), pp. 358 ff. ;

[275]Bancroft (1814), p. 349.

[276]Blackburne REVIEW (1803), p. 461.

Even the champion anti-contagionist Sir Gilbert Blane — together with his sympathisers he had been accused by one contagionist to favour 'occult miasms'[277] — did not seek to hide the fact that there were obvious limits to how much *his* suppositions could explain: in the miasmatic process 'there is a subtle, *incomprehensible impression* made on the living human body by marshy exhalations'.[278] Beyond this he would not venture to speculate. Dr Paterson, in 1838, was similarly restrained when he wrote that, 'of the fact, that a peculiar atmospheric influence had much to do with the [cholera] epidemic, I am fully satisfied, although in regard to its mode of action and its peculiar nature I can say nothing'.[279]

In the normal course of events, however, participants were not so restrained. They insisted that what they considered evidence provided strong support for their interpretations and postulates. One new 'theory' (and associated cure) followed closely upon another: the hydrogen theory of epidemic fever, the oxygen theory of the same, the cold water therapy for yellow fever, the theory that 'cold' was the cause of disease, and many more.[280] The diversity did not stop 'laws' being proclaimed, though law-makers could have had little reason to believe they would be taken seriously or even understood:

> As a general law, the violence of febrile efforts, whether sthenic or asthenic, will be in the direct ratio of the purity of the blood, influenced by the intensity of what are termed the febrile causes applied to the constitution, as well as by the length of time of their previous application.[281]

In brief, most evidence relating to diseases and their mode of propagation was considered unreliable because shared criteria for its reliability were practically non-existent. So were criteria relating to its use in reasoned published argument. 'It gives us pain', Dr Miller wrote from New York, 'to see such respectable physicians precipitately rushing into conclusions [about yellow fever] altogether unwarranted by the premises'.[282] Behind Miller's invective there is an allusion to a state of uncertainty that was all too evident in the periodical press. Uncertainty was at the core of medical evidence in the fever debates.

[277]Fergusson (1817), p. 129.

[278]Blane (1816), p. 26, emphasis added.

[279]Paterson (1838), p. 412.

[280]Respectively: Blackburne REVIEW (1803), p. 462; Domeier (1805), *passim*; O'Leary (1806), *passim*; and Clendinning (1832), *passim*.

[281]Blake (1832), p. 457. On the disputability of evidence, see Selden and Whitehead (1803), p. 270; Domeier (1805), pp. 106-109; Miller (1807), p. 284; and Dickinson (1817), p. 464.

[282]Miller (1803), p. 104.

Related notions were similarly infected. The anonymous 'Inquirer' writing for the *Edinburgh medical and surgical journal* alleged that many participants were 'totally unmindful of what is generally understood by [medical] *explanation*'. In his view, 'one fact is explained by classing it along with other facts of the same nature'.[283] Another commentator pressed similar advice elsewhere:

> even when allowing all [Dr Neale's] premises to be correct, they do not warrant the inferences he would draw therefrom. We wish we could impress upon the minds and memories of all speculative writers that the third term of a syllogism can never *con*clude more than the major proposition can fairly *in*clude.[284]

Because agreement over proper experimental procedure became possible only in the late nineteenth century, what few experiments were attempted in the early decades had limited appeal.[285] Reasoning about cause and effect was itself problematic. Dr Dickson suggested that:

> the difficulty of elucidating the nature of fever in general, is increased by *confounding cause and effect*; by not distinguishing between symptoms as the consequence of morbid actions, and those actions themselves; and again, by not attempting ... to rise from the latter to the proximate cause or essence of the disease.[286]

In sum, a great many conventions relating to formal argument and conduct in the investigation of disease, which we take for granted today, were missing from the first phase of periodical communication.[287]

[283]'Inquirer' (1805), p. 427.

[284]Neale REVIEW (1832), p. 162. The advice by Dr James Simpson is also telling: 'In arguing ... that every person who is subjected to the contagion [of cholera] ought, if the disease were actually contagious, uniformly to suffer an attack of it ... it seems to have been very generally forgotten, that to insure in any instance this invariability of antecedence and consequence among physical, metaphysical, or vital phenomena, the circumstances under which the causes are applied must be at all times, in all necessary points, perfectly similar' (Simpson [1838], p. 403).

[285]See, for example, Guyton-Morveau REVIEW (1802), p. 188; Dancer (1805), p. 388; Ffirth (1805), *passim*; and Cholera REVIEW (1831), p. 534.

[286]Dickson (1808), pp. 457-458, emphasis added.

[287]For further examples of problems arising from the scarcity of shared conventions on reasoning, see Hall (1802), p. 451; Harris (1803), pp. 26 and 28; Patterson (1803), pp. 107 and 109; Ryan (1803), p. 217; Bennion (1805), p. 138; Dancer (1805), pp. 385-388; Chisholm REVIEW (1807), pp. 128-129; and Miller (1807), p. 289;

1.4.4

By now some of the context of the language which suggested belief in a dualist disjunction will be clearer. Because epidemics were not reproducible in laboratories, because they gave rise to many different fevers, because they were observed in radically different environments around the world, because individual experience of diseases was generally confined to local manifestations, because communication, institutional research, and cooperation among physicians were poor, because notions of evidence, its reliability, and conventions relating to its presentation were not widely shared, and because reasoning in relation to disease matters was much disputed, progress towards consensus on the epidemic front during the first fifty years or so of medical journalism was minimal. As the quoted remarks of participants have indicated, the impasse proved very frustrating for correspondents and editors, especially for editors, who had expected that the elevation of medicine into serial format would bootstrap their science up to the plane of physics and chemistry.

Attacks on language were just *one* consequence of such frustration. Out of context they take on the appearance of quasi-philosophical statements drawing attention to a problematic disjunction of language and world. In context the attacks on language team with outbursts of frustration against a variety of targets. To interpret these reactions as expressing a metaphysics, or as furnishing evidence for a metaphysics, would be as implausible as to interpret the following editorial comment as a proposed philosophical dissociation of 'experience' and 'judgement':

> We may observe [on the causes of yellow fever], that those who ... form their opinion from personal observation, scarcely ever alter it. We, however, who have no experience in this instance to appeal to, — who only know the disease from the writings of others, — and who have not regulated our practice by our theory, and can, therefore, renounce the one, without renouncing the other, — we are less steady in our opinions, — we are perplexed by opposite experiences, and discordant statements; — we suspend our judgement on account of the suppression of circumstances, which, if detailed, would perhaps have no effect in directing it ... *The actual observers all seem to become parties in the cause; we, who never saw it, pretend to judge.*[288]

With this editorial the author is quite clearly reprimanding the unscrupulous among his audience. He is using irony to say that he and like-minded participants in the fever debate would like to see an end to irresponsible, selfish behaviour. *He is not*, needless to say, calling on correspondents to give up 'experience' in favour of 'judgement', nor is he advocating the metaphysical thesis that the two are in reality disjointed. He singles out *experience*, but, as earlier considerations show, he could just as well have

[288]Doughty REVIEW (1817), p. 239, emphasis added.

directed his invective at particular people, situations, evidence, reasoning, logic, and (of course) language, instead.

Problems of language in the investigations of epidemic fever in the early nineteenth century involved a struggle to find 'the right words', the right way of expressing what seemed to be going on.[289] But problems were not confined to language: almost *everything* about those investigations was problem-ridden (the range of cultural resources needed to make the desert bloom had yet to be developed and consolidated). Language was maligned as an instrument of obscurantism, but for the most part it was the language of *others* that was renounced.[290] There is no evidence of a genuine belief that language was somehow *in the way* of something else that was the true object of inquiry, nor is there any indication that a metaphysical disentanglement was being attempted of language from world. Thus there is no reason to believe that a philosophical problem of reference or representation was being implicitly addressed. The language/world dissociations in the writings of nineteenth-century physicians were rhetorical, and should be analysed accordingly.

[289]See, for example, Pym (1816), p. 186; Bancroft REVIEW (1817), p. 405; Dickinson (1817), pp. 462 f.; Dickson (1817), p. 46; Doughty REVIEW (1817), p. 240; Thomas (1817), *passim*; Yellow Fever NOTICE (1817), pp. 49-50; Fever REVIEW (1818), p. 538; Plague REVIEW (1820), *passim*; Hutchinson (1821), pp. xxxvii-xxxviii; Yellow Fever REVIEW (1822), p. 20; Johnson (1823), p. 377; Aiton REVIEW (1832), p. 488; and Venables REVIEW (1832), *passim*. On the immense difficulties that 'early' languages of science presented, consider Williams writing about Faraday: 'Why was it that his ideas were not recognised, his theories not taken seriously and his vision of field theory not developed until after his death? One part of the answer ... is that Faraday's writings are extremely difficult to understand. Until the 1840's he successfully hid his metaphysical assumptions from his audience and tried to present both his results and his theories in language that would not require metaphysical explanations. Failure to understand Faraday was ... due to Faraday's own obscurity of exposition' ('Should philosophers be allowed to write history?', p. 250).

[290]It was realised, of course, that obscurantism was as much the fault of people as language. Reginald Orton, who in 1832 expressed the opinion that 'the proximate cause of cholera consists in a diminution of the energy of the nervous system', was praised by a reviewer for the *London medical and physical journal* for avoiding 'a mere verbal dispute about the propriety of the term proximate cause': in the reviewer's opinion all that Orton intended by the term was 'to express the essence of the disease and the immediate origin of its symptoms' (Orton REVIEW [1832], p. 47). Verbal disputes were almost *fashionable* in pre-consensual science.

1.4.5

Philosophers of science have had little to say about 'problems of language' in the history of science. The problems range from the abstract and general ('do changes in technologies of communication affect what is regarded as scientific knowledge?', 'does language impose constraints on scientific thought and action?', 'are scientific conventions propagated and reinforced through language?'), to the relatively concrete and particular ('what rhetorical forms characterise scientific argument?', 'how is expression given to an entirely novel observation?', 'what is the role of language in exploratory experimentation?').

Yet a small number of philosophers do seem to have acknowledged that language is a powerful force in science, and that its history and role in scientific inquiry are relevant to the discussion of philosophical issues. Kuhn, who distinguished between two periods of science (the normal and the revolutionary), has suggested that the scientific language from the two periods can be differently characterised.[291] Rorty, expounding Kuhn's views on this matter, distinguishes between 'normal discourse' which is conducted 'within an agreed-upon set of conventions about what counts as a relevant contribution', and 'abnormal discourse' which is 'what happens when someone joins in the discourse who is ignorant of these conventions or who sets them aside'.[292]

Normal science, accordingly, enables and is characterised by normal discourse. By implication a philosopher must be sensitive to the distinction between the two kinds of language because the 'content' of each (in the one case associated with conventions, in the other with confusion about conventions) will be regarded and treated differently in the scientific community (for example, language recognised as a part of normal discourse is more likely to carry legitimacy and conviction).[293] Unfortunately, this and other attempts to bring scientific language into focus have been half-hearted in philosophy, and none have involved a systematic exploration of the issues.[294]

[291]See, for example, Kuhn, 'Commensurability, comparability, communicability', pp. 715-716.

[292]Rorty, *Philosophy and the mirror of nature*, p. 320.

[293]Under normal conditions of research, 'a scientific community functions as a producer and validator of sound knowledge' (Kuhn, *Essential tension*, p. 298).

[294]I list here four examples of comments on language in its conventional, rhetorical, and cognitive associations in science, that have been made by philosophers but left unexplored:

(i) Popper: 'It is impossible to deny that mathematics uses discursive thought. Euclid's discourse moves through propositions and whole books step by step: it was not conceived in one single intuitive flash. Even if we admit, for the sake of the argument, the need for pure intuition in every single step without exception ... the stepwise, discursive, and logical procedure of Euclid's derivations is so unmistakeable, and it was so generally known and imitated' (*Objective knowledge*, p. 131). The interesting suggestion here is that we should see the articulation of Euclid's geometry as due largely to the possibilities opened up by his vocabulary and compositional method.

(ii) Ziman: 'the report of an experiment is a very long way ... from that direct ... wrestling with ... Nature that the individual research worker experiences in his own laboratory. When we say that scientific knowledge is ... firmly based on empirical evidence, we do not mean that each scientist has seen with his own eyes all ... in which he believes. We mean that there exist a collection of reports of observations made by reliable witnesses and set out according to certain conventions ... These reports ... give ... a carefully edited version of ... events ... By becoming part of the stock of public knowledge they have become second-hand information, far removed from the direct experience of any one of us' (*Public knowledge*, p. 35). Here we find the suggestion that if scientific knowledge is considered to be contained largely in the formal published language of science, that knowledge is a highly conventional end-result of scientific practice.

(iii) Ziman, again: 'I want to ... suggest that the absolute need to communicate one's findings, and to make them acceptable to other people, *determines their intellectual form* ... In other words, each individual scientist is to be seen as concerned mainly with putting forward his own ideas, trying to make discoveries for himself and therefore explicitly describing his thoughts and behaviour in essentially personal terms. But because he is, indeed, a member of the scientific community, because he is bound to communicate his ideas and make them public, he unconsciously makes allowances for the rational behaviour of others, and learns to put himself in their place' (ibid., pp. 144-147, my emphasis). This rather solipsistic view of the scientist nonetheless contains the interesting suggestion that a (public) language affects what in the end a scientist says. Ziman here shows that he subscribes to the dualist conception of language.

(iv) Finally, Feyerabend: 'How does [Galileo] manage to introduce absurd and counterintuitive assertions, such as the assertion that the earth moves, and yet get a just and attentive hearing? ... arguments will not suffice ... and Galileo's utterances are indeed arguments in appearance only. For Galileo uses propaganda. He uses psychological tricks in addition to whatever intellectual reasons he has to offer. These tricks are very successful: they lead him to victory. But ... they obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been *invented*. They obscure this fact by insinuating that the new results which emerge are known and conceded by all ... In the circumstances we are considering now, propaganda is of the essence. It is of the essence because interest must be created at a time when the usual methodological prescriptions have no point of attack; and because this interest must be maintained, perhaps for centuries, before new reasons arrive' (*Against method*, p. 67 and p. 123). We find here the suggestion that an apparently rational and measured language may itself be a form of propaganda, disguising an actual dearth of conventionally 'good' reasons or reasoning. Language is not, according to Feyerabend, merely the carrier of content, it is also an instrument of conviction in its own right.

Historians of science, by contrast, have more often alluded to the powers and significance of language, although only in the last decade has the role of language in scientific practice and culture been systematically explored by historians. An early allusion is found in the final words of Owen Hannaway's historiographically path-breaking book *The chemists and the word* (1975):

> Against the background of the C16 textbook origins of chemistry, Lavoisier's *Traité élémentaire de chimie* acquires a fresh significance. In this text, designed specifically to win over the minds of youth to the new chemistry, Lavoisier employs the pedagogical and linguistic theories of the Abbé de Condillac to systematise anew the science. It is through a new theory of language — one that does not simply permit words to discriminate chemical species from one another but which gives words the power to penetrate the substance of chemical entities and to analyse them — that Lavoisier would write another chapter in the story of chemists and the word.[295]

Another allusion can be found in an article on late nineteenth-century American medicine by the historian John Harley Warner:

> Physicians took up science as an ideal before it offered much to help them allay the ills of the sick. The American medical profession chose to march under the banner of science for a variety of reasons that had little to do with its clinical application. An ideal of science was brought to medical dominance ... because it furthered the career goals of medical academics, provided the medical elite with *a culturally compelling language* that was a powerful tool in professionalization ... [etc.].[296]

A particular ideal language, Warner suggests, assisted in the achievement of a desired social organisation (could the adoption of a 'normal discourse', then, enable the emergence of a normal science? — this possibility goes far beyond the Kuhn/Rorty thesis).

The historian Roy Porter, to take one final example, hints that language carries various messages, and in a medical context — where issues of power in doctor/patient relationships, and political ideology in matters of public health, have always had a defining influence on what medical knowledge is knowledge 'of' — language can be analysed at various levels:

[295]Hannaway, *The chemists and the word: the didactic origins of chemistry*, p. 156; cf. pp. 59-61, 107-108, and 152. Cf. also Christie and Golinski: Lavoisier's politically most effective move was his decision 'to embody the new chemistry in an elementary textbook. Within a year it had been hurriedly translated into English ... In presentation, the text's most novel and striking aspect was its wholesale deployment of the new nomenclature formulated by Lavoisier and his colleagues. As opponents ... were not slow to realise, in learning the new words the student was learning equally, though less consciously, to reject the old concepts and theories' ('The spreading of the word', p. 259).

[296]Warner, 'Ideals of science and their discontents', pp. 454-555, emphasis added.

> Medical terminology affords a good instance of the multiple functions which language has to fulfil. It is a technical, esoteric jargon, yet it must also serve to communicate (or, sometimes, 'discommunicate') between doctor and patient, and enable the latter to make sense of sickness. It is a neutral, objective expression of scientific knowledge, while at the same time also entangled in socio-commercial transactions and therapeutic aspirations.[297]

'Theories of language', 'powers of words', a 'culturally compelling language', and a language 'entangled in therapeutic aspirations' — these are not factors commonly encountered in the philosophy of science. It has been up to medievalists and historians of early modern science and medicine to make the case that these factors and the metaphysical debates that surrounded them have had a formative influence on the science we are now familiar with. Their suggestions have yet to cause ripples in philosophy, while the elaboration of these suggestions by historians of science needs to be accelerated.[298]

In this final section of Part 1 I have looked at an historical period of 'abnormal discourse' that seems to exemplify a disjunction of 'language' and 'world'. Properly interpreted, however, the apparent evidence for a disjunction is really evidence for the altogether different condition of a science in crisis. Also, the case study gives us an indication of genuine *practical* problems of language and their equal ranking alongside

[297]Porter, 'Expressing yourself ill', p. 276. See idem, 'The language of quackery in England', and *Health for sale*. For other historical works touching on language and communication, see Crosland, *Historical studies in the language of chemistry*, chs. 2-4; McLuhan, *The Gutenberg galaxy*, pp. 86 f.; Pinch, 'What does a proof do if it does not prove?'; and articles in Burke and Porter (edd.), *Language, self and society*, and in Benjamin et al. (edd.), *The figural and the literal*.

[298]For a discussion of these works, see Part 2.1. On the importance of theories of language to the history of science, consider Carter on eighteenth-century botany: 'A profounder distinction between botany and exploration now emerges. For the difference between the two was not simply a matter of methodology: it embodied, more fundamentally, a disagreement about the nature of language, and its relationship to the world. For [Joseph] Banks, names enjoyed a simple, Linnean relationship with the object they denoted. They gave the illusion of knowing under the guise of naming. [Captain] Cook's names obey a different, more oblique logic, the logic of metaphor. His names do not intend to preserve the delusion of objectivity, for his standpoint is neither neutral nor static. Instead, they draw geographical objects into the space of his passage' (*The road to Botany Bay*, p. 29). On language and geographical exploration: 'Thus the marine surveyor John Lort Stokes names "Mount Inspection, a hill 105 feet high, and the most remarkable feature hereabouts" ... The early travellers [in Australia], then, invented places, rather than found them. This was what naming meant. To designate a place as "mount" might express, in fact, the absence of that desirable feature' (ibid., pp. 50-51).

other scientific resources that are fixed by convention. We can assume that when a branch of science is afflicted by pernicious problems of language, the consensus that exists about observations, techniques, method, interpretation, etc., will be quite limited. Conversely, the absence of problems of language should serve as an indication of a relatively 'normal science', busily reaping the benefits of 'normal discourse'.[299]

[299]In his 'Postscript' to *The structure of scientific revolutions*, Kuhn writes that although the initial stages of extra-ordinary science give rise to problems that first become evident in communication, such problems 'are not merely linguistic, and they cannot be resolved simply by stipulating the definitions of troublesome terms. Because the words about which difficulties cluster have been learned in part from direct application to exemplars, the participants in a communication breakdown ... cannot resort to a neutral language which both use in the same way ... Part of the difference is prior to the application of the languages in which it is nevertheless reflected' (p. 201). (Of course, I do not agree with the dualist view expressed here by Kuhn, that 'the languages' in this context merely reflect *prior* differences.)

PART 2

THE CONSTRUCTIVIST CONCEPTION OF LANGUAGE AND ITS FUNCTIONS IN SCIENCE

> If one takes your words literally, one would assert that reality is created through language and that therefore the primitive physicist and the quantum physicist live in different realities.[1] (Schlick)

In Part 1 I sought to demonstrate that philosophers of science have presupposed the dualist conception of language. They have paid little attention to what language does in science besides describe or represent. Their common assumption has been that language is in reality dissociated from those things to which it is directed, viz. appearances, observations, physical phenomena, natural facts, etc. These exist independently of language, but require formulation and expression in order to be brought within the realm of human knowledge. All that language does of philosophical importance is to put the aforementioned extra-linguistic findings into words (English words, German words, 'observational terms', 'theoretical terms', 'statements', or whatever).

It is tempting to question the soundness of the dualist conception of language by pointing to unclarities in its various formulations and uses — unclarities which few philosophers have sought to address. However, this strategy is not sufficient to eliminate dualism: a plausible *non-dualistic* philosophical account of language and its functions in science must first be found. Such an account would need to show, first, that language is an integral part of the activities that produce scientific conceptions of the world, and that the resulting physical world that scientists apprehend *is inalienably a part-linguistic construction*; and, second, that the new account cannot be reduced to the fundamental elements of language/world dualism. 'Constructivism' seems an appropriate name for the conception to replace dualism.

To develop a constructivist account of language it is not necessary to start from scratch. Evidence for a non-dualist conception has been available in print for over a decade. Primarily it is to be found in historical and sociological works on science whose orientation is philosophical. In most works of this kind the issue I have identified as the language/world disjunction has not been recognised as an issue — indeed, the expression of dualistic views is common to these very works. Nevertheless, it is also possible to find in them the building blocks for a constructivist conception of language.

[1]From a letter to Carnap, quoted in Coffa, *The semantic tradition*, p. 373.

Isolating, clarifying, and consolidating this material into an argument for constructivism is the primary task of Part 2. A related task is to show that what language in science achieves is far more complex than what the dualist model suggests. Language doesn't just *express* observations, natural phenomena and facts, scientific conceptions of the world, etc. More than a mere tool for their representation and communication, language contributes to their creation and underpins their subsequent existence.

2.1 Scientific language in history and the laboratory: matters of fact and 'out-thereness'

Towards the end of Part 1 I discussed a period in history during which the language of disease had become the object of general and vocal dissatisfaction. Cooperation on medical issues was extremely difficult as a result. From our own familiarity with the ways of twentieth-century science it is easy to see why this was so. Elizabeth Eisenstein expresses a contemporary perspective when in *The printing press as an agent of change* she writes that:

> all work in science is incomplete until the report has been written *and* presented in published form. The irreducible 'fact', the direct observation and any kind of raw data must be processed by being written down and made available for checking and confirmation by other eyes.[2]

'Direct observation' and 'raw data' aside, Eisenstein is undoubtedly right in saying that contributions to scientific knowledge are validated in part through publication — the process of publication exposes contributions to the scrutiny of the scientific community. Scrutiny of this kind was not always possible. Early nineteenth-century uncertainties about the language of disease (and disease itself) meant that communal scrutiny was not possible then. Several hundred articles on the subject were published in Britain in the opening decades of the century (I have listed a selection of these in part B of the Bibliography), but shared ways of interpreting and evaluating them were practically non-existent. Shared ways of *writing* articles to solicit or produce agreement were equally non-existent. These ways took time to evolve. The evolving journals of medicine were nevertheless fortunate in one respect. By the early nineteenth century it was generally accepted that scientific knowledge could not be validated in the privacy of one's own thoughts and actions. It had to pass the test of being made public.

This test was recognised as necessary even when the means for passing it (reliable witnesses, reliable language, conceptual infrastructure, journals, etc.) were unavailable. Only a century and a half earlier the principle of public validation was still under-

[2]Eisenstein, *The printing press as an agent of change*, p. 477.

developed in its application. At the time, in the second half of the seventeenth century, Robert Boyle was pioneering the argument that public scrutiny was important and that a certain style of writing, which he adapted from Michel de Montaigne, was suited to the task of putting the principle to work. He would face an uphill struggle convincing others. Boyle was nevertheless fortunate in one respect. He had access to a printing press. Just over a century and a half earlier there was no such thing. *Its* invention, along with the literary innovations and transformations of natural philosophy that followed it as consequences, are well described in Eisenstein's book.[3]

2.1.1

The now evident truth that work in science is incomplete until it is printed in a public domain that can comprehend and evaluate it conceals a history of linguistic innovation in science. Details of this history have appeared in recent years in the work of a growing number of historians of science. Their work identifies the conventional nature of linguistic practices that have characterised modern science. It suggests that at all times the production of scientific knowledge has depended upon the acceptance of certain social and discursive conventions. These, the historians show, have never been self-evident; rather, they have been introduced following heated debate over alternative, conflicting conventions, and the influence of key players. Steven Shapin's writings on Boyle's 'literary technology' and the early history of experimental science are representative of this new school of historians.[4] The relevance of their work to the argument against dualism hinges on a simple consideration: certain functions of language (specified below) can be shown to be incompatible with (and not reducible to) the elements of the dualist conception. If these functions are proper to science, dualism must be abandoned.

I should like to consider very briefly the following imaginary situation. It is a country with a building profession plagued by different sets of *units of measurement*. The situation is the cause of considerable frustration, with different guilds adhering stubbornly and proudly to the measuring traditions to which their members were apprenticed. Because builders sharing common measures are few, the building profession as a whole lacks a sense of community. There is little by way of social solidarity. This has meant permanent disagreement about how to settle the many disputes that arise at building sites, supply stores, and elsewhere where traditions clash. Manuals translating between sets of units have found little use in the face of deep-seated chauvinism. Now, however, a figure appears on the scene who by force

[3]See, for example, ibid., chs. 6 and 7. Cf. McLuhan, *The Gutenberg galaxy*, pp. 86 f.

[4]See Shapin, 'Pump and circumstance', and other works referred to below.

of character, rhetoric, idealism, political machinations, wealth, and so on, manages to bring most builders to accept a system of conventions devised by him alone, complete with an Institute of Weights and Measures where prototypes of the new units are held. Common standards now in place, divisiveness soon recedes into the past and is forgotten. Disputes do arise occasionally, but now the proper manner of their resolution — the proper way of generating assent to what is or is not the case — is apparent to all (recourse to the Institute in the extreme case).

At the risk of oversimplification, Shapin may be read as saying that the forging of the early community of *experimental scientists* came about in much the same way as the forging of the community of builders in my imaginary story. From a philosophical point of view, the crucial difference in the scientists' case is that the unifying convention determined not something as prosaic and arbitrary as a set of units of measurement, but *what were to count as matters of fact.* Matters of fact which defined and consolidated the early community of experimentalists were, in an important sense, *artefacts*, according to Shapin. That is to say, certain things, and a certain *category* of things, which many philosophers would say were 'fixed' by discovery, are said by Shapin to have been fixed by convention.

In his seminal 1984 article on Robert Boyle's 'literary technology', Shapin identifies a distinction reminiscent of the language/world disjunction central to dualism. He says that it is common for the production of knowledge and the communication of knowledge to be regarded as distinct activities.[5] The parallel with dualism rests in the conceptual separation of a set of linguistic activities from a set of practical, non-linguistic activities. Shapin wishes to argue against the separation, and specifically to argue that 'speech about natural reality' (as he calls it), far from being distinguishable from the knowledge it conveys, became (in the latter half of the seventeenth century) a means of *generating* knowledge. It also secured *assent* to knowledge and bounded it off from knowledge of lesser certainty.[6]

Philosophical implications render Shapin's largely historical argument of considerable interest. It runs as follows.[7] The focus is Boyle and his experiments in pneumatics in

[5]The idea that scientists experiment (produce knowledge) and then by way of language communicate knowledge gained thereby, is present in the works of Popper, Kuhn, Quine, and Lakatos, among others.

[6]Shapin, 'Pump and circumstance', p. 481.

[7]I provide no more than an outline of the argument that spans the lengthy article cited in the previous note. A considerably more detailed version of the argument may be found in Shapin and Schaffer,

the late 1650s and early sixties. At the time there was little agreement about what should count as *authentic* knowledge and how it should be distinguished from mere belief and opinion. Of course, even today, responses to this issue are not entirely self-evident and unproblematic. But for the most part the issue arises today only in philosophy and is of no concern to scientists. In the 1660s the situation was very different. The issue of authenticity was as much practical as it was philosophical. 'Technology' (material, technical, linguistic, social) for producing knowledge had to be developed and defended against attack from many sides.[8]

Boyle sought to found science (natural philosophy) on the experimental *matter of fact.*[9] His object was to make this the item of knowledge that would secure universal assent, and about which it would be legitimate to be 'morally certain'. He proposed that matters of fact be generated by a multiplication of *witnesses* of experimental trials. Nothing witnessed by one person alone would count as a matter of fact. Results of experimental trials had to be constituted as matters of fact by *extension of the witnessing experience*, in principle to all men.[10] Witnessing provided experiments with a public domain. It established matters of fact as worthwhile products of experimental work. In Boyle's texts witnesses were named, the status of their testimony regarded as proportional to their ranks in society, with the testimony of nobility especially highly prized.[11]

Boyle considered the multiplication of witnesses as fundamental for the 'fixing' of matters of fact. He invented three interlinked technologies to produce them: a material technology (primarily the air-pump), a literary technology (a set of linguistic/textual techniques for multiplying witnesses), and a social technology (conventions to determine proper manners in dispute). Together the technologies were to create, extend, and validate experience. They were to achieve the appearance of matters of fact

Leviathan and the air-pump. For a philosophical commentary on this work see Hacking, 'Artificial phenomena'.

[8]In Part 1.4 I argued that in the early nineteenth century what was to count *as* medical evidence in relation to epidemic disease was indeterminable in the absence of conventions. Using Shapin's terms, I would have said that the *technology* of evidence, linguistic and material, was underdeveloped at the time.

[9]For a detailed example of what was, and what was *not*, to count as a legitimate experimental matter of fact see Shapin and Schaffer, *Leviathan and the air-pump*, pp. 42-46.

[10]On the importance of witnessing in the early Royal Society and on the problem of the credibility of witnesses see Shapin, 'The house of experiment'; and idem, 'The mind is its own place', pp. 201-202.

[11]Schaffer, 'Making certain', p. 146.

as items given by Nature, independent of the workings of human agency. The second, literary technology became vital for Boyle's programme partly as a result of a limitation of the air-pump: functioning air-pumps were difficult to build and very expensive. A full one decade after the instrument's invention only six or seven air-pumps existed in the whole of Europe. The material technology, therefore, posed a problem of access. Because Boyle believed that essential to the constitution of matters of fact was the assurance of people other than the experimenter, and because he believed that assurance depended on witnessing, and that witnessing should be a collective enterprise, a problem of access to the place of experiment was a problem for the production of matters of fact.

In response Boyle crafted the literature of 'virtual witnessing'. (David Gooding's alternative phrase 'vicarious witnessing' is suggestive of other features.[12]) This technology was to make Boyle's reports a substitute for direct experience. It was to fix matters of fact by producing for readers an image of the experimental scene that substituted for, and thus obviated the necessity of, direct witness. Simultaneously it was to be a technology of trust and assurance that the trial had been conducted in the way that Boyle (or any experimentalist) claimed it had been.

Drawings printed from etchings, expensive at the time, were used liberally in Boyle's texts. They conformed to naturalistic conventions of the time: particular existing air-pumps were pictured, shadows and all, not abstractions. Images of this kind were to allay distrust, to announce that the experiment really had been performed. The text which they supplemented was similarly crafted to convey maximum circumstantial detail and to give the impression of verisimilitude. It was, by Boyle's own admission, 'somewhat prolix'. But it was the *correct* way of experimental reporting, in his view, and he promoted it by laying down rules about how to write proper scientific prose. He encouraged against the use of mathematical language (it was not expansive enough as a linguistic style).[13] He indicated appropriate moral postures. He encouraged the reporting of *failed* experiments to secure readers' trust. He urged literary conventions for displaying modesty. He demanded that experimental philosophers be loath to engage in controversy (though he exemplified ways of decently engaging in controversy when that was unavoidable). Both appropriate writing and appropriate manners were necessary to distinguish matters of fact from theories, hypotheses, speculations, and the like, which for Boyle lay outside of the experimental programme.

[12]See Gooding, *Experiment and the making of meaning*, p. 167.

[13]See Shapin, 'Robert Boyle and mathematics', p. 42.

Boyle's linguistic conventions at one level were designed to draw a boundary between those proposed items of knowledge about which one could expect certainty and assent, and those about which one could expect uncertainty and divisiveness. But because the former category of certain knowledge did not in fact pre-exist (it was not clear *what* to expect), at another level Boyle's literary boundaries *decreed* the categories of disputable and indisputable knowledge. In diffusing his new integrated literary and material technologies Boyle came up against considerable difficulties (notably the arguments of Thomas Hobbes and Henry More). In spite of such resistance Shapin shows that many of Boyle's views prevailed and were endorsed by the Royal Society.

In sum, for Boyle, the problem of producing experimental knowledge was the problem of maintaining a certain form of discourse and a certain form of social solidarity. His matters of fact were thus essentially a social category — not a 'natural' one as historians of science have been inclined to believe.[14] A philosophically significant consequence of Shapin's argument is that matters of fact — forming the foundational category of experimental philosophy — began life as artefacts of communication — more precisely, artefacts of the social relations deemed necessary to sustain and enhance communication. More than a mere instrument of representation, language was a technology as essential to the new science as the air-pump itself. A solution to the question of what was the correct language and manner of natural-philosophical experimentation was, according to Shapin, simultaneously a solution to the question of what natural reality itself was like.

Shapin's account of the doings of early experimentalists is supported by a number of other works in the field which reveal social interests integral to late seventeenth-century models of proper knowledge. For example, Simon Schaffer writes that the literary figures of the Scientific Revolution were intensely aware of the fact that knowledge was commonly considered the product and cause of *dangerous dispute*.[15] Such dispute, it was widely accepted, could easily degenerate into full-blown civil war. Models of proper knowledge (such as the one defended by Boyle) were as a result tailored to knowledge production that was likely to guard against or avoid such dangers. (Thomas Sprat, historian and propagandist of the recently founded Royal Society, supporter of 'a close, naked, natural way of speaking' — and so of Boyle's literary technology — described experimenters' work as an *antidote* to interested dispute.[16] Hobbes, in

[14]On what historians have thought, see Shapin and Schaffer, *Leviathan and the air-pump*, pp. 5-14.

[15]See Schaffer, 'Making certain'.

[16]Sprat is quoted in Golinski, 'Language, discourse and science', p. 113. On Sprat see also Vickers, 'The Royal Society and English prose style', pp. 5-6.

opposition, argued that experimentation would itself lead directly to war.[17]) Schaffer believes that the epistemological debates of the period were of enormous political and social significance:

> At the Restoration memories of civil war were used to defend epistemologies which would prevent further strife. Experimental work was presented as a communal activity which could guarantee the peace of the community of workers, whose allegiance could be won to legitimate matters of fact.[18]

A cooperative approach to natural philosophy became enshrined in the corporate character of the Royal Society. Peter Dear — building on the Shapin/Schaffer analysis — has argued that in the late seventeenth century cooperative inquiry was a novel sort of cognitive enterprise for which it was necessary to define standards and criteria.[19] Although it was generally agreed upon at the time that the authority of the 'ancients' should be rejected, there was little agreement on what should replace it. Detailed references to Aristotle were no longer acceptable as premises for assertions. A new way of *supporting one's statements* was needed. The Royal Society quickly became that new source of authority, according to Dear, facilitating the expansion of natural knowledge through cooperation of the kind advocated by Boyle. Fellows of the Royal Society wishing to make a contribution to knowledge were required to conform to certain standards. Above all they had to report experiences of which they as observers had been part. Establishing the actuality of a discrete event, linking it to a particular place and time, was a prerequisite that delimited what could count as a contribution to knowledge.[20] New contributions would stand in contrast with scholastic practice, which sought to explain how the world behaved in general. Post-scholastic authority thus was to be conferred by a new *literary form.* A fundamental conclusion of Dear's argument is that revised discursive conventions and new meanings for authority and experience — defining elements of the character of modern science — were fashioned as ideologies and firmly integrated conceptually and in practice in the late seventeenth century.[21]

[17]See Schaffer, 'Making certain', p. 138; and p. 141.

[18]Ibid., p. 137.

[19]Dear, 'Rhetoric and authority in the early Royal Society'. See also, idem, 'Narratives, anecdotes, and experiments'.

[20]Dear demonstrates how Newton *fabricated* certain events he described in a paper sent to the Royal Society in order that the paper should conform to the Society's required standards and thereby carry the proper weight ('Rhetoric and authority in the early Royal Society', pp. 154-155). On this matter see also Bazerman, *Shaping written knowledge*, pp. 90 f. On the Royal Society's standards see Vickers, 'The Royal Society and English prose style', p. 55.

[21]Dear, 'Rhetoric and authority in the early Royal Society', p. 161.

Further support for this thesis comes from Charles Bazerman's recent study on the emergence of literary and social forms in early modern science.[22] Bazerman recounts Newton's efforts to cast his early prism experiments into a written rhetorical form consistent with that which he perceived was required for publication in the Royal Society's *Transactions*. His 'A new theory of light and colours', duly published in the *Transactions*, was not received as incontestable fact, however. Confident natural philosophers holding contrary beliefs (notably Robert Hooke) engaged Newton in a controversy that lasted four years (ending in 1676). Newton's distaste for controversy set him on a long search for an alternative literary form in which to publish his optical results and render them persuasive and compelling. He published nothing else of substance in the *Transactions* or any other journal.

Newton eventually found that his material — his temperament, his habits of thought and expression — were amenable to a tightly sequential form, which also constrained and constructed the reader's reasoning and experience at every step. He developed this into the comprehensive argumentative system of the *Optics* (published in 1704). This literary technology established his 'facts' as reliably reconstitutable phenomena for all to see, both perfecting and modifying principles enunciated earlier by Boyle (Newton studied and mastered Boyle's works).[23] He invented a way of arguing that culminated in the powerful *Principia* and provided a model for the form of scientific argument that proved immensely influential. The *Principia*, according to Bazerman, far from being the spontaneous product of a creative mind at work at understanding nature, was 'a hard-won literary achievement forged through some trying literary wars'.[24] Newton, in Bazerman's view,

> dominated the history of science not just because he discovered a few major laws, but because in finding a way to articulate those laws he found a powerful, long-lasting (though ultimately and necessarily temporary) solution to the problem of how one should talk about the subject.[25]

2.1.2

The studies by Shapin, Schaffer, Dear, and Bazerman argue for the conventional status of received ways of experiencing, conceiving, and writing about nature and natural knowledge. They provide histories of conventions through which the world has been seen and experimental knowledge produced. The body of work supporting their

[22]Bazerman, *Shaping written knowledge*, especially pp. 80-127.

[23]See ibid., p. 126.

[24]Ibid., p. 124.

[25]Ibid., p. 317. Cf. Myers, *Writing biology*, pp. 63-100.

conclusions has been growing steadily.[26] The consolidation of a community of experimentalists, cooperative research, modes of regular and legitimate discourse, authority in argument, authority of the printed word, agreement about facts, non-facts, experimental procedure, etc., all represent — in their received form — the victories of certain individuals, groups, and institutions over others. Their victories have become our heritage, while the failed alternatives are to be found, if at all, in history books. Boyle, as a conscious inventor and exploiter of the literary device he called the experimental essay, had a considerable impact on the development of Newton's preferred literary forms.[27] Boyle's air-pump experiments, even when considered independently of their effects on Newton, have provided — for over two centuries — a heuristic model for the production of authentic scientific knowledge.[28] 'To a very large extent', Shapin writes, 'we live in the conventional world of knowledge-production that Boyle and his colleagues amongst the experimental philosophers laboured to make safe, self-evident and solid'.[29]

[26]See, for example, articles in Benjamin et al. (edd.), *The figural and the literal*, including: Cantor, 'Weighing light' (on the connections between theories of language and theories of light in the eighteenth century); and Christie, 'Rhetoric and writing in early modern philosophy and science'. See also Dear, 'Narratives, anecdotes, and experiments'; Golinski, *Language, method and theory in British chemical discourse*; idem, 'Robert Boyle'; Holmes, 'Argument and narrative in scientific writing'; idem, 'Scientific writing and scientific discovery'; Hunt, 'Rigorous discipline'; Latour, *The pasteurization of France*; idem, 'Visualisation and cognition'; Naylor, 'Galileo's experimental discourse'; Nyhart, 'Writing zoologically'; Ophir and Shapin, 'The place of knowledge'; Paradis, 'Montaigne, Boyle, and the essay of experience'; Rosner, 'Eighteenth-century medical education'; Shapin, 'Social uses of science'; and Vickers, 'The Royal Society and English prose style'. On the contribution of certain rhetorical forms to the identity of the discipline of chemistry during the Enlightenment, see Anderson, *Between the library and the laboratory*. Related arguments are developed also in Barnes, *Scientific knowledge and sociological theory*; Bloor, *Wittgenstein*; and Mendelsohn, 'The social construction of scientific knowledge' (on p. 4 Mendelsohn writes: 'Science is an activity of human beings acting and interacting, thus a social activity. Its knowledge, its statements, its techniques have been created by human beings and developed, nurtured and shared among groups of human beings. Scientific knowledge is therefore fundamentally social knowledge'). For works of greater generality on the 'invention of languages' for philosophy and science see, respectively, Havelock, *Preface to Plato*, and Ong, *Ramus, method, and the decay of dialogue*.

[27]See Paradis, 'Montaigne, Boyle, and the essay of experience', p. 85.

[28]See Shapin and Schaffer, *Leviathan and the air-pump*, p. 4.

[29]Shapin, 'Pump and circumstance', p. 482. On 'self-evidence', see also Shapin and Schaffer, *Leviathan and the air-pump*, pp. 5 f.

Shapin and like-minded historians wish to maintain, of course, that there was nothing self-evident or inevitable about the series of historical judgements that resulted in a natural philosophical consensus in favour of the experimental programme. To believe that there was, they argue, is to neglect the historical nature of the gradually negotiated consensus, and its dependence on the action of particular individuals and particular forms of social organisation. I have overviewed their arguments here because they provide an unusual perspective on uses of language in science. This perspective is at odds with dualism. The disjunction of language and the world — at the core of dualism — means that the structure and content of the world (whether phenomenal or factual) is entirely independent of anything scientists say about it. Their words have no effect on what *is* in the world, beyond describing or representing its structure and contents (for 'what is' is logically distinct from and prior to the words). World A is quite simply a 'labelled' version of world B.

But if natural reality in the experimental tradition — natural facts, as well as established facts of a recognisably modern kind — is an invention with a history going back no further than the late seventeenth century (when conventions defining the experimental tradition were first agreed upon), the language/world disjunction may itself be an invention. For not only did conventions delimit practice, fix a style of writing and mode of social conduct, and bound off legitimate (including 'true' and 'false') scientific experience and conceptions from the rest, they also provided experimentalists with a literary technology which enabled them to remove traces of human agency from their reports, and to provide matters of fact with an appearance of objective existence (calculated to achieve universal assent). The impression experimentalists wanted to give, according to Shapin et al., was that they spoke the way they did because *that was what the world was like*. The belief that the roots of modern science were somehow moulded to 'the natural world' (phenomenal or factual) follows from mistaking a desired (rhetorical) effect for an actual cause. Natural reality and the physical world were made notions of early-modern objectifying conventions and the experimental tradition. The latter constrained and gave communal sense to apprehensions of the former.

Already we see the outline of a language/world relation radically different from the realist and anti-realist versions represented in Table 1 (see p. 18). This new outline has language firmly part of world A, but it is not a language that reaches out to a ready-made world B. Rather, within world A, language mixes with a scientific ideology that sets constraints on how world A is going to grow within the (infinite) space provided by the pre-conventionally chaotic world B. Until the constraints are set there is no ontological framework for advancing the scientific exploration of world B.

Incompatible though the position of Shapin et al. is with dualism, historians of Shapin's persuasion are not saying that scientific knowledge was ever *completely* determined by arbitrary measures relating to expression and proper behaviour. Hands-on experience with material technology was central to the experimental programme, in practice as well as in ideology, a fact that the historians I have been discussing always presuppose and often draw attention to.[30] Hands-on experience, observation, material events, experimentation, and the physical world are crucial constituents of science as we know it, even if they are subject to conceptual constraints that are historical products. To persuade someone — now or the seventeenth century — of something you must show them what you have found. Experience generated by Boyle's material technology was, as Boyle believed, not an empirical fact with scientific meaning until it was seen, identified, and given a particular meaning by others as well as by himself. Natural philosophers required conviction, and conviction could only be generated within a literary tradition.

The studies I have described so far contain arguments that invalidate the dualist conception of language — both as a claim about the disjunction of language and world, and as a claim about the distinguishability of linguistically rendered knowledge from physically/phenomenally (viz., pre-linguistically) rendered fact. The arguments make out that early experimentalists, whose practices, traditions, and worldview contemporary science has inherited in modified form, drew on high-level linguistic resources to build their definitions of physical reality (and the proper demonstration of physical reality). Scientific conceptions of the world — indeed any and all apprehensions of the physical world and its make up — and linguistic resources and actions which enabled those conceptions, were and remain at a fundamental level of analysis indistinguishably related.[31]

2.1.3

The argument against dualism derived above from historical perspectives on language in science is suggestive of how further arguments of this kind could be developed and

[30]Dear claims that 'the style of science espoused by the Fellows of the Royal Society was more important than the substance of that science' ('Rhetoric and authority in the early Royal Society', p. 159). Presumably Dear is referring to the early days. 'The substance' must have become more important, as what counted as substance was better understood, as the novelty of 'the style' wore off, and as the material technology became more varied and accessible.

[31]It should be noted that the studies I refer to *suggest* an argument against dualism *despite* the dualistic views of many of their authors (Bazerman, for example, is a language/world dualist, as I shall discuss later).

refined. However, conventions made current a long time ago may seem far removed from the facts of contemporary practice. Boyle's influence on seventeenth-century science was very different from Faraday's influence over a century later, and tells us little about what takes place in a scientific laboratory today, beyond the pursuit of matters of fact. The studies by Shapin et al. are short on the detail we require to understand scientists' actions at observational and experimental frontiers. Equipped though scientists may be with quite conventional understandings of the category of 'fact', how does language function (if not, as dualists would have it, by description and representation) at the experimental coal-face? Moreover, as I said at the beginning of Part 2, a plausible and genuine *alternative* to the dualist conception needs to be developed before dualism can be convincingly refuted.

In the remainder of Part 2 I look at sociological, anthropological, and historical studies of scientific knowledge whose concern is with some of the detail of scientific research (with so-called 'microscience'), and whose orientation is philosophical. These studies of laboratory science inevitably touch on uses of language in science. Because there are functions of language that are incompatible with the dualist conception (such as those which legislate notions of fact and non-fact), laboratory studies purporting to be close to the coal-face are a potential source of arguments against dualism. However, a microscientific historical study which delivers the required amount of detail will not be introduced until Part 2.2.

In the early pages of *The manufacture of knowledge*, a study of a chemical and physical research centre in California, Karin Knorr-Cetina outlines a 'constructivist' image of a scientific laboratory:

> A local accumulation of instruments and devices ... shelves loaded with chemicals and glassware ... buffer solutions and finely ground alfalfa leaves, single cell proteins, blood samples from the assay rats and lysozymes. All of the source-materials have been specially grown and selectively bred. Most of the substances and chemicals are purified and have been obtained from ... industry ... or from other laboratories. Whether bought or prepared by scientists themselves, these substances are no less the product of human effort than the measurement devices or the papers on the desks.[32]

It would seem, she adds, that if 'nature' is to be found in the laboratory, then the term ought to be defined from the beginning as meaning what is already *the product of scientific work*. This interpretation is not uncommon among authors of laboratory studies. In *Life among the scientists*, an anthropological investigation of an immuno-

[32]Knorr-Cetina, *The manufacture of knowledge*, pp. 3-4. Cf. idem, 'The couch, the cathedral, and the laboratory', pp. 116-118.

logical research institute, Max Charlesworth and his co-authors note the special circumstances under which the institute's yearly supply of 170,000 mice (its main interface with 'nature') is produced:

> The mice behind the pathogen-free barrier are living artefacts — scientifically 'ideal' mice — in the sense that no mice in the real world live as the Ramaciotti [Laboratories] mice do.[33]
>
> They are ... disease-free and of genetically pure strains. Some pure-bred lines [lack] any immune system.[34]

Designer mice and the image of the laboratory as a pre-constructed reality serve as a preface to arguments that Knorr-Cetina uses to undermine what she calls the 'descript-ivist' philosophical model of scientific inquiry. Phenomena are not found, she argues, they are created. Knowledge is not discovered, it is made at every level. Knorr-Cetina furnishes something of a definition of constructivism (not yet a conception of language, however) in the following:

> the products of science are contextually specific constructions *which bear the mark of the situational contingency and interest structure* of the process by which they are generated, and which cannot be adequately understood without an analysis of their construction.[35]

Another aspect of laboratories that authors of laboratory studies like Knorr-Cetina emphasise, and emphasise more than is usual in the philosophy of science, is that laboratories besides being places of human design are also places of human *skill*. Little has been written about skills — or about the dependence of knowledge on the 'place' of experiment.[36] Most skills cannot be verbalised, so they have little visibility in formal scientific accounts. As a general rule, skills in science as elsewhere become invisible as they are mastered. The invisibility of skilled practices plays a major role, as I shall explain later, in the objective appearance or status of many matters of fact that emerge from laboratories.

I present here, in passing, two reasons why the consideration of skills is relevant to a better understanding of the role of language in science. First, a good deal of linguistic

[33]Charlesworth et al., *Life among the scientists*, p. 59.

[34]Ibid., p. 156.

[35]Knorr-Cetina, *The manufacture of knowledge*, p. 5, emphasis added.

[36]Kuhn speaks of his 'failure to recognise how large a part of the apparently non-linguistic component was acquired with language during the learning process' ('Dubbing and redubbing', p. 315, n. 4). For an explanation of the neglect of skills see Rudwick, 'The emergence of a visual language', pp. 149-150. On the local character of knowledge, see Ophir's and Shapin's pioneering 'The place of knowledge', and the excerpt from Lynch's book discussed below.

behaviour or ability is acquired during laboratory apprenticeship, a fact that suggests that language could, to some extent, be analysed *at the level of other skills* alongside which it is acquired. Such an analysis would examine the relations forged between language and other skills in the course of scientific apprenticeship, and ask if in function language complements those skills, and if its role is better thought of as creative or constitutive, rather than reflective, declarative, or descriptive. The following remarks by Charlesworth and his team begin to interrelate language-training and skill acquisition:

> Apprenticeship is essentially a practical rather than a theoretical system of education, well suited to the acquisition of tacit knowledge, secret recipes and techniques ... At the Institute ... Ph.D. students ... arrive scientifically literate, but with little or no experience of the practice of science at this level ... Along the way they will have a part in several publications, jointly authored with others in the laboratory, announcing the results of their work.[37]

> Scientists learn to write as they learn to do scientific research — in the course of an apprenticeship in the laboratory.[38]

Learning the skills of laboratory science includes learning a *great many* new uses of language. The range of new uses and its bearing on my argument will be specified more clearly in Part 2.2. The point to be emphasised here is that vocabularies and ways of speaking, thinking, and writing are passed down from experimenters to their students, in addition to other practical skills.[39]

The second reason for a closer examination of skilled practices is the existence of person- or laboratory-specific skills. In the following longish quotation from Michael Lynch's study of laboratory discourse, the author notes that some laboratory activities were described to him by scientists as accomplishments which were not amenable to written instruction:

> In some cases a written methodological formulation was said to be inadequate as an account which would allow a reader to perform the task competently without

[37]Charlesworth et al., *Life among the scientists*, p. 91.

[38]Ibid., p. 169.

[39]Kuhn touched on these issues in *The structure of scientific revolutions*: 'Scientists ... never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications' (p. 46; see also p. 47). On the acquisition of linguistic skills through practice rather than definition, see idem, *Essential tension*, pp. 302-307. Kuhn understood 'tradition' (to whose continuing authority scientific practice is subject) not as comprising certain abstract principles or concepts, but as embodied in the textual forms of concrete exemplars enforced as conventions of practice.

> prior training in the specific lab that wrote the account. This was more than a matter of the instructions being inadequate for someone who was untrained in the specific sub-discipline, as the inadequacy of the written accounts was said to apply to highly skilled practitioners in brain science from other labs. The tasks for which written formulation were said to be inadequately instructive were those which had been developed within the specific lab's researches. One such accomplishment ... was the use of media chamber apparati which kept excised 'slabs' of hippocampal tissue electrophysiologically viable for periods of time up to twenty-four hours. The construction and management of the chamber apparatus was claimed as a unique accomplishment by lab members, and no other laboratory was credited with the ability to keep *in vitro* tissues 'alive' for as long a period of time. For such tasks, it was said that no amount of written formulation would assure that other labs would achieve the same results. Often, when such methodological skills were transmitted from one lab to another, it was through the medium of a visiting post-doctoral emissary who would be trained in the skill and could then train colleagues in his original lab.[40]

Rare local skills produce phenomena that are *irreproducible* in other laboratories at that point in time. Language divorced from those skilled practices ('written methodological formulations') and used as an instrument for description proves ineffective. Copious language in the descriptive mode is unable to transfer the skills. Knowledge in this instance has a local character and is the achievement of a particular laboratory — a possibility that will remain concealed from students of 'mass-produced' phenomena, to whom knowledge will appear with a universal character, expressible in a universal language that points to the indisputable and reproducible workings of the world. From a fact-constructivist point of view the consideration of skilled scientific practices in relation to language demonstrates that the phenomenal world of laboratories is a created world, dependent on human agency at a physical as much as an intellectual level. In addition, and this is my own paltry addition to fact-constructivism at this stage, language is acquired and used at that level of physical skills.

[40] Lynch, *Art and artifact in laboratory science*, p. 154. Cf.: 'Knowledge is left to be only a part of a craft skill, existing in a person, at a place and time. Its (printed) means of publication may force it to be formally, abstractly stated, or there may be other advantages in such lean statements. But the forms are clearly not all of the skill of doing science, and cannot communicate the entire skill. This skill can only be recreated by people at work at a place and time, whether from master to student or colleague to colleague' (Krohn, 'Toward the empirical study of scientific practice', p. xiii). See also Charlesworth et al., *Life among the scientists*, p. 92; Collins, 'The seven sexes'; Knorr-Cetina, 'Tinkering towards success', pp. 359 f.; Marcus, 'Why is there no hermeneutics of natural sciences?', p. 22; Nyíri, 'Tradition and practical knowledge'; Senior, 'The vernacular of the laboratory', p. 165; and Whitley, 'The context of scientific investigation', pp. 311 f.

2.1.4

One of the earliest sociological/anthropological studies of scientific knowledge is Bruno Latour's and Steve Woolgar's *Laboratory life*, based on material gathered by Latour in the course of a two-year long placement in a leading neuro-endocrinological laboratory.[41] Ian Hacking has referred to the book as 'perhaps the most powerful anti-realist tract',[42] although in fact it is much more than that, and its anti-realist *arguments* are few and far between.[43] Irrespective of how the authors of *Laboratory life* wish to gloss their 'empirical evidence', this and other laboratory studies may be approached, as I have said, for potential arguments against dualism. Moreover, this particular study further elaborates the idea of the 'construction of facts'.

Latour and Woolgar state that they are primarily interested in the process by which scientists make sense of their observations and order them into tidied and systematic research reports. Language and its functions enter their account of laboratory practices at an early stage and remain a significant factor throughout. Members of the laboratory, technicians as well as scientists, are said to be 'compulsive and almost manic writers', who spend the greatest part of their day coding, marking, altering, correcting, reading, and writing.[44] A proliferation of 'inscriptions' is said to be evident everywhere in the laboratory, from numbers chalked onto the furs of rats to the large leatherbound notebooks of technicians, from files and drafts of articles to the library at the physical centre of the laboratory.

The authors of *Laboratory life* attribute a language-intense character to the laboratory's sophisticated machines when they describe them as sources of documents: matter extracted from rats (the animal on which Latour's laboratory relied) is said to be 'transformed' into publications by way of *inscription devices* — items of apparatus that convert assumed characteristics of material inputs into curves, diagrams, figures, or tables of figures *directly useable in arguments*. 'Bleeding and screaming rats', writes Latour in a later work, 'are quickly dispatched. What is extracted from them is a tiny set of figures ... Nothing can be said about the rats, but a great deal can be said about

[41]Latour's and Woolgar's *Laboratory life* first appeared in 1979. Its aim was to be an observer-study of science in the making. On its relationship to other works to emerge from the sociology of scientific knowledge, see Collins, 'The sociology of scientific knowledge'.

[42]Hacking, 'The participant irrealist', p. 277.

[43]Later I criticise these particular arguments as stemming from the dualist conception of language.

[44]Latour and Woolgar, *Laboratory life*, p. 48.

the figures'.[45] His and Woolgar's arguments amount to the claim that laboratory life should be understood as a set of language- or text-orientated activities: Latour's laboratory 'began to take on the appearance of *a system of literary inscription*'.[46] The production of papers, the authors say, was acknowledged by scientists themselves to be the main objective of their activity.[47]

Latour and Woolgar attempt to view both activity in the laboratory and the evolution of scientific facts — the road from uncertainty about the existence of, say, a substance, to

[45]Latour, 'Visualisation and cognition', p. 17. Commenting on Eisenstein's *The printing press as an agent of change*, Latour illustrates what he means by his claim that scientists have stopped looking at nature and look instead at inscriptions: 'The hagiographers say that [Tycho Brahe] is the first to look at planetary motion, with a mind freed of the prejudices of the darker ages. No, says Eisenstein, he is the first *not* to look at the sky, but to look simultaneously to all the former predictions and his own, written down together in the same form ... The discrepancies proliferate, not by looking at the sky, but by carefully superimposing columns of angles and azimuths. No contradiction, or counterpredictions, could ever have been visible' (ibid., pp. 19-20).

[46]Latour and Woolgar, *Laboratory life*, p. 52, my emphasis.

[47]Hacking, in his otherwise positive review of *Laboratory life*, is dismissive of the authors' inclination to view all laboratory apparatus as a system of inscription devices: 'I am afraid I regard this as a symptom of the now out-dated fascination with the sentence so characteristic of Paris intellectuals in the late sixties' ('The participant irrealist', p. 278, n. 1). Yet if Latour and Woolgar accord special status to the sentence/statement they are not alone. Philosophers of science in the analytic tradition have as a matter of course reduced language to sentences and statements (some examples were given in Part 1). Another criticism, this time by Knorr-Cetina, that Latour and Woolgar overemphasise the importance and power of 'inscriptions', because (i) many interpretations of a single inscription are possible, and (ii) an inscription does not *force* a 'dissenter' to look at it — is answered by Latour in language characteristic of his programme: 'such a position misses the point of my argument. It is precisely because the dissenter can always escape and try out another interpretation, that so much energy and time is devoted by scientists to *corner* him and surround him with ever more dramatic [inscriptions]. Although *in principle* any interpretation can be opposed to any text and image, *in practice* this is far from being the case; the cost of dissenting increases with each new ... redrawing. This is especially true if the phenomena we are asked to believe are invisible to the naked eye; quasars, chromosomes, brain peptides, leptons ... One *more* inscription, one more trick to enhance contrast, one simple device to decrease background, one coloring procedure, might be enough ... to swing the balance of power and turn an incredible statement into a credible one' ('Visualization and cognition', p. 18). For early examples of 'heroic devices' and their inscriptions see Hackmann, 'Scientific instruments', pp. 32-33 and 54. For yet another criticism of *Laboratory life*, see Gooding, 'How do scientists reach agreement?', p. 212.

its acceptance as a part of scientific common sense — through a series of transformations between statement types. A five-fold classificatory scheme has statements of type 5 'corresponding' to taken-for-granted facts, type 1 statements comprising conjectures or speculations, and intermediate types formed by various qualifying 'modalities' (type 1 statements being the least certain are the most heavily qualified).[48] Modalities are linguistic devices that can either enhance or detract from the facticity of a given statement. In the following example of a type 3 statement, the phrase 'is generally assumed to be' is a modality — a qualifier of knowledge: 'oxytocin is generally assumed to be produced by the neurosecretory cells of the paraventricular nuclei'.[49]

Latour and Woolgar argue that all scientific statements originate either in literature external to the laboratory or in inscriptions produced internally. Statements submerged in modalities are, in the authors' terminology, 'artefacts'. A scientist's task is to persuade colleagues, through conversation or publication, to drop all qualifications relating to his or her particular assertions and use them as established facts. The process of removing modalities removes also the social and historical circumstances on which the construction of a fact depends. A fact is always 'a statement with no modality ... and no trace of authorship'.[50] It is about something 'out there'.

We have here the outline of two ideas. The first is that between rats' brains and a report concerning a feature of their constitution lie a series of inscription-producing machines and a series of linguistic operations on those inscriptions, as well as on statements derived from those operations. The second is that the feature in question may eventually be caused to 'split off' from the practical operations, instrument readouts, debates, references, and authors assembled to constitute it, thereby taking on a life of its own. In other words, we have here the specification of three important functions for language. First, language (technical and highly structured) enters into the construction of facts in the guise of machine-produced 'data' (one could call them hard-wired interpretations) at a fundamental level. Second, by way of both formal and informal language scientists negotiate, rank, and qualify knowledge derived from the earlier level. Third, knowledge that is negotiated free of modalities is cast in a language that produces *objective* facts — not just 'established' but *natural* facts — apparently untainted by human agency. These three functions that govern the passage of a fact from hypothetical to objective existence mean that the laboratory's output, being the creation of a system of literary inscription and negotiation, is not only inalienably social

[48]Latour and Woolgar, *Laboratory life*, pp. 76-79 and 151.

[49]Ibid., p. 77.

[50]Ibid., p. 82, comment on Fig. 2.3.

(Latour's and Woolgar's view), it is inalienably linguistic. This is a conclusion at odds with the dualist conception of language, which supposes a disjunction of language and the physical or phenomenal constitution of its object (the natural or perceived fact itself).

What are the relevant arguments in *Laboratory life*? Specifically, how do the authors envisage the social construction of a fact? One fact they consider closely is TRF (thyrotropin releasing factor or hormone), a hypothalamic substance postulated in 1962 as the trigger for the production of thyrotropin in the pituitary. Latour and Woolgar provide an overview of the research effort which eventually determined the structure of TRF in 1969. They argue that the substance was at first constructed out of the difference between peaks on bioassay curves (elementary machine inscriptions). TRF was initially *nothing but* the superimposition of two peaks over the course of several trials (no other criteria of identity existed for TRF).[51] Until about 1966 scientists themselves generally avoided categorical statements about fractions being or containing TRF (they were merely said to display TRF-like activity in physiological tests, but other substances could have been responsible or the activity re-interpreted). With the passage of time and as instruments, techniques, and resulting inscriptions accumulated in the laboratories, statements about TRF were rendered less and less subjective and artefactual, and bioassay inscriptions came to be regarded as representations or indicators of an independently existing entity. TRF was said to *exist* in purified fractions.

In the meantime, individual and collective judgements and decisions had changed the field of TRF research and the meaning of TRF itself.[52] Latour and Woolgar argue that if it wasn't for those particular judgements and decisions, TRF research could have gone in a number of different equally 'rational' directions that would not have led to the 1969 result. In any case, the instruction to put every resource into determining the structure of TRF was not given until 1968.[53] By 1969 a synthetic replica of TRF — at least of its biological activity — had been assembled. Through a series of chromatograph tests it was compared with 'natural TRF'. The resulting inscriptions led to protracted negotiations between members of the laboratory. It proved extremely

[51]See ibid., pp. 124 f. See also footnote 45 above, on Latour, Eisenstein, and Brahe.

[52]See Latour and Woolgar, *Laboratory life*, pp. 120 f. Further on the subject of 'defining' a field of research see Latour, *The pasteurization of France*, p. 25.

[53]Latour and Woolgar, *Laboratory life*, p. 136. See also Charlesworth et al., *Life among the scientists*, p. 156; and Hacking's 'realistic' interpretation of this issue, 'The participant irrealist', pp. 291-292.

difficult to decide whether or not the chromatographs of the synthetic and natural samples were sufficiently similar. It was concluded, finally, that they were not.[54] Comparable negotiations followed the proposal of the (synthetic) structure that was finally accepted: Pyro-Glu-His-Pro-NH_2. Latour and Woolgar argue that because judgements about difference and identity in this and in all such cases depend on the context in which the judgements are made, and on negotiations between investigators, it is unacceptable for *observers* of this process (philosophers, etc.) to conclude without qualification that TRF is or is not Pyro-Glu-His-Pro-NH_2.[55]

Now, according to Latour and Woolgar, at this point in the internal history of TRF there is a turning point. No longer did scientists say that purified fractions of TRF give rise to inscriptions 'similar to' those obtained from Pyro-Glu-His-Pro-NH_2, nor did they say that TRF was 'like' the synthetic compound. Instead most modalities were dropped and type 4 statements, such as 'It has now been established that TRF is Pyro-Glu-His-Pro-NH_2', became commonplace. Soon thereafter modalities were removed altogether.[56] The 'turning point' was practical as much as rhetorical. In the early sixties the production of *1mg* of TRF required the crushing of millions of pigs' brains. After 1969, TRF became an off-the-shelf product and more was in existence than in the entire history of the universe. Predictably, nobody bothered to raise millions of dollars to crush millions more brains to obtain enough TRF to submit to conventional chemical analysis to confirm its 'identity' with Pyro-Glu-His-Pro-NH_2. TRF just *became* Pyro-Glu-His-Pro-NH_2 and vice versa.[57]

Latour and Woolgar attach a great deal of importance to this stage in TRF research, for it was then, in their view, that a thoroughly socially constituted identity between TRF and a particular structure was severed from all reference to its producers, the production process, and determinants of place and time. This happened despite the real possibility

[54]Latour and Woolgar, *Laboratory life*, p. 144. See also judgements and negotiations discussed on pp. 154-166.

[55]Ibid., p. 145. On negotiation see also Bloor, 'Durkheim and Mauss revisited', pp. 274-275; Collins, 'The seven sexes'; idem, 'Son of seven sexes'; Charlesworth et al., *Life among the scientists*, p. 158; Gooding, 'How do scientists reach agreement?' (on the creation of 'sameness'); Knorr-Cetina, 'Tinkering towards success', pp. 361 f.; Pickering, 'Against putting the phenomena first'; Pinch, 'Towards an analysis of scientific observation', pp. 13 and 29; Travis, 'Replicating replication'. Concerning a different level of negotiation see Myers, *Writing biology*, pp. 165-185.

[56]Latour and Woolgar, *Laboratory life*, p. 147.

[57]Ibid., p. 245. Latour's and Woolgar's remark that reality is that 'set of statements considered too costly to modify', should be understood in this context.

that TRF might have yet turned out to be an artefact — the supposed cause of an ill-considered effect or process — and might still do so.[58] Having said that, Latour and Woolgar do not wish to maintain that TRF and its mentioned structure are not *facts* — they most certainly are facts, in their view, but like all facts they are *made* facts. The transition from 'established' to 'natural' facts is a rhetorical transition from type 4 to type 5 statements. There is nothing more to the distinction between the two types of fact than that, nothing like a clearer revelation of nature in its true detail. The authors are careful to explain their use of 'fact' and 'artefact':

> Facts and artefacts do not correspond respectively to true and false statements. Rather, statements lie along a continuum according to the extent to which they refer to the conditions of their construction. Up to a certain point on this continuum, the inclusion of reference to the conditions of construction is necessary for purposes of persuasion. Beyond this point, the conditions are either irrelevant or their inclusion can be seen as an attempt to *undermine* the ... fact-like status of the statement.[59]

2.1.5

I mentioned above that two important ideas may be found in the early chapters of *Laboratory life*. The first idea — that basic data from which facts are constructed are linguistically constituted from the very start through the use of inscription devices (hard-wired interpretations) — is not sufficiently argued for or developed in later chapters. In the present thesis, sufficient evidence for the first idea, or at least a version of it — crucial to the refutation of dualism — will not be presented until Part 2.2. It is Latour's and Woolgar's second idea — about the rhetorical construction of the objective existence of facts (and, to a lesser extent, the importance of negotiations over facts and artefacts in the laboratory), that is argued for at length in *Laboratory life*.

The analysis of fact-construction presented by Latour and Woolgar is, therefore, limited. Laboratory *practice* — the manipulation and transformation of objects, substances, relationships, and processes, and the development of skills — is almost completely neglected. The authors' suggestion that TRF began life as an idea put forward by some scientists, and that it continued life as a signal judged by members of a laboratory to be distinct from the background of the field and the noise of the instruments, is too sketchy to support the refutation of dualism. The authors' claim that TRF exists only within a network of social practices (which makes possible its

[58]Ibid., p. 176. See also Hacking, 'The participant irrealist', pp. 289-290.

[59]Latour and Woolgar, *Laboratory life*, p. 176, my emphasis. See also Gilbert, 'Referencing as persuasion'; Latour, 'Is it possible to reconstruct the research process?', p. 57; and Ihde, *Instrumental realism*, pp. 125 f.

existence) is supposed to demonstrate the fact's social constitution;[60] but this too is too vague as it stands. It is the subordinate claim, namely that at the point of 'stabilisation' of a fact the distinction between 'objects and statements about these objects' is *rhetorically* fixed, that foreshadows a manageable argument against dualism.[61]

There is a connection between arguments I examined earlier and Latour's and Woolgar's contention that linguistic resources in a process governed by language are utilised to create the very disjunction of language and world which dualists say exists as a natural or inevitable concomitant of scientific interest in the external or perceived world. Shapin, with Boyle and the early experimentalists in mind, made much the same point about functions of language in early-modern science. Additional and more detailed support for the thesis that objectivity, 'out-thereness', and the non-conventional and non-linguistic nature of facts are together an artefact of the scientific process can be derived from several other historical and sociological studies.[62] I shall briefly overview arguments from two historical studies, and then consider a sociological approach in somewhat more detail.

In his article 'Glass works: Newton's prisms and the uses of experiment', Schaffer documents tactics employed by Newton, his assistants and supporters, to convince fellow natural philosophers to regard the prism as an untroubled experimental instrument. (Early on in Newton's time, the prism was not considered an *instrument* at all and was a novelty in scientific circles.) After a series of experiments with prisms in 1666, Newton reached the conclusion that an 'uncompounded' ray sent through a prism would not change its colour. Many counterinstances to his conclusion were then brought forward by experimenters in other parts of Europe. Newton dismissed all objections, asserting that the prisms employed to produce the counterinstances must have been defective. After becoming President of the Royal Society in 1703 he used his influence to standardise the prism by claiming that all *proper* prisms would replicate his colour experiments and guarantee the truth of his doctrine (all other objects simply

[60]Latour and Woolgar, *Laboratory life*, p. 183.

[61]For the context of the quotation see ibid., pp. 176-177. On science as hypersocial hyperrhetoric see Latour, *Science in action*, pp. 61-62.

[62]See, for example, Charlesworth et al., *Life among the scientists*, ch. 7; Christie and Golinski, 'The spreading of the word'; Harvey, 'The effects of social context'; Latour, *The pasteurization of France*; idem, 'Give me a laboratory and I will raise the world'; Pickering, 'The role of interests in high-energy physics'; idem, 'Against putting the phenomena first'; Pinch, 'The sun-set'; idem, *Confronting nature*; and Rudwick, 'The emergence of a visual language'. On the 'black-boxing' of instruments see Latour, *Science in action*, ch. 2.

weren't prisms). According to Schaffer, Newton thus made 'assent to [his] theories of colour ... a precondition of seeing [the] instruments as untroubled objects'.[63] Because the prism had been brought into line with the widely accepted Newtonian theories of colour, it soon came to be generally regarded as a scientific instrument, uncontroversial in its use. In other words, the prism's capacity to unproblematically demonstrate objective facts about the world was not a given, natural capacity. It was an early eighteenth-century construction.

In another study, David Gooding discusses the importance of visual representations in the reaching of agreement 'about the experience of Nature'.[64] He focuses on Michael Faraday's use of 'magnetic curves' or lines of force. The curves were a method of imaging unfamiliar electrical phenomena, an invention adapted to this purpose from other branches of natural science in the 1820s. In the years following 1836, Faraday used the lines to draw patterns or structures in iron fillings spread near magnets and wires, and also to make sense of and model the interaction of electric and magnetic forces. In origin, lines of force were thus procedural. They were artefacts of a method of observation. They elicited phenomena, and made them communicable and cognitively significant. Yet with the passage of time, as electromagnetic theory developed, the phenomena were both conceived of and treated as independent of the practices and inscriptions that gave rise to them. They came to be seen as *evidence* for concepts, rather than their source. Lines of force, Faraday was to argue in 1852, were physically real facts (his magnetic field theory stated that force exists as a field of physical lines connecting material bodies). Original craft practices had, at this stage, become 'transparent'. In Gooding's words,

> Faraday became so accomplished with many of his experimental practices that he lost sight of them as essential to the production of the experiences he described. As the constructive and enabling practices were mastered, they could be dropped from accounts of how to produce and see natural phenomena. Most of the practice became invisible and ... only the phenomena remained. The residue of phenomena thus came to appear as objects independent of human intervention.[65]

The achieved transparency of the prism (as an instrument) and of the lines of force (as a representational practice) are of a kind with the achievement of objectivity and out-thereness discussed by Latour and Woolgar. Instruments become transparent when they uncontentiously deliver phenomena. Practices of representation become transparent when they show phenomena as they exist in nature. In general, experiments become transparent when, as Gooding puts it, 'the equipment and techniques have been

[63]Schaffer, 'Glass works', p. 99; see also p. 92. Cf. Shapin, 'History of science', pp. 162 f.

[64]Gooding, 'Magnetic curves'; the quoted phrase is on p. 192.

[65]Ibid., p. 216. Cf. Gooding, 'Empiricism in practice', p. 66; and Nersessian, 'Aether', pp. 180 f.

mastered so that they appear to contribute nothing to the outcome'.[66] Similarly, it might be supposed, linguistic practices, conventions, and operations with language become transparent when they are nothing but labels, signs, or descriptions of natural processes — of things, their properties, and interactions — as they stand in their own right in nature (or appear in our common perceptions).[67]

2.1.6

Nigel Gilbert and Michael Mulkay conducted a series of interviews over a period of two years with about half the scientists active in the field of bioenergetics in Britain and North America. (Bioenergetics is the study of organic processes that create, transport, and store chemical and other kinds of energy.) The two sociologists concurrently undertook an analysis of all bioenergetics research papers.[68] In their book they describe certain recurrent features of scientists' formal and informal discourse. In particular, they identify two common and contrasting 'rhetorical repertoires' in scientists' accounts of their own and their colleagues' actions and beliefs. They refer to these as the empiricist and contingent repertoires.[69]

Research papers, Gilbert and Mulkay argue, tend to give experimental data chronological and logical priority. Authors' involvement with or commitments to particular theories are played down, their social ties with other experimenters in the field not mentioned at all. Characterisations of experimental procedures are highly conventionalised — they are presented as instances of impersonal and universally effective techniques. Style of writing is almost completely impersonal too — eliminating all traces of an interpretative product. By adopting these linguistic devices, argue the two sociologists, scientists construct texts in which the physical world seems to speak for itself. These are the texts of the 'empirical discourse', texts employing devices of the empiricist repertoire.[70]

[66]Gooding, 'In nature's school', p. 132. Cf. Tiles, 'Experimental evidence vs. experimental practice?', p. 107.

[67]Langer, from within the traditional philosophy of science, has commented on the 'transparency' of language — see *Philosophy in a new key*, pp. 75-76.

[68]Gilbert and Mulkay, *Opening Pandora's box*. For a defence of the importance of analysing scientists' talk see Knorr-Cetina, *The manufacture of knowledge*, pp. 20 f. For a different analysis of scientists' discourse, see Lynch, *Art and artifact in laboratory science*.

[69]See Gilbert and Mulkay, *Opening Pandora's box*, pp. 55 f.

[70]Gilbert and Mulkay, *Opening Pandora's box*, p. 56. On scientists' de-contextualisation of their formal work, see Knorr-Cetina, *The manufacture of knowledge*, pp. 94 f.; Medawar, 'Is the scientific paper a fraud?'; and Woolgar, 'Writing an intellectual history of scientific development'. On

philosophers' tendencies to de-contextualise scientific work see, for example, Lakatos, 'History of science and its rational reconstructions'; and McMullin, 'Philosophy of science and its rational reconstructions'. Charlesworth et al. quote a scientist from a medical laboratory: 'In general in this lab, we're simply following an intellectual train of thought until we've got enough data. Usually it goes in spurts: you go for six or eight months and then you think. 'I must write some of this up'. So you write two or three papers. Or you reach a point where there's a quantum change in the information, and you write that up ... The work is not divided into paper-sized sections' (*Life among the scientists*, p. 167). Cantor comments that the scientific publication is a 'retrospective narrative', involving an impersonal and passive reconstruction which draws attention to those theories, tests, and data which are considered to be of interest to the scientific community ('The rhetoric of experiment', p. 160). It is known that differences between published journal papers and the laboratory notebooks to which they are related are sometimes immense, not only in 'content' but also in the literary conventions to which both are subject. An example is the differences discovered between Millikan's laboratory notes and published results for his oil-drop experiment. On this see Holton, *The scientific imagination*, pp. 51-83. Laboratory notebooks are compiled in the midst of uncertainty about the meaning of at least some of the results and uncertainty about the eventual outcome of experiments. Published papers, by contrast, provide an at least temporary settlement of what has transpired in the notebooks and elsewhere in the laboratory in the course of experimentation. They do so in a language that is cleansed of idiosyncrasies and accidents of practice and expression (see, for example, Latour and Woolgar, *Laboratory life*, pp. 76 and 176). Cf. Gross: 'Experimental papers ... are not so much reports as enactments of the ideological norm of experimental science: the unproblematic progress from laboratory results to natural processes ... By means of its patterned and principled verbal choices, science begs the ontological question: through style its prose creates our sense that science is describing a reality independent of its linguistic formulations' (*The rhetoric of science*, p. 17). Woolgar comments on another rhetorical aspect of the construction of out-thereness: 'The externality of the events described in the text is in part provided for ... by the use of passive formulations and by the invocation of community membership. At the same time the text is clearly about the author's individual part in the events leading up to the discovery. The solution to this particular aspect of the author's dilemma is the portrayal of the *coincidence* of the inevitability of unfolding events with things which happened to the author' ('Discovery', p. 255). Pickering suggests a communal framework for the emergence of out-thereness: 'the "reality" of concepts emerges from this flux of changing networks in the following way. Firstly, consider the particular network ... engaged in the elaboration of a particular exemplar. Entrenched within the practice of this network will be certain concepts central to the exemplary achievement. These concepts will be at the heart of the way in which members of the network make sense of their own and each other's work ... Because of this a limited 'out-thereness' is achieved — the concepts become the relatively impersonal 'property' of the network ... Secondly ... it is possible for members of distinct networks to be engaged in the elaboration of the same concepts in different research areas ... Thus, the concepts originally entrenched in the practice of a single network

Should anyone think that discourse of this kind comes naturally to scientists or is uniquely suited to scientific inquiry, Gilbert and Mulkay retort that a discourse flowing from an *incompatible* repertoire is just as widespread, only not in formal contexts. They call this the contingent repertoire and see it manifested in scientists' informal talk about action and belief. In informal contexts, they write, scientists present their actions and beliefs 'as heavily dependent on speculative insights, prior intellectual commitments, personal characteristics, indescribable skills, social ties, and group membership'.[71] In other words, the informal accounts scientists construct are radically different in content from those appearing in comparable formal texts. Gilbert and Mulkay mean the word 'contingent' in 'contingent repertoire' to allude to scientists' acceptance in informal contexts of the possibility that their scientific views would be different if their personal or social circumstances had been different.

The authors suggest that the striking difference between the two repertoires (as well as the fact that scientists can switch between them at will) is best demonstrated when scientists account for error and correct belief. The demonstration proceeds from an analysis of interviews during which speakers identified the views of one or more bioenergeticists as mistaken, while also providing some sort of an explanation of why those scientists had adopted an incorrect theory or failed to adopt a correct one.[72] Gilbert and Mulkay note that, when the different interviews are compared, speakers can be seen to be advancing a wide variety of conflicting views about a fairly narrow range of biochemical phenomena. Despite this narrow range, all speakers considered their personal positions to be unproblematic direct representations of nature. By contrast, they asserted that the actions and judgements of those scientists they believed to be in error were comprehensible in terms of various special social attributes that the (wrong-headed) scientists possessed as individuals: they all fell within the category of 'strong

can come to be entrenched in the practice of others, and their 'out-thereness' correspondingly increased' ('The role of interests in high-energy physics', p. 110).

[71]See Gilbert and Mulkay, *Opening Pandora's box*, p. 56. Gilbert and Mulkay's remarks on the contingent repertoire are reminiscent of Latour and Woolgar's emphasis on the importance of negotiations carried out in the course of ordinary everyday experimental life. Formal accounts of products from this sphere are merely highly conventionalised renderings, recognised as such by insiders, but taken by some outsiders unfamiliar with fact-construction as displaying the 'logic' of discovery (see *Laboratory life*, pp. 154-166). On the importance of 'discursive interaction' see Knorr-Cetina, *The manufacture of knowledge*, p. 14.

[72]See Gilbert and Mulkay, *Opening Pandora's box*, pp. 63-89.

individuals who want to interpret everything in terms of their theories'.[73] Speakers' 'empiricist' reconstructions of their own views were given interpretative priority — they were presented as reflecting realities of the natural world — whereas divergent views were presented as in need of explanation. To explain how it was possible that other scientists came to represent the natural world so inaccurately, speakers resorted to contingent discourse. It was possible, they asserted, because of distorting, non-scientific, non-experimental influences on other scientists' views.

Accounts of error were, thus, organised around assymetrical uses of empiricist and contingent versions of action and belief. Gilbert and Mulkay appear to suggest that empiricist discourse in science is restricted to just two contexts: formal publication, and justification of one's views. The central theme of correspondence of formally expressed beliefs to the world, which they see as a characteristic of empirical discourse, is thereby nothing but a rhetorical flourish — a convention, a tradition — that is atypical of laboratory life in general (which is dominated by the contingent repertoire), and thus atypical of the discursive circumstances in which scientific knowledge is produced.[74] Similarly Latour, in a recent book, argues that the world's being in a certain way is made the *reason* for the settlement of a scientific controversy (by scientists or positivist historians) only *after* a controversy has settled. In the course of a controversy all participant scientists know that what the world is like is something they will have to sort out amongst themselves. As long as a controversy lasts, no appeal to the nature of the world can possibly help any side. After a controversy is settled, what the world is like emerges all of a sudden both as the cause of the controversy's resolution and as the ultimate justification of the victorious scientists' beliefs. In Latour's opinion such rhetorical invocations cannot be taken seriously.[75]

[73]Ibid., p. 68.

[74]On scientists' methods of justifying their past actions and present beliefs, see Gilbert and Mulkay, 'Experiments are the key'. See also Gilbert, 'Referencing as persuasion'; Gilbert and Mulkay, 'Contexts of scientific discourse'; Kemp, 'Controversy in scientific research'; Mulkay and Gilbert, 'Accounting for error'; Mulkay and Gilbert, 'Scientists' theory talk'; Mulkay and Gilbert, 'What is the ultimate question?'; and Yearley, 'Textual persuasion'.

[75]Latour, *Science in action*, pp. 96-100. Cf. Latour and Woolgar, *Laboratory life*, pp. 180-182; and also Latour on the inescapability of reconstruction: 'Each interview, each manipulation of analog, each writing is, in a way, a reconstruction. This does not mean that there is something 'wrong' or 'dishonest' with this process, because there is nowhere any account of the research that could be something more than a fiction' ('Is it possible to reconstruct the research process?', p. 69). See also Gilbert and Mulkay, 'Experiments are the key', p. 113.

In the preceding pages I have looked at several ways in which language has been found to be central to the construction of facts. At the highest level language is used to establish what things should count *as* facts and what things should not [Shapin]. It is used to recommend exemplary uses of experimental language (and undermine others), to enforce linguistic styles and conventions to be followed in the expression of facts (again, at the expense of others), to get experimental experience beyond laboratory walls (by means of the literary technology of virtual witnessing), to structure social relationships that promote consensus, and to develop a (language/world) rhetoric that carries conviction [Shapin, Bazerman]. In the laboratory, language is used to negotiate novel phenomena, to decide or produce consensus about the significance of instrument readings, to evaluate the abilities of rival investigators, to redefine research objectives and criteria of identity and success, and to qualify knowledge claims. Language is used to establish the factual status of some knowledge claims by separating them off from circumstances of their production, or to challenge the facticity of other claims by reintroducing those circumstances [Latour and Woolgar]. Language is used to render transparent instruments and practices of representation by which phenomena are constructed and depicted [Schaffer, Gooding]. Rhetorical strategies are employed to systematically underplay human agency in the course of justifying 'correct' belief and to emphasise agency in the course of explaining 'erroneous' belief [Gilbert and Mulkay]. Other uses of language ensure that scientific facts, consensus, and certainty appear ultimately grounded in 'the way the world is' rather than in human negotiations about what the world is like [Latour].

This summary of functions of language in the production of knowledge, though it does not refer to functions at a fundamental level of fact-construction (viz. exploratory experimentation and the creation of new facts), far surpasses anything ever defended by philosophers of science in the analytic tradition. Consider, for example, a suggestion by Norwood Hanson which pales by comparison:

> Another influence on observations [in addition to prior knowledge] rests in the language or notation used to express what we know, and without which there would be little we could recognise as knowledge.[76]

[76]Hanson, *Patterns of discovery*, p. 19. Cf. Ravetz: 'even though the scientist is concerned with properties of the external world, the work he produces will be characterised by a certain style unique to himself' (*Scientific knowledge and its social problems*, p. 104). In Shapere's, 'The influence of knowledge on the description of facts', there is another lukewarm attempt to play down the language/ world disjunction: 'The language ... of science [is not] independent of and prior to the attempt to arrive at knowledge-claims about the world; on the contrary [it is] no less learned than are the substantive

Here language is depicted not as a resource with which people actively construct and engage with their world (the blooming desert metaphor). but as a passive notation into which observations are transcribed (the ink-blot metaphor). The implication is that observations and 'what we know' are in principle disengageable from language (even though Hanson indicates that the former two are heavily dependent on the latter). The 'influence' that language is said to have on observations, besides providing a means for their expression, is left unexplained. Hanson's passive conception of language is underscored a few pages later when he talks about 'series of statements' (again, the supposed influence of language on observations is left unexplained):

> The 'foundation' of the language of physics, the part closest to mere sensation, is a series of statements ... Our visual sensations may be 'set' by language forms; how else could they be appreciated in terms of what we know? Until they *are* so appreciated they do not constitute observation: they are more like the buzzing confusion of fainting ... Knowledge of the world is not a montage of sticks, stones, colour patches and noises, but a system of propositions.[77]

The contrast between this view of language — according to which language 'sets' the buzzing confusion of sensations by assigning to it a system of propositions — and the view that is implicit within the works of the historians and sociologists I have mentioned, may be put down to different definitions of 'language'. In the first case, it may be said, all that is meant is a familiar notational system (an intermediary between our knowledge and the as-yet unknown world). In the second case, what is meant is everything from words, statements, styles of expression, conventions of writing, and rhetorical repertoires, to typical *uses* of language including, for example, negotiating results and creating consensus. But differences may also be put down to different conceptions of what scientific inquiry involves, and to definitions of language that *these* conceptions allow. In the first case scientific inquiry involves the recording of observations in the light of theory, ultimately producing knowledge of the world in a system of propositions. Here the conception of language as a medium of guided

claims at which science arrives. We learn *how* to learn and talk about the world *as* we learn about it' (p. 297).

[77]Hanson, *Patterns of discovery*, p. 26. Hanson writes that in nineteenth-century physics 'it became unthinkable that any event could be describable in both [corpuscular and wavelike terms] at once. 'Unthinkable' here means not only 'unimaginable' but also 'notationally impossible'. For in the only languages then available for describing particles and wave dynamics, such a joint description of phenomena would have constituted a virtual contradiction' ('Philosophical implications of quantum mechanics', p. 42). Language here is said to limit ways in which thinkers can express themselves. Still, Hanson treats language and its effects as little more than a notational system used in the 'description of phenomena'.

transcription almost suffices.[78] In the second case scientific inquiry involves the negotiation of everything, from facts and how to talk about facts to what knowledge and natural reality themselves are. Here the conception of language as a variety of *practices* seems more appropriate.

2.2 Gooding on the experimental making of meaning

Nowhere is the latter conception better investigated than in Gooding's *Experiment and the making of meaning*. The book is a detailed study of the making of scientific facts and the first distillation and systematic examination of the major philosophical themes often concealed behind historical and sociological detail in other works (such as the works mentioned earlier). In the following pages I draw on arguments developed in Gooding's book to clarify and secure the constructivist conception of language.

2.2.1

Experiment and the making of meaning begins with a proposal to investigate experiment as a source of knowledge, meaning, and reference. Modern philosophy, Gooding claims, has neglected the practical and procedural aspects of 'empirical access'. We thus have no theory of how new information is gained through experiment, 'of how the unfamiliar can be represented yet still retain its potential to change the structure that represents and explains it'.[79] In a programmatic article published in 1986, entitled 'How do scientists reach agreement about novel observations?', Gooding emphasised the importance of understanding the processes by which individual scientists construct representations of novel aspects of nature (defined as those aspects which no one knows how to represent) and endow them with common meanings. Two closely related issues were shown to be at stake. First, how do scientists begin to make their experiences meaningful? Second, how does novel perceptual information make 'the passage from personal experience to public discourse'?[80]

[78]Cf. Coffa: 'One imprecise but vivid characterisation of [Carnap's] protocol language is that of a language in which the "original Protocol" is written. The scientist's protocol is the book in which he registers the theoretically unpolluted results of his daily observations in the laboratory. "Protocol sentences" are statements in that book ... The idea was to think of the scientist's utterance of a protocol sentence as essentially indistinguishable from an ammeter displaying a scale reading' (*The semantic tradition*, pp. 357 and 359).

[79]Gooding, *Experiment and the making of meaning*, p. 25. See also pp. xi f.

[80]Gooding, 'How do scientists reach agreement?', p. 206.

The two issues are about explaining low-level scientific fact-making and consensus. How is the emergence of novel observations and consensus about novel observations possible? Where should we look for evidence for it and how should we conceptualise it? *Experiment and the making of meaning* attempts a detailed response to these questions, so crucial to the refutation of dualism. The following passage contains a general statement of the book's methodology:

> How do observers move from the concrete, practical context of individual experience of particulars to the realm of discourse about shared experience in which generalization, argument and criticism are possible? To answer this we must venture beyond the boundaries of explicit, declarative knowledge into the observational frontiers at which experience is fashioned and procedures for making and communicating it are mapped out.[81]

Gooding appears to promise an extensive consideration of language and its functions. If we are to shun merely 'declarative' knowledge, and go beyond the boundaries of its explicit expression, it seems that we shall encounter procedures — presumably language-driven — by means of which experimenters 'fashion' experience and make it communicable, and that from here we shall be able to follow the movement of experience into the realm of public 'discourse'. The distinction between declarative knowledge and knowledge that is fashioned or made suggests the possibility of a distinction between 'passive' (descriptivist or dualist) and 'active' (interventionist or constructivist) conceptions of language. There is a promise, then, in the book's methodological statement, that Gooding's arguments will have a significant bearing on issues of language.

2.2.2

The argument in *Experiment and the making of meaning* proceeds from an analysis of the development of Faraday's electromagnetic theory, from its roots in Hans Christian Oersted's discovery of electromagnetism in 1820, to its mature phase over three decades later, when Faraday was able to specify the relationships between electric and magnetic quantities. A chapter on the search for quarks extends the argument into late twentieth-century science, complex experimental systems, theory-driven research, and thought-experiments.[82]

In 1820 Oersted's experiments with freely suspended magnetised needles and a wire conducting electricity led him to report anomalies in the behaviour of the needles in the vicinity of the wire — anomalies, that is, within the context of prevailing Newtonian

[81]Gooding, *Experiment and the making of meaning*, p. xiii.

[82]Ibid., ch. 8.

(ponderomotive or push-pull) assumptions about force.[83] It appeared to Oersted that a conducting wire acted on a suspended needle 'circumferentially', that is, at right angles to what would have been expected at the time. Oersted's report was somewhat vague in its specification of his actions and the phenomena observed. Gooding shows Oersted struggling to find language in which to interpret what he did and what he saw. His concepts were as yet too underdeveloped to talk about electromagnetic facts independently of his particular experimental situation. His ways of seeing (like those of his contemporary fever theorists and disease doctors) were as yet not compelling, not even to himself. In the event, enough specification of the effect on the needle and the curiosity of the effect was there to induce others to look for it. Within a month of the original publication various experimenters were in agreement that the 'transverse' or 'skew' effect, as it was variously called, existed.

In 1821 Faraday, who had repeated Oersted's experiments, reported in a review of the emerging field that it was 'easy to see' what Oersted described. He even went so far as to say that it was 'easy to see ... that the movement of the needle took place in a circle round the wire',[84] even though no one had produced experimental conditions to realise circular motion. Sir Humphry Davy (Faraday's senior at the Royal Institution) claimed that the effect was 'perfectly evident', and Jean-Baptiste Biot, in Paris, wrote that it 'necessarily' lead to the idea of a circular force. André-Marie Ampère concurred.[85] Gooding (who has repeated many of the early electromagnetic experiments contained in the original notebooks) argues that by the time these confident statements were published, Faraday, Davy, Biot, and Ampère had already come to understand their own experimental work in terms of a reconstructed, retrospective rationale. They had become:

> fluent in the manipulation and interpretation of phenomena which *are in fact* unruly, especially for the inexperienced practitioner each had been in September 1820. They had mastered observational techniques and constructed representations which *made* the phenomena 'easy to see' and 'self-evident'.[86]

The tentative techniques and constructs disseminated in Oersted's report enabled an early consensus about electromagnetic observations. Gooding shows that that consensus depended on other experimenters refining Oersted's techniques and constructs and, in turn, disseminating language and visual representations (stable, ordered images). These had made experimental outcomes visible to individual experimenters

[83]See Gooding, *Experiment and the making of meaning*, pp. 30 f.

[84]Ibid., p. 59.

[85]Ibid., pp. 35-36.

[86]Ibid., p. 36.

and were intended to do so to fellow experimenters and readers more generally. Consensus grew around represented phenomena, of course, not uninterpreted stimuli.[87] Consensus, moreover, arose as much in *interactions among scientists* as in impersonal interactions with nature. Interactions among members of the emerging electromagnetic community, argues Gooding, were 'a necessary condition for their interventions in the course of nature and of their conceptualizations of the results'.[88] He calls the products of early attempts at representing novel experience 'construals'. They are the currency at the frontiers of the experimental economy:

> Construals enter discourse as the practical basis for realizing and communicating novel experience. Until the wider significance of novel information has been sketched out, construals of it retain the provisional and flexible character of possibility: they may be compatible with several theories or with none. They are practical, not theoretical facts.[89]

Construals are thus not associated with established observational practices. They are only potentially meaningful beyond the short term. Those that survive the early stages of meaning-making may end up as more broadly accepted theoretical interpretations, abstracted from observational practices and standing in a reference relation to those practices. The reference-generating consequences of successful construing are examined later in this section.

By the end of the year 1820 experimenters on both sides of the English channel were in agreement about the anomalous transverse electromagnetic action.[90] Tentative and 'private' results had become public, collectively witnessed, and self-evidently natural facts. But whereas Biot and Ampère maintained that the anomalous action was not a good guide to the real nature of electromagnetism, and that attraction and repulsion were primary, Davy's and Faraday's investigations were more open-ended. It was not until the 1850s that the emphasis in London would shift from the accumulation of experimental information to its theoretical interpretation. Gooding presents this as an

[87]Ibid., pp. 36 f.

[88]Gooding, 'How do scientists reach agreement?', p. 205. See also Gooding, 'A convergence of opinion on the divergence of lines'.

[89]Gooding, *Experiment and the making of meaning*, p. 115. Tiles writes of Gooding's construals: 'the crucial process, which the philosophy of science has overlooked, is that by which experimenters transform epistemologically worthless, privately experienced, interactions with the world into publicly accessible phenomena, by generating an interlocking vocabulary and determinate set of procedures in which other members of a community can participate' ('One dimensional experimental science', p. 347).

[90]For a list of the London scientists involved see *Experiment and the making of meaning*, p. 97.

example of the capacity — neglected by philosophers — of exploratory experimentation to lead to agreement about observed effects *despite* theoretical underdevelopment or diversity:

> Comparing Davy's early investigations of the phenomenon to Biot's shows that, despite fundamental differences in their view of reality and of appropriate methods of investigating it, they elicited very similar results and they at first construed these in almost the same way ... Differences in the theoretical vocabulary that developed can be explained in terms of theoretical differences [reflecting cultural differences in beliefs about the evidential status of phenomena] rather than the primary phenomena and their replicability.[91]

Gooding's argument is that despite the involvement of 'cultural' resources in the construction of phenomena, outcomes of low-level experimentation cannot be regarded as theoretical postulates or purely linguistic constructs. Even without the cross-Channel comparison, it would seem that when, for example, Davy wrote that 'the steel fillings arranged themselves in *right angles* always at right angles to the axis of the wire',[92] he was not acting under the compulsion of theory, even though arguably he was through his own agency constructing, idealising, and generalising phenomena that could not be easily or without dexterity accomplished. The simplest cases of exploratory experimentation engage cultural resources, *but not solely.* This point is amplified later.

Gooding details a number of experiments conducted by Davy and Faraday in the years 1820-21, from which the clarity they claimed for their results emerged.[93] The experiments — most of which Faraday recorded in his notebooks — highlight the interaction of observers' manipulations of objects, percepts, and concepts. They speak of the accumulating skills and inventiveness of the experimenters, in particular of Faraday, who was engaging in delicate operations designed to break down the behaviours of needle/wire configurations into stable and repeatable needle 'responses'. Skilled manipulation of apparatus, argues Gooding, structured the space in which phenomena were made visible. Outcoming phenomena (construals of experience) were, thereby, 'practice-laden':

> When ... Davy and Faraday arranged their operations and the outcomes in images and devices they tacitly embodied accumulated observational skills which were *impossible* to communicate in representations that were easy to communicate ... They drew upon ordinary, familiar images and relationships (circles, discs, axles) to represent aspects of the phenomenon.[94]

[91]Ibid., p. 50.

[92]Quoted in ibid., p. 51.

[93]Ibid., pp. 52 f.

[94]Ibid., p. 63.

Practice-ladenness is an important concept for Gooding, as it draws attention both to the practical skills on which the making of meaning and knowledge depend, and to what he calls 'the symbiosis of thought and action' in exploratory experimentation.[95] Construals are the upshot of that symbiosis because, among other things, they bring together language, manipulations of experimental apparatus, reactions of the apparatus, and reactions of other experimenters. They are situational and context-dependent. Observers' agency is implicated here as essential both to eliciting experiential possibilities and to rendering them as observable features of nature.[96]

Does the practice-ladenness of communicated phenomena described in the passage above amount to something more than the claim that their made visibility *presupposed* (rather than 'embodied') complex practical skills? Put simply, is Gooding not saying that language required to make phenomena disseminable and more widely visible was brought to bear *declaratively* on what had already been locally understood and pre-structured by extra-linguistic activity? Is not Gooding's conception of language one that has language merely transcribe and communicate? No, for two reasons.[97] First, Gooding does not distinguish experimental operations with language from other factors which he says make investigative outcomes practice-laden. He presents linguistic operations as inseparable from low-level experimental practice: construing is a pre-theoretical interpreting (language-dependent) activity that only has sense in the context of action. Second, Gooding maintains no distinction between pre-linguistic and linguistically-rendered phenomena (between 'facts' and 'expressed facts'). Making sense of novel phenomena is a process which depends on experimenters finding or inventing ways of expressing sense as they go along: 'the natural world contributed something to what Biot, Davy and Faraday saw. Yet they had to make something *of it* in order to share it'.[98] Construals are what those experimenters dealt in. They were compounds of language and practice. The evidence from the resolution of unruly needle behaviour by Faraday and others into something exhibiting structure rather than

[95]Ibid., p. 60.

[96]See ibid., pp. 71 f.

[97]See ibid., pp. 72 f.

[98]Ibid., p. 71. Cf.: 'The observer must ... invent a description which he can communicate so that the attention of others can be directed to an aspect of a phenomenon ... New concepts may have to be constructed, or existing concepts reconstructed. In either case, their adequacy will have to be argued and demonstrated' (Gooding, 'How do scientists reach agreement?', p. 208).

chaos is taken by Gooding as evidence for the thesis that 'the experience of unfamiliar phenomena is inseparable from rendering it'.[99]

The suggestion here of an interdependence of conceptualisation and exploratory action calls into question the language/world disjunction and associated correspondence views of the relationship of representations-of-the-world to the world itself:

> The indexical, action-bound nature of observation makes human agency a *part of* the 'stimulus' or causal context. The dependence of 'stimuli' on observers' own agency compromises the objective status claimed for them by stimulus theories, as emanating unassisted from the natural world.[100]

Here the earlier idea of 'embodiment' has a clearer, literal sense. Low-level construals of phenomena are not the products of experimenters confined to disembodied minds. Rather, the construals (and the phenomena themselves) are the products of the experience of communally situated actors. As indicated earlier, Gooding's account of novel observation emphasises that observers' interactions with each other are as important as the time-honoured 'observer-world' interactions. Interactions with the world, Gooding argues, cannot be made intelligible *independently* of interactions with other observers.[101] Only when measured against the experience of others can a construal's ability to convey and shape experience be established. The language of electromagnetism was shaped in many places besides laboratory benches, including lecture theatres and the pages of journals where construals of phenomena were put forth, argued for, and exchanged.

2.2.3

Gooding is able to demonstrate that in Faraday's *Diary* natural phenomena make a progression 'from their inception as personal, tentative results to their later objective status as demonstrable, natural facts'.[102] To this progression Faraday was to give a quite different *public* face:

[99]Gooding, *Experiment and the making of meaning*, p. 75. On experimentation as 'progressively refined articulation' see Galison, *How experiments end*, p. 127. Cf. Travis, 'Replicating replication', p. 17.

[100]Gooding, *Experiment and the making of meaning*, p. 75. Cf. Charlesworth et al.: 'The products of data-generation systems are then not simply constructs, but are the combined effects of human labour applied to materials through the use of tools and techniques ... The data produced by this labour process are as a consequence materially constrained by reality in the sense that they reflect the conditions of their production' (*Life among the scientists*, p. 159).

[101]See, for example, Gooding, *Experiment and the making of meaning*, p. 85.

[102]Gooding, 'In nature's school', p. 108.

> [Faraday] presented his experiments as a process of learning from nature. He wanted his audiences in the great lecture theatre of the Royal Institution to think of themselves as being in 'Nature's school'. Closer examination of the development of his experiments suggests a different image of nature as collaborator rather than instructress.[103]

Faraday succeeded in making continuous, rotatory motion late in 1821. His first successful experimental apparatus involved a section of wire with a crank in it suspended by cork in water, its upper and lower ends dipped into containers of mercury connected to the poles of a battery. Faraday managed to bring the crank to move around the wire's axis on the approach of a magnet. He later succeeded in making a device for demonstration purposes exhibiting continuous motion of the tip of a vertically suspended (and straight) wire around the end of a cylinder magnet.[104] Using Faraday's laboratory notebooks Gooding attempts to recover the sequence of practices and construals that led to the realisation of the rotations.

In the wake of Oersted's discovery of circumferential tendencies of needles in the needle/wire setup, a prediction had been vaguely formulated (initially by William Wollaston) that the wire should have a tendency to rotate or 'revolve' around its own axis. Gooding shows that this prediction, or expectation, was only retrospectively 'clarified' by Faraday, whose investigations involved a very flexible notion of what the possible phenomena were. With problems not well-formulated at the outset, experimental practice rather than theoretically-driven practice was the source of Faraday's representations. His exploratory activity was carried out in the face of uncertainty about outcomes, and some of that uncertainty is preserved in his notebooks. As a rule, however, reconstructed accounts of experiments (including laboratory notebooks) hide procedural uncertainty from hindsight. They impart an inferential structure to the narrative, especially reconstructions that are intended for publication. They describe experience that has been made 'stable' and reproducible and contain little information about how it was formed.[105] Gooding argues that the recovery of

[103]Ibid., pp. 105-106.

[104]Gooding, *Experiment and the making of meaning*, pp. 118 f.

[105]In Charlesworth et al., *Life among the scientists*, a scientist is quoted on the importance of *tinkering with instrumentation* in malaria research: 'If you think about, say, the pulse field work ... it was really a matter of "here's a new machine, now let's play around with it and see what it does". There has been a lot of tinkering and playing, and out of it has come some useful results' (p. 153). The results are written up in tidied, reasoned, purposeful research reports. Charlesworth et al. argue that the concept of 'experiment' is itself mostly put to retrospective, order-producing, chaos-disguising uses: 'Having a set of data-generation systems in place enables a number of experiments to go on

procedural uncertainty from Faraday's notebooks shows that the phenomena he disclosed were not ready-made or self-evident facts about nature.[106]

Faraday accumulated understanding as he construed his exploratory behaviour and its outcomes, but his *experience* remained plastic. Gooding observes that Faraday's notebooks show him returning to earlier entries to interpret them in the light of later construals. This process of reconstruction wound down — and the plasticity of experience diminished — as phenomena (such as circular motion) eventually stabilised. In making sense of his experience in this way Faraday was solving practical problems of representation. He needed to be able to move from discrete actions in three dimensional space and real time to words, images, and models which would make those actions intelligible to himself and communicable to others. This movement took place within one, unitary conceptual space, not across the disjointed ontological spaces of language/world dualism. Because movement from actions to words, images, and models was not guided by explicit rules, it was the cause of uncertainty additional to the procedural uncertainty mentioned above. An instance of this representational uncertainty is Faraday's inclusion of instructions to himself on how to read his own drawings.[107]

When Faraday first put a wire in continuous motion what he observed was quite unlike what he expected. The setup he used was a precursor of the first apparatus described earlier — the wire had no crank to start with. On the approach of the magnet the wire 'was thrust ... from side to side'.[108] This *lateral motion* was the first indication Faraday had that some sort of continuous motion was achievable. Possibly in an attempt to analyse lateral motion he proceeded to vary the apparatus by adding the crank to the wire. Gooding shows that in moving from one variation of the apparatus to the next Faraday was constantly 'shifting between actions, construals, description of procedures, inference to other possibilities and interpretation of behaviour and its results'.[109] He was open to the possible significance of effects quite unlike those he

concurrently. These interactive pieces of research require further tactical decisions about which particular elements should be isolated and designated with the title 'experiment' for the purposes of presentation in a journal article. This rhetorical device, 'the experiment', conceals the complex set of interactions between instruments, skills, techniques, raw materials, theory and social networks which together provide for the possibility of experimental data generation' (ibid., p. 159).

[106]Gooding, *Experiment and the making of meaning*, pp. 121 f.

[107]Ibid., p. 142. See also Gooding, 'Putting agency back into experiment', p. 99.

[108]Quoted in Gooding, *Experiment and the making of meaning*, p. 128.

[109]Ibid., pp. 129-130.

had articulated previously. The plasticity of his apparatus matched the plasticity of his understanding of his experimental activity.

Gooding locates Faraday's agency in the experimental process by means of a novel system of representing experimental practices. Gooding's multidimensional 'maps of cognitive activity' display the interaction of the 'conceptual and material worlds'.[110] They are abstractions of practical reasoning which put thought processes on an empirical footing by showing how they connect with empirical activity. They demonstrate, in Gooding's words, that 'phenomena *always* appear as outcomes of human activity. No map begins or ends with a phenomenon, there are no disembodied acts and no meaningfully disembodied thoughts, decisions, or conclusions'.[111] The wider use of these cognitive maps of practice will certainly further our understanding of the origin of 'natural facts', but there is no need to reproduce the system in any detail here. Suffice to say that the system preserves features of experimentation that are absent from rational reconstructions of experiment and retrospective accounts of science generally. In these accounts experiment is reconstructed as a process of reasoning resembling formal argument, guided by rules and expectations, leading to readily representable and communicable outcomes that are then evaluated theoretically in terms of those expectations. It will be apparent by now that Gooding's reading of Faraday's notebooks does not support this kind of reconstruction. Faraday did not reason his way through experiments in the way rational reconstructions suggest. His experimental activity *enabled* argument. It was not the unfolding of an argument. Gooding's experimental maps graphically demonstrate these points.

The motion of the wire that Faraday construed as lateral was not expected. Nor, as he proceeded to establish, was it an artefact of his apparatus. The perceived constancy of the result led him to consider new possibilities for magnet/wire interaction and redesign his apparatus. We have here, writes Gooding, an example of Faraday's 'engagement with the natural world' (the apparent *recalcitrance* of the result demonstrates that engagement).[112] To emphasise Faraday's uncertainty about the appearance that

[110]The experimental maps are developed in ibid., chs. 1 and 6. See also Gooding, 'Putting agency back into experiment'; and idem, 'Mapping experiment as a learning process'.

[111]Gooding, 'Putting agency back into experiment', p. 109.

[112]Gooding, *Experiment and the making of meaning*, p. 144. Pickering writes of a comparable incident: 'Morpugo encountered resistance in the material world. It is important to note that it was a *situated* resistance: it was only because of Morpugo's rigid phenomenal expectations that it counted as a resistance at all. If, for example, he had been prepared to accept that charge was continuously

continuous circular motion, if achievable, might assume, Gooding — drawing on the *Diary* — argues that on the day that motion was produced (3 September 1821), Faraday had at least six possible outcomes to contemplate, although not the outcome that he came in the end to regard as most satisfactory, which he produced on 4 September.[113]

In the event, and after some trouble, Faraday's addition of the crank realised just one of the six possibilities, namely what Faraday construed in his notebook as the rotation of 'the magnetism' (no longer identical with the wire itself) about the wire's axis. He achieved continuous rotation of the crank by repeatedly bringing a magnet close to the wire and withdrawing it as the crank swung by. This motion was unprecedented — an important enough result — and Faraday could have called it a day and ended there. Yet he persisted. His decision to persist — self-evident to us who are familiar with unassisted artefactual rotation — Gooding puts down to intellectual or 'cultural' considerations, and more specifically the belief that human action (in this case bringing a magnet close to a wire and withdrawing it again) did not involve the expenditure of 'natural powers', could not be regarded as a 'natural cause', could not give rise to 'natural motions', and therefore did not lie within what Faraday perceived as the proper domain of his investigations.[114]

Faraday persisted because he was searching for motion that did not depend on human intervention. As I indicated, this he achieved on the day after the crank wire experiment, using an apparatus made up of a cylinder magnet set in a basin of mercury and a section of wire loosely hung above the magnet with its lower tip immersed in the mercury. The upper tip and mercury were connected to the poles of a battery. After various adjustments Faraday got the wire to move unassisted around the magnet.[115] Gooding, in reference to his mapping of the experimental cognitive activity contained in the relevant sections of Faraday's notebooks, writes:

> My account recovers enough of the uncertainty of actual experiment to show that explicit ... hypotheses did not lie behind 'decisions', that more was required than blind tinkering and, therefore, that Faraday's 'success' requires a quite different explanation. This must recognise the emergence of reflexive understanding of the parameters and outcomes of his own agency in a changing experimental situation. Faraday built up a model of what he was doing: this included a physical setup, a set of phenomenological possibilities, a set of actions and of course a record of their outcomes. Initially, *all four were*

divisible, there would have been no obstacle to overcome. But within the space of Morpugo's phenomenal expectations the material world did resist him' ('Living in the material world', p. 288).

[113]Gooding, *Experiment and the making of meaning*, pp. 146 f.

[114]See ibid., pp. 149 and 152.

[115]See Faraday's account, reproduced in ibid., p. 123.

> *variable*. At some point this model became complex and stable enough to specify a configuration of materials and operations that does in fact produce continuous motion of a current-carrying wire about a magnet.[116]

Linguistic resources contributed to the achievement of *stable interactions* with bits of the world. They also imparted reasoning. *Inferential structure* emerges only as the process of experimentation is retrospectively articulated into a narrative. Initial construals of phenomena are developed and refined to produce stable interactions, or, to use Gooding's term, a *convergence* of representations to their objects as they are disclosed in experimental practice. Convergence (about which more will be said below) is the achieved mutual compatibility of construals of phenomena, models of the operations of the experimental apparatus, and interpretations. It leads to formal scientific narratives which bring about *a separation of representations from their objects*. What I have called the disjunction of language and world is effectively explained away by Gooding as an artefact of the experimental process.

2.2.4

I mentioned briefly in an earlier section that the language responsible for the new images of electromagnetism was in part imported from other fields where its use was already well developed. The language of 'magnetic curves' and 'lines of force', in particular, was introduced by scientists whose interest in electromagnetism overlapped with interests in terrestrial magnetism, geometrical optics, optical fields, mapping, and related topics.[117] When Faraday discovered electromagnetic induction in 1831 he relied on the language of lines, curves, and their images to further explore phenomena of induction. This reliance is particularly evident in his experiments to determine the connection between the motions of an inducing magnet and their effect on the needle of a current detector. Here, Gooding argues, Faraday used the lines as a framework in terms of which to make sense of his interventions in the magnetic space surrounding his apparatus.[118] At this early stage the lines were 'merely representations' (as Faraday himself stated), not a physical theory. However, beginning in 1852, Faraday was to argue that the lines were physically real. One of his main arguments was that some phenomena could be produced only by methods that presupposed the existence of the lines. By this late stage, concludes Gooding, the phenomena without the lines were *unintelligible*.[119]

[116]Ibid., p. 160.

[117]Ibid., pp. 101 f.

[118]Ibid., p. 108.

[119]Ibid., p. 108.

In other words, the method of representation with which Faraday structured the space in which he produced observations affected his conceptualisation of that space. Observational techniques exerted influences that were *cognitively* significant. Gooding expresses this shaping of conceptualisation by representation in broader terms:

> Space ceased to be the ubiquitous backdrop in which actions are possible. Experimenters' space became a framework of action, necessary to the description that '*X* is what you *do* in order to get the effect *Y*'. Having become necessary to the description of *X*, it then became part of the explanation '*why* you get *Y* when you do *X*'. The representational significance of space ... would become self-reinforcing.[120]

Over time the representational significance of language would go in the *opposite direction* (from integral to the phenomenon to mere description):

> As ... practices became increasingly familiar, so they became less visible. This development helped to disguise the dependence of the observation language on ... presuppositions about nature, making the language appear independent of what it described.[121]

Implicit in *Experiment and the making of meaning* is a constructivist conception of language. In my present account of the book I have drawn attention to arguments that support that conception. The argument above, on the cognitive significance of observational techniques, shows how experimenters actively engage nature by language and other means, and that emerging conceptions of what nature is like are structured by these resources. Gooding's emphasis on the practical, constructive, and cooperative aspects of sense-making in low-level (pre-theoretical) experimentation has no equivalent in traditional philosophy of science, where the emphasis is on theoretical, descriptive, and individualistic aspects of sense-making.[122] Take, as a first example, Kuhn's expression of the dualist conception of language, cited in Part 1:

[120]Ibid., p. 113. Cf. Pickering on the constitution of conceptual practice by material practice, 'Living in the material world', p. 285.

[121]Gooding, 'Empiricism in practice', p. 48.

[122]However, in the following unelaborated remark, Kuhn does seem to echo Gooding's position: 'observation and conceptualisation, fact and assimilation to theory, are inseparably linked in discovery' (*The structure of scientific revolutions*, p. 55). So does Bloor's equally unelaborated remark: 'The point is that a theory that postulates a single, unique relationship between language and the world will never come to terms with the subtle involvement of language and life' (*Wittgenstein*, p. 23). On this see Gooding, 'Experiment and concept formation'. Hackmann argues that instrument design — invariably represented as subordinate to theoretical demands — has not always been led by theory: 'The empiricism of eighteenth-century experimental science meant that the development of scientific instruments influenced the formulation of new concepts; a two-way process for new theory also affected instrument design' ('The relationship between concepts and instrument design', p. 205).

In the early stages of the development of any science different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways.[123]

Contrast the situation envisaged by Kuhn with the early stages of the development of electromagnetism outlined above. No indication in Kuhn's statement that phenomena (natural facts) might be communally negotiated from the start. No indication either that phenomena *could* be similarly 'described' by different experimenters at an early stage. In Kuhn's view, genuine similarity of description must await the arrival of a paradigm.

The primacy of theory is apparent elsewhere in philosophy. Popper (by his own admission deeply moved by Einstein's theory of relativity, which Einstein made convincing almost entirely independently of any experimental work) writes:

> I should have emphasised that ... observation statements and statements of experimental results, are always *interpretations* of the facts observed; that they are *interpretations in the light of theories.*[124]
>
> The aim of the scientist ... is to invent more and more powerful searchlights [better theories] ... thereby leading us to, and illuminating for us, ever new experiences.[125]

Similarly Quine states (notice the disjunction of conceptualisation and observation):

> Physical theory is indeed uncannily successful in the corroborations that it predicts and in the power over nature that it confers, but even so it is ninety-nine parts conceptualisation to one part observation.[126]

[123]Kuhn, *The structure of scientific revolutions*, p. 17.

[124]Popper, *The logic of scientific discovery*, p. 107, n. *3. For a criticism of this statement, see Hacking, *Representing and intervening*, p. 155.

[125]Popper, *Conjectures and refutations*, p. 361. Cf. Putnam: 'What Popper consistently fails to see is that *practice is primary*: ideas are not just an end in themselves ... nor is the selection of ideas to "criticise" just an end in itself. The primary importance of ideas is that they guide practice, that they structure whole forms of life' ('The "corroboration" of theories', p. 78). See, moreover, Ackerman, *Data, instruments and theory*, who argues the view that the primary task of the scientist is to interpret data in the light of theory and to revise theory in the light of interpretation.

[126]Quine, *Theories and things*, p. 97. Also, according to Quine, the existence of indeterminacy implies that scientific terms are meaningless and denotationless *except relative to their own theoretical framework*, for it is only within such a framework that their meanings are specified; given this, it follows that there is no intertheoretical reference, and that truth is immanent to the conceptual scheme. (For an exposition of Quine's views on this matter, see Field, 'Quine and the correspondence theory'.) Although Quine may want to admit certain possibilities of translation and interpretation between theories, he considers that it makes no sense at all to say that the terms of a theory denote or signify

Feyerabend has much the same to say:

> Facts and theories are much more intimately connected than is admitted ... Not only is the description of *every single fact* dependent on some theory ... there also exist facts which cannot be unearthed except with the help of alternatives to the theory to be tested.[127]

The conception of science as a theory-intensive, theory-led, and epistemologically solitary activity goes hand in hand with a specific conception of language. If, as Feyerabend says, 'the description of every single fact' is dependent on some theory, then the language of description will be seen to derive from theory rather than exploratory experimentation. Language according to dualism, by nature disjoint from the world, is affiliated most closely with theory, and for most dualists is little more than a theoretically guided tool for saying how things *are*. Contrast this to the conception of language that goes with the activity of construing, so important to Gooding's analysis. Construing, which when successful produces communicable representations of novel observations, involves the creative deployment and development of language combined with other resources and skills. Scientists construe and reconstrue their experience in the course of experiment and in the light of what other observers take theirs to be. Construing is thus a creative and inherently social activity to which observers' agency is central: if a construal is going to make any sense beyond the short-term, other experimenters must be able to arrange for and see similarities between this construal and the world of their experience.

Language in the context of construing, far from being curtailed in its role and content by theory, enables experimenters to engage pre-theoretically with the world in order to make sense of and generalise their experience. Biot verbally related a number of discrete needle positions which he had obtained by manipulating his apparatus over time into a textual instant suggesting continuous circular motion of the needle around the wire. Language enabled him to summarise and purify discrete needle positions into circles. He compressed, as Gooding says, 'successive positions of needles obtained in a lengthy sequence of operations into a single image which ... then functioned as an heuristic for further manipulation'.[128] The verbally enabled and induced image of an

anything outside of the theory. Gooding's argument demonstrates, however, that Quine's claim that scientific terms are meaningless and denotationless except relative to their own theoretical framework is *exaggerated*. It overemphasises sense-giving by theory at the expense of sense-giving by practice.

[127]Feyerabend, *Against method*, p. 27, emphasis added; see also p. 116. For a criticism of these statements, see Hacking, *Representing and intervening*, p. 174.

[128]Gooding, *Experiment and the making of meaning*, p. 125. Cf. idem, 'Putting agency back into experiment', p. 92. Holmes writes: 'In some circumstances ... the writing of scientific papers defines,

uninterrupted circle interrelated needle positions that had previously been chaotic and unintelligible. At the same time it functioned as an argument to bring other experimenters to structure their experience in the same way. Language enabled Biot to keep possibilities in play, reflection upon which could be conducted in *nothing but* language. Thus he reasoned: 'for on dividing in imagination the whole length of the conjunctive wire, into an infinity of segments of a very small altitude, we perceive ...' (and so the impracticable thought experiment unfolds in language).[129] Gooding remarks that Biot 'elicited structure out of chaos' — through patient and arduous experimentation 'his agency in the world produced the structure he observed in it'.[130] Operations with language helped impose order on the physical world. Yet, as Gooding demonstrates, it was not an unprepared, unmanipulated physical world. Language and experimental action went hand in hand.[131]

The constructivist conception of language incorporates the idea of language as one among many *activities* associated with experiment. But it is not an activity that can be reduced to 'declarative' uses of physical inscriptions, words, statements, or other such linguistic elements. Oersted made sense of novel electromagnetic observations by inventing and disseminating procedures. In the course of experiment he created potentially meaningful renditions of experience. Their meaning was 'experimental' too in the sense that it was revisable and was in fact revised, again in the course of experiment. Oersted did not discover ready-made correspondences between things in reality (phenomena or facts) and concepts denoting them, nor was he just confirming correspondences that were theoretically pre-specified. His linguistic resources were not

or redefines, the objectives, the boundaries, and the meaning of the investigations themselves. Lavoisier's paper on respiration, for example, joined two sets of experiments on seemingly different problems — the calcination of mercury and respiration, into one broader inquiry. [Here] we can see Lavoisier knitting together experiments carried out for diverse purposes to create coherent investigations on paper. We can find other examples in which the writing of a scientific paper produced bounded, discrete investigative units by lifting more limited problems out of the investigative stream within which the experiments involved were originally performed' ('Scientific writing and scientific discovery', pp. 226-227).

[129]Quoted in Gooding, *Experiment and the making of meaning*, p. 43.

[130]Ibid., p. 41.

[131]Cf. Woolgar, 'On the alleged distinction between discourse and *praxis*'. On language as an *instrument* see Nersessian, 'Aether', pp. 188-189; Shapin, 'Robert Boyle and mathematics', pp. 51-52; and cf. Wittgenstein: 'Not: "without language we could not communicate with one another" — but for sure: without language we cannot influence other people in such-and-such ways; cannot build roads and machines, etc.' (*Philosophical investigations*, §491).

limited to a descriptively deployed notational system. They enabled him to propose new ways of doing, thinking, and seeing. Like others, he found it necessary to place and locate *himself*, his perspective, in his construals in order to show other observers how to see what he had produced.[132] The words and statements of his report disseminated activity (practical and conceptual), not reference relations between language and world.

The functions of language that have been developed or implicit in Gooding's account of meaning-making in experimentation have been many. At the frontiers of knowledge language is a creative resource, central to an activity that is both conceptual and practical. In relation to manipulations of experimental apparatus it is a tool for building interpretations of what is observed. It is used to order phenomena whose appearance is unruly at first. It aids the construction of idealisations of the behaviour of apparatus, a necessary step toward the resolution of observational perplexity. Because self-satisfaction with interpretations is of little long-term importance, uses of language in a public context assist with the formation of a broader consensus. Experimenters actively develop shareable ways of experiencing phenomena. Language disseminates construals, whose meaning and life-span crucially depend on the success other experimenters have in making phenomena of the same kind visible. Language thus enables the meaning of experiments to transcend the spaces in which they are performed. On occasion novel phenomena will become 'easy to see' and 'perfectly evident'. The language in which they are construed emerges with and presupposes an emergent fluency in the manipulations of material apparatus and acquired observational skills. Not a mere transcription device that is brought to bear on completed observations, language is essential to the earliest stages of experimentation. It enables the creation of phenomena in conjunction with other forms of experimental practice. It furnishes formal reasoning and inferential structure on retrospective accounts. Language, or language-use, cannot be easily or sensibly differentiated from other skills experimenters acquire, for it does not function independently of those other skills. The great deal of know-how needed for stable experiential outcomes of experiments and then arguments which transform these outcomes into empirical evidence, is conjointly practical and linguistic know-how.[133]

[132]See Gooding, *Experiment and the making of meaning*, p. 129.

[133]For more on the powers conferred by language see Bazerman, *Shaping written knowledge*, pp. 291 f.; Cantor, 'The rhetoric of experiment'; Gooding, 'Empiricism in practice', pp. 56, 60, and 67 (on Faraday's contribution of the 'language of observables'); Latour, 'Visualization and cognition', pp. 14 f. and 20-22; Ong, *Orality and literacy*, ch. 4; and Rosner, 'Eighteenth-century medical education'.

Table 2, below, is the closest that Gooding comes in *Experiment and the making of meaning* to a formal classification of language and its functions. Here Gooding distinguishes six kinds of *reconstructive activity*, beginning at the lowest level with 'cognitive' reconstruction in real-time experimentation (at this level scientists seek to produce and communicate accounts of what is going on). The highest level of reconstruction is the domain of philosophers who reflect on the narrative products of levels 3, 4, and 5 (which are not laboratory-based), and bring scientists' deliberations into line with their theories of scientific method.

Table 2. Gooding's table showing six levels of reconstruction. Functions of language associated with each level remain implicit in the column entries.[134]

	Activity	*Narrative*	*Enables*
1. COGNITIVE (real-time, non-linear)	constructive, creative, reasoning	notebook, sketches, letters	representation, communication, argument
2. DEMONSTRATIVE (real-time, non-linear)	reasoning, argument	drafts of papers and letters	ordering, description. demonstration
3. METHODOLOGICAL (retrospective and linear)	demonstration	research papers, monographs	communication, criticism, persuasion, reconstructions 4,5,6
4. RHETORICAL (prospective and linear)	demonstration	papers, treatises	persuasion, dissemination
5. DIDACTIC (prospective and linear)	exposition	textbook, treatise	dissemination of exemplars
6. PHILOSOPHICAL (linear)	reconstruction	—	logical idealization

Gooding favours the term 'reconstruction' because its sense preserves (at levels 1 and 2 especially) the reflective and recursive nature of science (we saw that as Faraday considered the outcomes of some of his later moves the meaning of earlier steps changed), and also the requirement that scientists acknowledge narrative traditions (at Gooding's 'methodological' level evidential argument is made to conform to the conventions of a particular experimental discourse).

Nevertheless, the term 'reconstruction' has an unwanted connotation, suggesting the existence of basic or 'authentic' outcomes of scientific inquiry that have been re-cast

[134]Gooding, *Experiment and the making of meaning*, p. 7.

(re-constructed) in language and thereby 'distorted' to a greater or lesser degree. It is more accurate from the point of view developed in this thesis to talk of levels of *construction*. Language is active at the furthest observational frontiers where world A is shaped — it is not limited to reconstructions of that which is already shaped.[135]

The studies of Shapin and Schaffer, Latour and Woolgar, and Gilbert and Mulkay, summarised on p. 122, elucidate constructive functions of language associated with Gooding's four *highest* — and 'linear' — levels of reconstructive activity.[136] What about functions of language at the observational frontiers? In Table 3, below, I present a classification of constructive functions of language based on five 'spaces' that Gooding sought to take into account with his aforementioned maps of cognitive activity.[137] The purpose of this new classification is to elucidate interconnecting and overlapping functions of language associated with Gooding's two 'non-linear' levels of reconstruct-ion, closest to the interface with world B. (Dualist philosophers would, of course, regard these two levels as best exemplifying the language/world disjunction and the bridging function of language.) The classification is incomplete: more language-focused studies of scientific activity at the laboratory bench are required before a comprehensive understanding of functions of language in science is possible. However, it is unlikely that the proposed classification will fail to accommodate new findings.

Table 3. Classification of some constructive functions of language present in the case studies in *Experiment and the making of meaning*, using the five 'inter-connected spaces' over which Gooding's maps of cognitive activity range.

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1. PHYSICAL CONSTRUCTION
[Functions associated with physical (laboratory) spaces in which observations are fashioned]

- *language as archive*: notebooks mediate action, perception, and memory, recording construals that can be tried against both present and previous experience

- *language as physical environment*: in the early stages of exploratory experimentation language functions to articulate scientists' expectations *relative to an experimental situation* which is itself new

[135]Holmes writes that a common aim among historians of science has been to reveal the actual historical investigations that lie hidden behind published papers. If, however, *the writing of papers is itself part of the investigation*, then that is not an accurate way to express the situation. See 'Scientific writing and scientific discovery', pp. 229 and 235.

[136]Linearity is what is achieved when an author 'irons the reticularities and convolutions out of thought (and action) to make a flat sheet on which a methodologically acceptable pattern can be printed' (Gooding, *Experiment and the making of meaning*, p. 5).

[137]Ibid, p. 16.

Table 3 (cont.)

- *language as mnemonic*: a resource for the production of new relationships in a way that makes it possible to see them — mnemonics have the power to 'order' phenomena

- *language as multi-levelled tool*: doing an experiment involves an accumulation of understanding about what is going on — scientists must interpret their own behaviour at the same time as they interpret the character of phenomena

- *language as framework*: the magnetic lines became the framework in terms of which Faraday made sense of his own manipulations in the magnetically active space surrounding his magnets

- *language as inter-dimensional tool*: Faraday was supposed to describe for himself in two dimensions an interpretation of something that was supposed to happen in three, and in real time

- *language as coagulator of phenomena*: scientists translate private construals of experiments first into a more durable, recoverable form, then into a public context — the more firmly language embeds construed phenomena in a model, the more their plasticity is reduced

- *language as tool for the creation of correspondence*: construing experience to create the correspondence of representations to experiences — language interweaves a sequence of exploratory moves with their associated outcomes

2. COMPUTATIONAL CONSTRUCTION
[Functions associated with computational spaces in which analytical procedures are carried out]

- *language as elementary formulaic system*: making a novel observation involves solving problems that are not well-formulated in any space (let alone propositional space) at the outset

- *language as elementary inferential system*: inferences may not be conscious and intentional — an inference from temporal sequence to spacial structure may be assisted by language, as well as driven by it

- *language as medium for mathematization of phenomena*: moving from eliciting phenomena to constructing facts that are susceptible or reducible to mathematical procedures

- *language as tool for decomposition and 'resolution' of perception*: reduction of chaotic or complex perceptual experience to orderly visual experience

- *language as medium of abstraction*: moving from the finite set of distinct operations to the infinite set needed to produce the ideal, geometrical image of a continuous circle — expressing a *possibility* suggested by observed or 'actual' behaviours

- *language as framework* (again): language supplies a construal that structures the space in which effects are expected to occur, and with respect to which they will be made visible

- *language as mnemonic* (with law-making and prescriptive potential): making *rules* for interpreting aspects or elements of the phenomena

Table 3 (cont.)

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3. MENTAL CONSTRUCTION
[Functions associated with mental spaces in which exploratory imaging and modelling take place]

- *language as conduit and adaptor of established models*: in an emerging phenomenal domain scientists articulate possibilities implicit in their practices by drawing upon concepts already developed in other contexts

- *language as conduit and adaptor of commonplace concepts*: the iron core was said to have a 'concentrating' effect on the magnetism produced by the current — scientists may draw upon ordinary, familiar images

- *language as intermediary between observation and theory*: uninterpreted traces are interpreted as the tracks of elementary particles (a significant ontology is created out of an insignificant one) — a tool for incorporating results into an existing system of visual and verbal representation

- *language as creator of stable images*: the compression of a temporally extended set of actions into a single instant allowed Biot to order successive positionings of the wire as a continuous 'circular contour' — this fixed a stable image

- *language as grader of stable images*: after Faraday succeeded in getting continuous motion the 'side-to-side' motions retained their actuality but lost their significance: they were demoted in favour of effects that *more closely resembled the one sought*

- *language as tool for testing construals against background knowledge*: creating the *possibility* of a new interpretation of a class of natural phenomena by drawing attention to their anomalous aspects — Faraday, by bringing the phenomenon of unassisted rotation to bear on the nature of electromagnetic forces, gave his results an interpretation which enabled him to engage and confront theory

- *language as thought experiment*: without engaging material procedures, apparatus, or actual phenomena Morpugo's team considered and discarded models of a possible apparatus[138]

- *language as multi-levelled tool* (again): language enables the transition from constructive, exploratory experiment to demonstrative, argument-oriented experimentation — from thinking mostly *through* practice to thinking mainly *about* practice

4. LITERARY CONSTRUCTION
[Functions associated with literary and rhetorical spaces in which observations are reported and put to work in arguments]

- *language as tool for the individuation of experiment*: transforming exploratory into demonstrative experiment requires the subdivision of the exploratory phase into manageable, 'free-standing', and 'logical' segments

- *language as conduit and adaptor of conventions*: experiments are recorded and described (and reduced from four- to two-dimensional space) with a set of concepts embodying established representational conventions

[138]See ibid., p. 196.

Table 3 (cont.)

- *language as repository of narrative structure and reasoning*: writing the structure of evidential argument into experimental narratives; translating reticular, dynamical interactions with the world into the linear order of experimental narratives — rationales for action often emerge as the account unfolds

- *language as grader of evidential status*: the evidential status of a result changes with each pass or reconstruction

- *language as objectifier of knowledge*: the world of experience and the world of words are made independent as human agency is written out of experimental narratives — language *demodalises* factual claims as references to persons and places are dropped from scientists' talk and facts or laws are made context-free

5. SOCIAL CONSTRUCTION
[Functions associated with social spaces in which observers negotiate interpretations of each others' actions]

- *language as moderator of the private/public distinction*: experimental uncertainty or disagreement with other experimenters allows for the reconstruction and reinterpretation of one's own experience

- *language as link between experience and competence*: the evidential status of results depends on negotiations about the quality of the setup and competence of experimentalists

- *language as measure of understanding*: a scientist must elicit responses from other people so as to tell whether they have grasped what he or she intends them to — only then can outcomes (such as images and devices) be *disseminated*

- *language as pathway to veracity and fidelity*: the potential of language to bring about trust in scientists' honesty and accuracy in reporting their experience (and therefore in the experience itself)

- *language as currency of an economy that depends on the division of labour*: the fixing of an image obviates the need for other observers to acquire the skills embodied in it

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In this five-fold classification we find descriptions of functions of language that take us all the way from world A at the interface with world B, to world A as it appears in published reports. In the course of this journey language becomes involved in 'producing phenomenal possibilities; inventing ways of making them into intelligible, shared experiences; constructing interpretations which make them relevant to theories; and verifying and demonstrating this relevance by reconstructing the activity as the brainchild of hypotheses or theories'.[139] Table 4, below, provides an illustration of the earlier stages of this sequence.

[139]Ibid., p. 141.

Table 4. Adaptation of Gooding's abstraction of a sequence of material/conceptual operations from the first page of Faraday's laboratory notes for 3 September 1821. Functions of language involved with each move appear in brackets as references to the classification of Table 3.[140]

.	*set up* apparatus to explore wire/needle interaction	[1]
.	*query* battery configuration	[3]
.	*set up* reference frame to record positions of wire and needle	[1, 2]
.	*recall/invent* representation of wire	[2, 3]
.	*record* position of wire	[1, 4]
.	*plan* exploration of region of wire	[2, 3]
.	*ascertain* needle behaviour	[1, 4]
.	*recall/invent* representation of needle behaviour	[2, 3]
.	*record* needle behaviour	[1, 4]
.	*repeat/re-examine pre*vious sequence	[1, 3]
.	*record* needle behaviour (side view and top view)	[1, 4]
.	*transform* — represent effects as wire positions instead of needle positions	[2, 3, 4]
.	*record* effects as wire positions	[3, 4]
.	*construct* new possibilities — 'indicated' by needle movements	[3]
.	*imagine* continuous motion of wire perpendicular to fixed needle	[3]
.	(...etc.)	

Even though exploratory and incomplete, tables 3 and 4 demonstrate above all else that the 'referential' function of language is *completely insignificant* in science (it is a rhetorical, derivative function).[141] This is important because if world B is to be under-

[140]Adapted from ibid., Table 6.3, p. 140.

[141]In Goodman's *Ways of worldmaking*, we find a sketchy and unelaborated classification of functions of language that is in the spirit of Table 3. It is four-fold: **(i) synthesis and analysis of entities**: 'Much but by no means all worldmaking consists of taking apart and putting together, often conjointly: on the one hand, of dividing wholes into parts and partitioning kinds into subspecies, analysing complexes into component features, drawing distinctions; on the other hand, of composing wholes and kinds out of parts and members and subclasses, combining features into complexes, and making connections' (p. 7); **(ii) sorting into relevant and irrelevant kinds**: 'Some relevant

stood as a source of 'resistances' (as I think it should), my position invites the following (dualist) rebuke: even if we accept that language contaminates resistances in the process of referring to them, who is to say that there are no things-in-themselves beyond those inputs that we are simply unable to refer to directly?

The first clause of this question is based on a factual error: laboratory practice reveals that *language is never used to refer to inputs*, nor is it employed in the vain hope of establishing reference to ontologies beyond world A. Table 3 underpins the conclusion that 'the results scientists present as bold, self-evident facts about nature have been painstakingly elicited, shaped, and made visible through the invention of methods of observation and representation'.[142] Language does not, as a matter of fact, 'refer' to world A or in any way reach out to world A — *language is firmly part of that very world.* In other words, dualism, which forges a disjunction of language and world and reimposes a connection that is referential (see the drawings of Table 1), is refuted.

2.2.5

Phenomena, natural facts, and their effects have 'no fixed, essential nature that can be assessed independently of the manipulations that [the construals of scientists] construe'.[143] At the same time they are not mere fictions. Gooding takes his analysis of the experimental making of meaning to support a special kind of realism — as he puts it 'a modestly realistic interpretation of what representations are about'.[144] I shall try to explain what this non-traditional realism amounts to, and demonstrate its irreducibility to the dualist conception of language.

Gooding's discussion of realism proceeds from his aforementioned notion of the 'convergence' of the main features of the experimenter's space. After Faraday

kinds of the one world, rather than being absent from the other, are present as irrelevant kinds; some differences among worlds are not so much in entities comprised as in emphasis or accent, and these differences are no less consequential' (p. 11); **(iii) deleting and supplementing**: 'The scientist [goes about] rejecting or purifying most of the entities and events of the world of ordinary things while generating quantities of filling for curves suggested by sparse data, and erecting elaborate structures on the basis of meagre observations. Thus does he strive to build a world conforming to his chosen concepts and obeying his universal laws' (p. 15); **(iv) deforming or reshaping**: 'The physicist smooths out the simplest rough curve that fits all his data' (p. 16). Needless to say, Goodman did not set out to produce a classification of functions of language.

[142]Gooding, 'Thought in action', p. 139.

[143]Gooding, *Experiment and the making of meaning*, p. 186.

[144]Ibid., p. 36; cf. 'a realistic role for *experiment*', p. 165.

succeeded in realising continuous assisted rotation of the crank wire, he was faced with the task of building upon three related elements in his experience: (i) practical knowledge of the experimental conditions under which the crank would swing past the magnet; (ii) mental models of that phenomenon and of the phenomenon of unassisted rotation he sought; and (iii) a mental model of the apparatus that could realise unassisted rotation. By 4 September 1821 these elements had been developed and had *converged* with the desired effect. Faraday had achieved mutual compatibility of his construals of phenomena, his models of the manipulations and workings of (actual and conjectural) experimental apparatus, and his observations and interpretations.[145]

According to Gooding's argument, Faraday's pursuit of circular 'natural motion' shows that convergence is not easily achieved (and that it needn't come at all, or may be quickly dissolved), and is always the result of revisions of construals and material arrangements, and accumulation of theoretical and observational skills, in the course of making experience intelligible. Experience begins to become intelligible when models and practices yield phenomena that are stable and reproducible and satisfy tentative expectations about outcomes.

Convergence in Faraday's experiments of September 1821 came when he achieved rotation of a crank wire while moving a magnet to and fro. The result suggested ways of simplifying the configuration of wire and magnet and producing an orderly, seemingly non-artefactual natural phenomenon.[146] It also clarified the reasons behind earlier unexpected phenomena, such as the continuous sideways motion of the wire, and inspired a tentative distinction between electric current and conducting matter. Is it reasonable to believe that when convergence occurs scientists possibly *learn* something about the way the world is? Does the term 'convergence' imply that experimentation is

[145]Gooding's construals, models, and interpretations correspond closely to Pickering's own triumvirate of material procedures, instrumental models, and phenomenal models: 'By material procedure, I refer to experimental action in the material world: setting up the apparatus, running and monitoring it in the laboratory. The instrumental model expresses the experimenter's conceptual understanding of how the apparatus functions, and is central to the design, performance and interpretation of the experiment ... The third element, which endows experimental findings with meaning and significance, is a phenomenal model, a conceptual understanding of whatever aspect of the phenomenal world is under investigation' ('Living in the material world', p. 277). For fact-production to occur, the three must hang together and reinforce one another. Pickering's term for this achieved relation is '*coherence*' (for an example, see ibid., pp. 286-287).

[146]See Faraday's compact rotation device in *Experiment and the making of meaning*, p. 152. For other rotation apparatuses see Gooding, 'In Nature's school', p. 121.

a process converging *to* something besides a certain sense of order, reproducibility, and control? What room, if any, is there here for realism?

Gooding's response is in two parts. First, he argues that Faraday's systematic reconstruction of his experience, conceptual and practical, clearly shows that he engaged factors beyond his control. His command over his apparatus, his construals and expectations only partially determined his experience. Emerging interpretations were a consequence of a continuing engagement with nature.[147] For Gooding this much realism or empiricism is inescapable. Contact with nature is demonstrable early on in the lives of theories, in the course of what might be called a 'proto-theoretical' stage. It is manifested in the recalcitrance of nature to be manipulated and construed in certain ways.[148]

Gooding is keen to point out, however, that engagement with nature need not compel assent about empirical matters. Both Faraday and Ampère, for example, engaged nature but were led to different theoretical interpretations. As for the *natural facts* that emerge from engagement with nature, Gooding comments on the considerable effort needed, both in the accumulation of skills and the fine-tuning of experimental apparatus, to elicit and distinguish very specific sought-after outcomes from the large number of potential outcomes that engagements with nature reveal:

> During a repetition of [trials with a replica of Faraday's early rotation device] we found it very difficult to obtain rotations even with the 'correct' configuration. We made trial and error adjustments to the depth of immersion of the wire, to balance the effect of the viscosity of the mercury (which inhibits the motion) against the effect of allowing the wire too much freedom of motion. If not immersed deeply enough, it will oscillate like a pendulum.[149]

Gooding distances himself from realists who maintain that engagement occurs when theories are confronted with decisive experimental results, for one can show that experimental results need not be treated as theoretically decisive. At the same time,

[147]Gooding, *Experiment and the making of meaning*, pp. 179-180.

[148]Davy and Biot elicited phenomena by using very similar experimental setups, deriving from Oersted's experiments (ibid., p. 50). Thus, experimenters with different theoretical views, in different parts of the world, elicited very similar novel phenomena which they at first construed in very similar ways. The 'engagement' argument runs as follows: the fact that people in different places with different ideas using different languages can elicit similar phenomena (and construe them similarly) shows that there is a factor, nature, that is independent of their cultural circumstances and explains (through its pre-given make-up) the coincidence of their accounts (for more on this argument, see section 2.3.3 of this thesis).

[149]Gooding, *Experiment and the making of meaning*, p. 153.

Gooding maintains, one can hardly deny that empirical constraints do often operate decisively at the level of (what I have called) proto-theoretical experimentation. The empiricists' desire to locate engagement with nature in corroboration or falsification has meant a neglect of its presence in the early processes of articulation: 'the empirical basis that really constrains theories is found in experimentation itself, not in observation reports of experimental results'.[150] Rhetoric and traditions of narrative writing are responsible for the placement (or misplacement) of the empirical base in observation reports, and for the notion of an unmediated compulsory force of nature.

Having thus indicated and secured a place for empirical constraints in his scheme, Gooding goes on to attack the realist metaphor of gradual approximation of theories to reality, or convergent realism.[151] He argues that the metaphor of gradual approximation derives from the consideration of polished scientific accounts — a metaphor, in Kuhn's words, 'conditioned by science texts' — in which the testing and matching of predicted and observed values has been given rhetorical priority, and from which most of the process of developing and defending theories has been edited out.[152] In other words (and in the context of Part 2 this is becoming a familiar theme), the appearance of correspondence of words to things, and of the convergence of theoretical

[150]Ibid., p. 193. Herein lies the difference between Gooding's account and Quine's 'ontological relativity'. In 'Two dogmas of empiricism' (*From a logical point of view*, pp. 20-46), Quine talks of the raw experience upon which we impose order by means of a conceptual scheme. Our conceptual scheme must accommodate this experience, but is under-determined by it. We impose order on raw experience by cutting it up into objects. There are different ways of doing this, different objects that can be posited. In ordinary common sense we posit physical objects — but even here we can conceive of alternative schemes, which cut experience up into different objects — rabbits, rabbit stages, rabbit-bushes, etc. And of course we might choose to posit other objects: atoms, sense-data, numbers, etc., in addition, or instead of the ordinary objects of our conceptual scheme. Which theory (which ontology) we choose depends, for Quine, *solely on the explanatory value its objects hold for us*. This emphasis on explanation (and theory) completely fails to acknowledge Gooding's level of practice (and direct constraint). Putnam approaches the matter from the same angle, with a similar result: 'Internalism does not deny that there are experiential inputs to knowledge; knowledge is not a story with no constraints except *internal* coherence; but it does deny that there are any inputs *which are not themselves to some extent shaped by our concepts*, by the vocabulary we use to report and describe them, or any inputs which admit of only one description, independent of all *conceptual choices*. ... The very inputs upon which our knowledge is based are conceptually contaminated' (*Reason, truth and history*, p. 54).

[151]For two varieties of convergent realism, see *Experiment and the making of meaning*, p. 185.

[152]Ibid., pp. 180-184.

propositions on real structures, is a *made image* that has had a powerful but unwarranted influence in philosophy. Gooding recommends that convergent realism be replaced by 'asymptotic realism', an image meant to preserve (as I explain below) the central thesis of his analysis, viz. that what is disclosed about nature is made, not discovered ready-made. Alternative pathways in Faraday's researches, defined by different sets of preferences and different end results, and evident in Gooding's maps of experimental cognitive practice, demonstrate that practices do not converge (as convergent realists might wish) to an outcome that is given, or 'there' to be discovered.[153]

Gooding depicts asymptotic realism as the approach of two curves, *a* and *b*, having the *x,y* coordinates of a Cartesian system as asymptotes (in his diagram they converge upwards along either side of the positive *y* values). Curve *a* is the curve of representations and theoretical practice; *b* is the curve of phenomena *that scientists make stable* (and seemingly real) by manipulating and construing nature (both curves belong to world A).[154] The curves represent different aspects of the engagements of human agency with nature (world B), even though each one is a compound of both agency and recalcitrant nature. Their mutual embodiment of empirical constraints means that they need not (and most often do not) converge dramatically in practice. But when they do converge, a very real psychological sense of correspondence is inevitable, which then finds expression in conventional objectifying rhetoric (discussed in Part 2.1). Gooding thus displaces the 'metaphysical convergence' of convergent realism with his own notion of procedural (hands on, psychological/rhetorical) convergence. As he explains,

> when the mutual approach becomes sufficiently close and sufficiently enduring, convergence is taken to be correspondence between representations in the conceptual world to real things in the material world.[155]

[153]See ibid., p. 154.

[154]Pickering writes: 'The interaction between the *instrumental and phenomenal images* ... hints at a potential anarchy in the empirical base of science, with individual experimenters free to produce whatever data suit their fancy. One can, however, see that such disquiet is unnecessary, if one is willing to regard science as a social enterprise. Experimenters have to *argue* for acceptance of their results within their community, and when they do, the instrumental and phenomenal images are part of the currency of exchange. Experiments are performed and evaluated within a socially sustained matrix of commitments — beliefs and practices — and this matrix severely constrains the acceptable constructions an experimenter can place upon his esoteric experience. The empirical base of science is 'softer' than it is often conceived to be, but it is not anarchic' ('The hunting of the quark', p. 235).

[155]*Experiment and the making of meaning*, p. 187. Cf. 'asymptotic convergence' in Honner, *The description of nature*, p. 21.

It is at that point, in other words, that practicing scientists become satisfied that their talk and thought has engaged the world and revealed pre-existing structures. Phenomena that were at first made 'easy to see' and then 'evident' finally are seen to exist 'out there'. The role of human intervention is gradually removed from accounts altogether, and phenomena appear as independently given.[156] Correspondence is the psychological and rhetorical upshot of the mutual approach of the products of experimental practice. But although the correspondence of representations to entities and properties is a made relationship, *it is not arbitrary*. It is grounded in the resistances of practice and constrained by the mutual pursuit of consensus.

Gooding's asymptotic realism thus accounts for an *appearance* of language/world fit without relying on the language/world disjunction that convergent realism presupposes. But is asymptotic realism really *realism*? Gooding argues that the direct and analysable relation between world A and world B is one of made convergence, not natural correspondence. A realistic role for experiment is secured by the empirical constraints to which the processes of experimentation are subject. Theory, with its origins in experiment, is a product of these constraints despite the fact that it is empirically underdetermined, and despite the fact that after a certain point in its development theory may take on or seem to take on a life of its own. Are Gooding's empirical constraints alone sufficient grounds for a realist philosophy of science?

In one respect this is just a matter of definition. If what is understood by realism is the view that theories or existence claims involving theoretical entities are true or false, and that that depends on whether they correspond to facts about the world (the ink-blot metaphor), then Gooding is not expounding realism. But if realism is understood as the view that scientists' use of cultural resources *does not undermine* — and is necessary to make possible — their use of nature (the blooming desert metaphor), then Gooding is indeed expounding realism.[157]

It could be said, however, that Gooding's notion of nature is too vague. If asymptotic realism does not posit a fixed reality what *does* it posit? Even weak versions of familiar realism, such as Hacking's realism about entities, tend to specify a kind of reality that they posit. Hacking's belief that 'nothing ... in the realist attitude ... demands that

[156]On the risks associated with the rhetoric of 'high externality' see Pinch, 'Towards an analysis of scientific observation', pp. 23 f.

[157]Gooding seems to accept a distinction between nature and culture. However, Shapin, Latour and Woolgar, Gooding himself, and others have argued that the distinction is rhetorically constructed.

there is a humanly expressible, uniquely richest statement of how the world is',[158] does not prevent him from arguing that at least some postulated unobservable entities, such as electrons, do exist (and exist independently of the fact that we believe they do).[159] So what if anything does Gooding posit? In the article on novel observations mentioned at the beginning of this section Gooding writes that his account

> restores content to the scientific realists' claim that interpretations and theories *are* about aspects of the natural world. They are not merely projections of the images, categories and interests that shape observational activity.[160]

Yet if those 'aspects' of the natural world that theories are 'about' are specified in terms of 'categories' that are not just projections of social contingencies, should we believe that at least some of the categories are natural, not just apparently natural, just as Hacking believes that electrons (which could be said to be a category of matter) are natural, not just apparently natural? Is Gooding *positing something* with the above remark?

If Gooding's account is to stay intact the answer must be, in part, no. Recall that the depiction of his brand of realism as two curves converging asymptotically has no separate 'reality' component (the x,y asymptotes have a diagrammatic function only). There is no room for natural categories distinct from products of human agency operating under empirical constraints. Any such distinction would necessarily mean that we are dealing with convergent realism in disguise. To think that Gooding ought to specify nature more concretely than he does for his viewpoint to qualify as realist is to misunderstand, I believe, what his arguments argue for and what they rule out. To begin with, Gooding *does* 'posit' a reality, only one that is constructed, not fixed. It is precisely the reality disclosed in the course of scientists' interactions with the uninterpreted world (the blooming desert). It is what scientific disclosure of nature *amounts* to. This kind of disclosure should be distinguished from the kind which includes, for example, the disclosure of the next mountain to the bear who has just reached the top of this one (Gooding's example) — although even here the 'fixed reality' is viewed from *somewhere*.[161] To take another terrestrial illustration (this time from Hacking):

[158] Hacking, 'The participant irrealist', p. 290.

[159] Hacking, *Representing and intervening*, pp. 21-31. On the existence of 'real mind-independent distinctions in the real world', see ibid., p. 95.

[160] Gooding, 'How do scientists reach agreement', p. 230.

[161] Gooding, *Experiment and the making of meaning*, p. 187.

> Consider a device for low-flying jet planes, laden with nuclear weapons, skimming a few dozen yards from the surface of the earth in order to evade radar detection. The vertical and horizontal scale are both of interest to the pilot who needs both to see a few hundred feet down and miles and miles away. The visual information is digitized, processed, and cast on a head-up display on the windscreen. The distances are condensed and the altitude is expanded. Does the pilot *see* the terrain?[162]

Hacking believes the answer is yes. This case is not, after all, 'one in which the pilot could have seen the terrain by getting off the plane and taking a good look'.[163] With so much landscape to see at such great speed there is no way *to* see but with an instrument. Likewise, Gooding may be understood as saying that there is no way of scientifically viewing nature but by 'constructively' engaging with it. It is not as if scientists could step out of their brains, bodies, and cultures and take a good look.

Gooding's arguments effectively differentiate his position from convergent and other varieties of realism in one more respect. They rule out the dualist conception of language.[164] *Experiment and the making of meaning* expounds the view that theories are developed in the course of scientists' interactions with the world and with each other — theories are the products of those interactions. Traditional (metaphysical) aboutness has no place in this account: Gooding rejects the 'received philosophy of scientific theories ... [which reifies] the distinction between words and the world they describe'.[165] On the other hand, dualism and traditional realism are interdependent. The complementarity is especially evident in the case of convergent realism, which could be depicted as a trajectory of theories or representations gradually approaching a baseline curve that represents the world. Language in this picture belongs entirely to

[162]Hacking, *Representing and intervening*, p. 207, emphasis added.

[163]Ibid.

[164]This is not a point that Gooding formulates concretely, which may account for loose dualistic expressions in his own writings. The passage quoted above, where Gooding talks about restoring content to the view that theories are 'about' aspects of the world, is an example. Talk of this kind is dualist because it contains: (i) the assumption that theories (expressed in language) either are *about* aspects of the world or they are not — either they identify actual properties of the world or they fail to do so — and (ii) a suggested theory/world disjunction. For other examples, see Gooding's apparent acceptance of the terms of Quine's distinction between 'stimuli' derived from the world and linguistic glosses of them ('How do scientists reach agreement', p. 209). Consider also his question, 'How do individuals translate novel experience into a communicable form?' (ibid., p. 206), which could be read as, 'How does one *put language to* novel experience?'. The latter question assumes a separation between language and (the object of) experience.

[165]Gooding, *Experiment and the making of meaning*, p. 165.

the trajectory, and has nothing but a referential relation to the baseline curve. Its function is to give expression to evolving theories, and thereby to represent the structure of the world increasingly accurately.

The same holds for other realisms, for example realism about entities. According to Hacking, we know that a good many postulated scientific entities are real because we can intervene and manipulate them in the light of our representations.[166] Language in this scheme belongs to the realm of representations and has nothing to do with the reality of entities. (In *Representing and intervening*, and contrary to the apparent implications of the 'jet plane' story, Hacking at one point seems to say that 'doing' might get us *outside* of representations and language and directly in touch with the real things themselves).[167] By contrast, Gooding's asymptotic realism follows from an account of language that implicitly undercuts the dualist conception. Language in that account is integral to interpretations, construals of phenomena, construals of the apparatus producing them, and all else that the converging curves of asymptotic realism stand for. The language/world disjunction and the language/world aboutness relation appear in Gooding's scheme as secondary effects only, and not as pre-given metaphysical categories.

Thus Gooding does not intend and cannot consistently posit a reality along familiar realist lines and continue to uphold a scheme that is incompatible with the dualist conception of language. Given the constructivist alternative, empirical constraints and nature's recalcitrance as discussed by Gooding are both sufficient and the *only* grounds on which to develop a 'realist' (and anti-anti-realist) philosophy of science. To demand that he posit more than that is to fail to appreciate the conception of language that he is implicitly advocating.

2.2.6

It could be said, however, that no matter how Gooding's conception of language is to be taken, he is still looking to have things both ways by presenting a hybrid of realist and anti-realist views. His experimental maps are intended to show that all natural phenomena are bounded by human activity, activity that is in turn subject to the constraints of practical and material resources. His emphasis on the context of practices is supposed to counteract his emphasis on experimenters' *agency* and 'the implication that "making" or "constructing" phenomena undermines [the capacity of phenomena] to

[166]See Hacking, *Representing and intervening*, p. 31.

[167]Ibid., pp. 273-274.

tell us something about the world that is *not* made by us'.[168] Yet what *could* phenomena tell us about *that* world (world B) in addition to what they already tell us about world A, the world which scientists explore and conceptualise, and which is to some extent of their own making? Gooding's words above suggest that he wants to have phenomena both agent-made and agent-independent.

The objection has no real force. An upshot of (procedural) convergence is that we come to know something about 'the world that is not made by us'. Gooding cannot consistently hold that we can also *really* know something about that world. Known phenomena bounded by human activity rule out the possibility of 'view-from-nowhere' knowledge. Gooding is certainly not an advocate of view-from-nowhere knowledge (despite his occasional contrary remark). Is he, possibly, an advocate of 'view-from-somewhere' knowledge of the unmade? The generous and appropriate answer is deceptively simple: if we *accept* that human involvement in the making of scientific knowledge gives rise to ontologies that are sustained by, and are dependent on that involvement, we can then *choose* to speak whichever way we like about knowing the 'real' world. That is, some will say that within the bounds of that accepted philosophical framework we can indeed speak of knowledge of 'the world not made by us'. Others will say that our role in the creation of ontologies can only mean that we have no such knowledge (and that we should not blur the world A/world B distinction by loose talk). In any case, the choice is in respect of *made* knowledge. So, on the understanding that all knowledge is made knowledge (and *Experiment and the making of meaning* claims no less), Gooding can be excused for saying that, as a result of empirical constraints, we can have knowledge of 'the world that is not made by us'. By contrast, the views examined in Part 1 of this thesis, which did not assume the constructivist conception of language, cannot be allowed to get away with that much.

Hacking is a case in point. He recognises that phenomena are 'elicited', yet draws and accommodates a metaphysical disjunction between scientists' representations (and their content) and the world (and its contents).[169] This leaves intact the problem of perceptual access, that is, the problem of access to both representations and the world for the purpose of assessing their correspondence. In Gooding's account there is no such problem because in principle we have — and in practice historians have had — access to the *activities* and *technologies* that produce correspondence.[170] They are the

[168]Gooding, *Experiment and the making of meaning*, p. 143.

[169]On the 'elicitation' of phenomena, see Hacking, *Representing and intervening*, ch. 13. The disjunction I refer to can be found in ch. 16.

[170]Gooding, *Experiment and the making of meaning*, p. 89.

selfsame activities and technologies that produce construals and the convergence of representations to what are disclosed as their objects in the course of developing and refining construals. In Gooding's account, experiment is a process by which scientists learn whether proposed interpretations of nature do actually engage nature. This learning presupposes practical (including literary) mastery over a material environment.[171] Hence Gooding claims that

> if philosophers want to understand how a scientist or a group of scientists use experiment to make talk refer to the world and to inform scientific argument, they must pay attention to embodied, empirical practice.[172]

Embodied, empirical practice is only rarely the focus of the historical and sociological works examined in Part 2.1. Shapin, for example, has very little to say about 'engagement with nature'. When he writes that 'we account for our language by saying that it flows from reality, even as we construct our versions of reality using linguistic resources',[173] he expresses a view that Gooding would most certainly endorse, but Shapin says little about what the 'linguistic resources' are in practice, or to what uses they are put. His emphasis, instead, is on the legislative powers of language, especially Boyle's use of those powers to make matters of fact (Boyle's 'theatre of persuasion').[174] Shapin emphasises the rhetorical advantages of the language/world metaphor, but he does not explain why the adoption of that metaphor or rhetorical strategy *in specific instances* (when specific bits of language are set back from, and made to refer to, bits of the world) sometimes succeeds and sometimes fails. After all, scientists cannot at whim declare a new item of knowledge to exist 'out there', untainted by language. Shapin and Schaffer together correctly point out that:

> To identify the role of human agency in the making of an item of knowledge is to identify the possibility of its being otherwise. To shift the agency onto natural reality is to stipulate the grounds for universal and irrevocable assent.[175]

Gooding explains why this shift, though artefactual, *is not arbitrary*: it is constrained by empirical/social practices at the laboratory level. Shapin and Schaffer, who do not engage with the nuts and bolts of low-level experimentation, by contrast are not equipped to explain the shift.

[171]Ibid., p. 216.

[172]Ibid., p. 271.

[173]Shapin, 'Robert Boyle and mathematics', p. 52.

[174]For the expression, see Shapin, 'Pump and circumstance', p. 503.

[175]Shapin and Schaffer, *Leviathan and the air-pump*, p. 23.

Latour and Woolgar, who locate their study in a context most suitable to reveal engagement with nature, leave such engagement out of their account — more accurately, they exclude it from their account as illusory. They are keen, instead, to stress the 'artificiality' of the environment of the laboratory. Like all anti-realists they are dualists. To take an example, they claim that once the TRF assay was decided, or settled on, it created the criteria of identity for the substance of interest. It created its own truth. It was self-authenticating, for there was nothing else to authenticate it: 'the bioassay is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance';[176] (yet what would be *like* to independently be given an entity?). This conclusion is typical of anti-realists, who deny that theoretical entities physically exist and say that they are fictions, logical constructions, or parts of an intellectual instrument for reasoning about the world.[177]

The thesis that entities like TRF postulated by theories are at best useful intellectual fictions is as much a result of adherence to the dualist conception of language as the thesis that the entities really do exist independently as described. In both cases the language/world disjunction holds sway, but unlike realists, for whom a direct correspondence relation between language and the world bridges the distinction, anti-realists deny that we can have true access to the physical side of the duality. It is to language and its fabrications (or at most, the phenomena of perception) that true access is limited.[178] Latour's and Woolgar's emphasis on the 'literary' constitution of their laboratory is partly explained by their adherence to dualist presuppositions. Whereas realists need an ontologically ready-made world in which to anchor a corresponding language, anti-realists draw a hermetic boundary between words and sub-phenomenal things, across which no relationship can be known to exist. They see experiments as rhetorical devices and nothing more, in contrast to constructivists like Gooding who will admit, merely, that experimental results gain rhetorical power once they have been situated in arguments. They may be artefactual but they are not fictitious.[179]

Bazerman concentrates on the evolution of literary conventions that have made science public and shaped scientific thought and argument, but he too is operating within the

[176]Latour and Woolgar, *Laboratory life*, p. 64; see also pp. 182-183. For Gooding's criticisms of anti-realism, see *Experiment and the making of meaning*, pp. 210 f.

[177]See Hacking, *Representing and intervening*, p. 27.

[178]Gross writes, on DNA: 'the sense that a molecule of this structure exists at all, the sense of its reality, is an effect only of words, numbers, and pictures judiciously used with persuasive intent' (*The rhetoric of science*, p. 54).

[179]Cf. Gooding, *Experiment and the making of meaning*, p. 143.

dualistic framework. Like Latour and Woolgar, Bazerman is a useful source of arguments for the constructivist conception, but unlike Gooding constructivism is not implicit in his account. To take an example, Bazerman provides a list of authors who have recently articulated reasons for distrusting the direct correspondence, as he calls it, between scientific formulations and nature.[180] It is not the *idea* of correspondence that is questioned by the authors in the list (Bazerman never questions that idea either), but the *clarity* of the correspondence: the common argument is that scientific discourse, as a discourse about nature, is opaque. These authors have argued, for example, that scientific language incorporates basic assumptions about the nature of reality that science simply perpetuates; that scientific formulations embody extraneous ideological components; that scientific language gives science an elevated character and a means of establishing and maintaining its authority; that language is partisan and manipulated for individual gain rather than being an objective representation of nature; that reference to actual events is obscured by such things as the lack of precise replication of results, the importance of inarticulate craft knowledge to produce results, and the selective representation of results. All this seems to leave scientific language 'with no overt means of doing the empirical work which has been considered the work of science', namely '*giving us ... direct access to things in themselves*'.[181] Bazerman believes that recognition of the opacity of language should nevertheless not lead us to disown empirical constraints on what we say. He believes it is possible to specify a unitary concept of language-use that allows social factors in alongside what he considers the properly scientific empirical work of establishing reference to the actual world. Yet he is not inclined to realise this possibility by breaking away from the dualist conception of language.

In this section (and in Table 3 in particular) I have indicated ways in which language is involved in the *construction* of scientific facts and knowledge of facts. I have attempted to show that if we look closely at scientific practice, in historical perspective and at the frontiers of research, the view that there is a physical or phenomenal world distinguishable from language, to which scientific conceptions conform, will reveal itself as illusory. The practice of scientists in world B constructively generates the reality of world A. This reality, as Gooding and authors of laboratory studies have documented, is built from sophisticated measurement devices, chemical reagents and ingredients, specifically bred test-animals and plants, laboratory-specific and other skills, mathematical procedures, negotiations and innovations, and other language-enabled, language-dependent, and language-generating resources, apparatuses, and actions.

[180]Bazerman, *Shaping written knowledge*, pp. 293-295.

[181]Ibid., pp. 294-295, emphasis added.

From the constructivist viewpoint language is a tool that carves out, fixes, bolsters, objectifies, weakens, and demolishes the world's ontologies. While world A is thus constantly refashioned in world B, language is found setting the parameters everywhere, from the chaotic frontiers of observation and communication to the orderly pages of demonstration and reason.

2.3 Realist, anti-realist, and other objections to the proposed conception

In Part 2.1 I looked at arguments supporting the view that the intellectual category of scientific facts, and conventions relating to proper language, persuasive rational narratives, scientific communication, inter-subjectivity and objectivity, etc., have been shaped by historical debate and have not always been self-evident and familiar. Like all conventions they have had a 'metaphysical' component, and an historical and practical contingency. In the same place I looked at an analysis of functions of language in science from a sociological perspective, especially rhetorical functions having to do with the transformation of the factual (or artefactual) status of statements, and the making of instrumental transparency. I also looked at claims concerning the role of 'out-thereness' in scientists' discourse in general, and explanation of belief in particular. Part 2.2 covered Gooding's analysis of the making of meaning in experiment, exploring functions of language closer to the 'empirical basis' than those considered earlier. I argued that Gooding's analysis supports a constructivist conception of language, incompatible with dualism.

Constructivism may be summarised as follows: language manifests itself as a variable resource of skills and actions by means of which scientists communally make sense of their experimental experience and construct their knowledge. Because the empirical basis of science is shaped in the course of pro-active literary engagements with the world, from a philosophical point of view there can be no disjunction of language and the world that it is about. An epistemically *accessed* world (whether of phenomena or natural facts) is necessarily a language-laden world. An epistemically *accessible* world — existing notionally in our imagination — is also a language-laden world insofar as its ontology is specified. Only the 'uninterpreted world' (world B) is not language-laden. That unconstructed world, onto which we project the ontologies of world A after our role in them has become transparent, *together with conventions of scientific practice*, place constraints on our versions of world A, from its minutest concrete details to its most speculative and fanciful aspects. Whereas the dualist strives to bring language to a ready-made world, the constructivist recognises that the world must be made first before language can appear to describe it.

In this section I want to consider likely objections to the constructivist conception of language. I have subsumed them under three categories. First, objections about the appropriateness of the sources of evidence that I claim support the constructivist conception. Here I also defend the clarity and novelty of the conception. Second, objections deriving from familiar varieties of realism. And finally, the category of anti-realist objections. It will be seen that the last two categories overlap slightly as realists and anti-realists attempt to reduce constructivism to one of the traditional language/world positions. To this common end similar arguments are often used.

2.3.1

The objection that constructivism relies on evidence that cannot in principle support it gives expression to the common reservation that a philosophical thesis cannot be supported by historical or sociological evidence. History and philosophy of science must be kept distinct at some level, or so it is said.[182] The two conceptions of language contrasted in this thesis, although philosophical in character, seem to have been evaluated solely empirically, that is, solely in terms of historical and sociological evidence. Because a philosophical evaluation must draw on philosophical considerations, the present evaluation cannot be conclusive.

I do not wish to challenge the view that philosophical argument cannot be reduced to 'empirical' argument. As I mention later in this section, realist philosophers often attribute philosophical beliefs to scientists solely on the basis of non-philosophical beliefs these scientists are found to hold (for example, that atoms exist). Such attributions are unjustified. It is the task of historical or sociological research to reveal what scientists do or do not believe, do or do not do. No philosophical conclusions can follow *directly* from such revelations. Scientists who believe that atoms exist are not necessarily realists about atoms. And, of course, even if it could be established empirically that all scientists *are* realists (that is, converts to a particular philosophical view), that would hardly validate scientific realism — other philosophical theses, like anti-realism or constructivism, would have to be refuted first.

[182]See, for example, Finocchiaro, 'The uses of history in the interpretation of science'; Giere, 'History and philosophy of science'; McMullin, 'History and philosophy of science'; and Wartofsky, 'The relation between philosophy of science and history of science'.

What I do wish to challenge is the claim that the dualist and constructivist conceptions of language have been evaluated only empirically in the course of this thesis. The constructivist conception has been developed to be *consistent* with empirical considerations, not a consequence of them. The aim has been to develop a conception of language in science for which it is *not* an axiom that language functions to be about aspects of an independent physical/phenomenal world, and *not* an entailment that scientists use language to establish reference to such aspects. Constructivism is, of course, underdetermined by facts that I have cited in its support. Unlike dualism, however, what it entails about functions of language (by design, if you will) is not contradicted by facts of an historical or sociological nature. Philosophers have regularly sought to support their epistemological views by demonstrating their compatibility with historical accounts of science. Others have used historical accounts to refute philosophical theories of science.[183]

In Part 2.2 I argued that Gooding's asymptotic realism presupposes the constructivist conception of language and is incompatible with dualism. His qualified realism is developed in the course of an historical analysis, but it is also an analysis that seeks to refute the philosophical thesis of epistemological individualism.[184] Gooding reveals the social factors on which the making of meaning in the course of experiment depends, social factors that are given no significant role to play in the works of epistemological individualists. There is just one credible way of refuting epistemological individualism, and that is to show that it is incompatible with what we know about the making of scientific knowledge. Having dealt with this problem Gooding can then attack convergent realism and its disjunction of knowledge and the knowable world-as-it-is — of knowers and the real objects of their knowledge. Supplanting dualism with constructivism enables new forms of philosophical argument. The latter conception is not just a dreary empirical account of the many things scientists achieve with language, it is part of a broader philosophical view of what human knowledge of the world amounts to.

Even if constructivism is allowed to be a philosophical thesis whose defence goes beyond considerations of historical detail, it may still be unacceptably *vague* as a thesis

[183]Feyerabend's use of Galilean history to refute rationalism is one famous case in point (see *Against method, passim*). For another example, see Worrall, 'Thomas Young and the "refutation" of Newtonian optics'. See Williams, 'Should philosophers be allowed to write history?', for the view that *all* philosophical theories of science are refuted by careful history.

[184]Gooding, *Experiment and the making of meaning*, pp. 19 f. Cf. Shapin, 'The mind is its own place', pp. 201 f.

about language. For if language is broadened beyond the bounds of a representational or symbolic system and redefined as a resource for shaping and interacting with the world, this conception will fail to capture that which is *distinctly* scientific about functions of language in science.

Two clarifications need to be made in response to this objection. First, the issue at stake is whether the role of language in the creation of scientific knowledge allows for a philosophical dissociation of it from real structures or phenomena of the world that it is aimed to express. Constructivism gives a clear negative answer. Consideration of just a *few* of the constructive functions of language shows that language is not deployed 'declaratively', descriptively, or referentially to give expression to ready-made structures of the world, *except* in reconstructions of discovery where what is sought is rhetorical effect or conformity to tradition (Table 3). At the frontiers of discovery language is inextricably part of that which scientists believe or know there is. Second, there is no good evidence for the claim that there *is* something distinctly scientific about uses of language in science. The rhetoric that is thematically related to the language/ world disjunction may itself be distinctly scientific, but I have argued that it is wrong to believe that there are real functions of language that correspond to that metaphor. The wish to identify a distinctly scientific function of language is reminiscent of failed attempts to identify a distinctly scientific *logic.*

Finally, in this first category of objections, it could be said that there is *nothing novel* about the constructivist conception. Even if it is true that philosophers have paid insufficient attention to broader issues of language in science, they have nevertheless been aware of the importance of many of the functions brought together under the supposedly novel conception. They have simply *presupposed* those functions and relegated them and much else to the 'context of discovery'. Everyone accepts that language functions in science as more than a mere transcription device for the expression of experience, as more than a mere instrument for the creation of reference-relations to the non-conceptual physical or phenomenal world. Many also accept that science has depended throughout its history on the inventive deployment of cultural resources like language and metaphysics. It is just that the problems philosophers of science have inherited from their predecessors have happened to focus on rationality, method, knowledge and belief, the context of justification, ideal languages, etc., rather than ordinary language in use.

The response to this objection must again take the form of a clarification. Even though it may be true that philosophers of science in the analytic tradition have concentrated on issues other than language for historical reasons, and have presupposed a rich and varied context of discovery, the issue at stake is whether they have made assumptions about language that have conditioned their treatment of the issues they *have* concentrated on. Constructivism derives some of its novelty from the contrast I draw between it and dualism, and in my claim that the latter conception exists in the form of a widespread, though unjustified assumption in the philosophy of science, an assumption that has conditioned the definition and treatment of many issues. Given that *assumptions* about language cannot be completely consigned to the context of discovery, it is important to make them consistent with the constructivist conception for which justification (or corroboration) exists.

In addition, constructivism draws some of its novelty from a strikingly novel feature of *Gooding's* analysis: his demonstration that the construction of low-level experimental knowledge acts as its own justification. It does not await justification by theory, formal published argument, or whatever. (Such knowledge remains revisable, nevertheless, and gains in importance and relevance as it is affiliated with theory.) Overt reasoning does indeed grace the rhetoric of the published report, but demonstration of the world's make-up has already taken place in less overtly reasoned social contexts. The belief that justification stands distinct from discovery partially explains why the processes of experimentation (as opposed to their dressed up and theoretically contextualised results) have been largely neglected in philosophy. In reality discovery and justification-in-language are largely undifferentiated, or, to put it otherwise, co-exist already in the most basic reconstructions of experimentalists (Table 3).

2.3.2

I now turn to a category of likely objections best described as realist. Scientific theories, it will be said, are above all theories about the properties things have and the structure of the world itself. To say that these properties and structures are language-laden — to suggest that there is a problem with treating them as entirely independent of human actions and interests — is either to misunderstand what scientific theories are all about or to challenge their credibility. 'Atom' is an item of language, an atom isn't. Language-ladenness and the constructivist conception are passing anti-objectivist fads, predictable offshoots of the ongoing fascination with 'discourse'. The fact is that scientists are realists and their behaviour corroborates the metaphysics of realism. Most historians and philosophers of science have therefore rightly reconstructed the history of science realistically.

Nevertheless [the objection continues], let it be granted that language *is* important to the formation of scientific knowledge. That cannot be relevant to what scientific knowledge is knowledge *of*. Even if it can be shown that language is invested in the construction of facts at various levels of practice and thought, why can't language also function to characterise independently existing natural phenomena, that is, phenomena whose properties are independent of our interest in them and our means of characterising them? Consensus about facts in science means that there is no reason to doubt that certain physical processes and objects have the particular physical properties and structures attributed to them. Constructivism is nothing short of an attempt to cast doubt upon well-established facts by arguing their social provenance.

Much of the above objection is evident in the words of a scientist from a biological laboratory:

> The structure of RNA ... is a typical scientific fact if there ever was one and it seems perverse to explain it as a 'social construct'. Our belief that DNA is a double helix is simply a result of DNA being structured that way in reality. Further, we have good reasons to back this claim. On one hand, there is no reason to doubt it, since there is no competing alternative theory, no controversy, and there are no unresolved problems. On the other hand, knowledge of the structure of DNA has led to successful application in many areas like genetic engineering. If there was ever any 'social' element in the establishment of the structure, it has simply been *winnowed out*, leaving only the facts which correspond directly to what there is in nature.[185]

I shall return later to the argument that 'successful applications' of a known structure constitute proof of that structure's independent reality, to the idea that initial social input to scientific knowledge is gradually 'winnowed out', and to the claim that realistic reconstructions of scientific history are justified. First I consider the question, Why, if language is so rich in its functions, can it not function to characterise independently existing properties of things, such as DNA? The question assumes that physical objects can be said to possess properties independently of any scientific determination and attribution of properties — they are possessed, in Locke's words, 'whether we perceive them or no'.[186] So, why cannot language serve to pick out properties things really have, at least those that it is within our means to determine?

The claim that scientists discover properties — 'properties', 'attributes', 'physical objects', etc., are ontology-laden concepts of analytical philosophy, and therefore hardly 'discoverable' — that pre-exist in nature can be checked against actual processes of discovery. Gooding's account of early investigations of electromagnetism provides one such check. According to arguments overviewed in Part 2.2, low-level

[185]Quoted in Charlesworth et al., *Life among the scientists*, p. 49, my emphasis.

[186]Locke, *An essay concerning human understanding*, II.8.23. 'How *can* there be real objects that have no properties in themselves?', Goldman wonders (*Empirical knowledge*, p. 213).

experimentation is a process during which *nothing* is obvious or found ready-made.[187] What is discovered is always the product of human intervention in nature, physical as well as linguistic and cultural. These arguments undermine realism in two ways: they deny that there are in the world properties and entities that are independent of our interpretative resources, and they deny that scientists discover and describe pre-existing properties and entities of the world.

As an alternative to Gooding's analysis of Faraday's notebooks, consider my constructivist interpretation of Latour's and Woolgar's account of TRF research. It would be simplistic to think that TRF is (or ever was) just a particular extracted fluid in a test tube, a synthesised white powder, a molecule of known structure, or an otherwise standard philosophical 'object'. At its inception, TRF existed as a hypothetical trace ingredient in a vastly rich biological soup. From the very beginning it was studied in bioassays, that is, in its presumed physiological *relationships* to organic parts. Knowledge of TRF has always been knowledge of a *system* or *network* of selected physiological relationships and processes in which TRF has been thought to be operative. Are those *relationships* found ready made in nature? (Are cause and effect found ready made?)

The simple name 'TRF' brings together a varied range of characterisations that have amassed to the substance over the years. Thousands of scientific papers have provided characterisations of how TRF behaves in different circumstances — what it triggers, how and where it flows, what hormones it works with or against, what it doesn't do, etc.[188] A great number of scientists and technicians have spent years at laboratory benches becoming skilled at manual and mental exercises involving TRF in various physiological contexts. It is this research, guided by human interests and classificatory schemes, challenged by uncertainties and controversies, that has constituted scientific

[187]Gooding writes about the early stages of an 'object': 'New experience is made possible when the whole, chaotic field is temporarily ordered in terms of one of its aspects. The models select features or *aspects* of an effect in a concrete, visual and operational way. This establishes those aspects as a clue to interpretation of it as an "object" of interest, manipulation or further investigation. If that "object" makes present investigation intelligible and further investigation possible it then becomes a candidate for communication to others, and a possible basis for a shared way of seeing the phenomenon' (*Experiment and the making of meaning*, p. 74).

[188]Hacking writes: 'at least 1,000 research papers about the substance are published every year. The research ... leads to questions about where in the bodies of living organisms one can find TRH, how one can stimulate or simulate its production, and what increases in production do to other chemicals in the body [etc., etc.]' ('The participant irrealist', pp. 287-288).

conceptions of what TRF is and, thereby, what TRF *is*. Networks of physiological relationships and processes in which TRF is meaningful are — like the category 'hormone', or the area of the brain called the hypothalamus — ordering categories applied to nature, not ready-made natural categories. The conception of TRF as just a certain liquid or powder in a test tube — or a molecule with a well-understood structure — in brief, an object to which a snip of language attaches, removes the substance from its scientifically understood procedural contexts and reduces it to something that can be of *no* scientific interest.

Of course, it is quite possible, and may even be common, to use 'TRF' to refer to nothing but a certain molecule, natural or artefactual, and its structure. However, any biologist pressed by an 'outsider' ('Why is *this* substance TRF and not that?', 'Why is *this* structure a TRF molecule and not that?') has only the physiological relationships to fall back on ('Because that other substance/structure has a quite different effect under such-and-such conditions ...'). In other words, it is quite possible to think of TRF as a molecule with a certain structure — an object with a property — found ready-made in nature (it is, after all, found in jars on shelves), but that is only possible now that the externality accorded to the substance allows it to stand free, in most contexts, of the (constructed) network of effects in which it acquired its meaning. Although TRF can be found ready-made today, this is not how it was discovered.

The realist objection raised above suffers also from the problem of perceptual access. If in pressing a realist thesis one were to insist: 'the facts about TRF have existed all along; scientific theories are about those facts' — one must be thinking about facts stripped of all the characterisations of TRF that scientists regularly employ. But then who is to know that the facts indicated by theories and the supposed facts themselves are one and the same? A constructivist will not deny that TRF has always had a certain structure and has always been secreted by the hypothalamus of higher vertebrates. There is nothing wrong with believing that, so long as *philosophers* realise that it was only after a certain date (1969) and a particular series of laboratory events, exchanges, and negotiations that a structure for TRF did become a fact. Only after that date did it become true to say that that structure was *always* a fact, that that new patch of blooming desert was nothing but an advance of the blot of ink.[189] The non-philosophical belief

[189]The physical world A is what one *makes* of world B. World A today does not include phlogiston. But world A did include phlogiston (variously conceived) for most of the eighteenth century (indeed, from Stahl to Priestley). That is what they *made* of world B in those days. We, of course, are inclined to say that *world B has never included phlogiston*, neither then nor now, because past 'resistances' led us to abandon the substance. But that is to speak imprecisely, in my view, for we can never know

that TRF has always had a certain structure does not justify the philosophical belief that that structure can be understood to exist independently of the linguistic resources that went into its discovery (that brought nature into language), nor does it justify the belief that the structure is entirely dissociated in reality from the language that gives it expression. Gooding's asymptotic realism shows how non-philosophical beliefs of the kind in question may emerge within a constructivist account of knowledge as a consequence of the convergence of construed phenomena and their interpretations.

According to dualism of the realist kind, language expressing scientific knowledge is about the world itself. Language-rendered knowledge of a natural thing is entirely distinct from what that thing is like in nature, and vice versa. At the foundation of the objection that constructivism fails to recognise that scientific theories are about real things, like DNA, and that it seeks to undermine science and its aims, lie dualist assumptions about language and knowledge. But the rejection of this dualism does not instantly eliminate the view that science is about real things and seeks objective knowledge.[190] Constructivism accommodates this view with the proviso that the reality of things and the objectivity of knowledge cannot be grounded in a world beyond scientific practices and conventions. Scientific knowledge, the product of cognitive practice, *is* subject to empirical constraints — but they are the constraints of the uninterpreted world B. Facts are interpretations of that world.

(A note of caution: is nature's recalcitrance — the constraints of world B — or, rather, the '*resistances*' found at the interface of world A and world B — are *they* found ready-made in nature? The answer is, of course, No: resistances are only manifest *relative to prior expectations* [pre-verbal and practical, as much as theoretical]. Resistances are themselves situated in a social context.)

anything about world B. Resistances are important, for they are the points of contact of worlds A and B. But what we make of these resistances belongs only to world A. — 'How can phlogiston have both existed and not existed?' The trick is to see that it is perfectly possible for phlogiston to have physically existed and not existed in type-A worlds, for they are constantly changing. The notion of physical existence associated with these worlds is much looser than that associated with traditional philosophical 'objects', which are not allowed to come in and go out of existence in the same way.

[190]In *Language and the discovery of reality*, Church writes that 'we can manipulate symbols in ways impossible with the things they stand for, and so arrive at novel and even creative versions of reality' (p. 95). On my account, the novel and creative versions of reality are arrived at not through the manipulation of symbols linked to pre-linguistically existing things, but through the linguistically empowered construction of things, for which symbols are (in the process) 'made to stand for'.

The argument that, over time, social inputs to knowledge are 'winnowed out' is another common defence of realism. The argument has been variously expressed. In the following instance, Heinz Pagels argues that by varying the language or 'symbolic representations' in which knowledge is expressed, cultural artefacts cancel out. Language and mathematics, he writes,

> are both symbolic means of representing the world ... Some representations emphasise the wave-like properties, others the particle-like properties, but it is always the same entity that is being represented ... It is by varying the symbolic representations through transformations that we arrive at the notion of *invariants*: those deep, intrinsic properties of an object which are not just artifacts of how we describe it.[191]

Pagels illustrates the problem of perceptual access when he claims to know that 'it is always the same entity that is being represented'. His is not a case, after all, of two very different symbolic systems evolving independently, yet converging on the same 'invariants'. A better case for 'winnowing out' is made by Hacking, who argues that *techniques* — artefacts not merely of description but of practice too — cancel out to reveal facts independent of human intervention:

> We purify some aspect of nature, isolating, say, the phase interference character of light. We design an instrument knowing in principle exactly how it will work, just because optics is so well understood a science. We spend a number of years debugging several prototypes, and finally have an off-the-shelf instrument, through which we discern a particular structure. Several other off-the-shelf instruments, built upon entirely different principles, reveal the same structure. No one short of the Cartesian sceptic can suppose that the structure is made by the instruments rather than inherent in the specimen.[192]

The argument in this passage needs to be assessed in the context of Hacking's realism about entities, a thesis that is supposed to draw its strength from the ways in which scientists 'intervene' in nature. Hacking's philosophy is an important source of possible objections to the constructivist conception precisely because it purports to derive from the actuality of scientific practice. Before I turn to a more detailed examination of that philosophy, I would like to draw attention to two features of the quoted passage, which I shall return to. First, Hacking refers to the protracted process of 'debugging' — that is, the telling apart of fact from artefact that leads to the

[191]Pagels, *The cosmic code*, pp. 83-84.

[192]Hacking, *Representing and intervening*, p. 204. Cf. Galison: 'Our grounds for faith in our instruments' reports are manifold. In addition to tests by correlation among diverse instruments, our ability to intervene, and our understanding of underlying physical principles ... the mark of the new large-scale physics is the creation of data reduction as an integral path of the experiment. All these techniques have figured in our vastly increased ability to extract real effects from the merely artifactual' ('Bubble chambers and the experimental workplace', pp. 358-359).

elimination of the latter — needed to make and to competently use an off-the-shelf experimental device. Second, Hacking's reference to the Cartesian sceptic shows that what is at stake in his argument is the degree to which *belief* in structures considered to be non-artefactual is *justified.* Both these features are important to understanding why the 'winnowing out' argument fails to discredit the constructivist conception.

2.3.3

Philosophers' concern with scientific practice (as opposed to theorising or reasoning) has been limited, and limited by the assumptions of dualism. Consider, for example, part of Steven Toulmin's criticism of Popper's third world:

> differences between mathematics and natural science have serious implications for the character and contents of the so-called 'third world' ... If the intellectual content of any genuine natural science embraces, after all, not only *propositions* but *praxis* — not only its theoretical statements, but also practical procedures for their empirical application — then neither scientists nor philosophers can afford to confine their 'rational' or 'critical' attention to a formal idealization of its theories.[193]

Toulmin follows on to strengthen his distinction between propositions and praxis and to insist 'on giving the non-linguistic praxis of science as much attention as its linguistically formulated propositions'.[194] In this scheme, propositions are linguistic, praxis isn't. The former are 'applied' and are about whatever is discovered in the course of the latter. An emphasis on practice need not stray from the dualist framework.[195]

The importance of scientific practice as a source of philosophical lessons has been advocated most notably among philosophers by Hacking. In his article 'Do we see through a microscope?'[196] — where realism is not in any way restricted to the 'medium-size theoretical entities' revealed through microscopes — Hacking argues three things. First, as indicated earlier, he calls attention to the similarity of results obtained by different types of apparatus based on different physical principles. Second, he argues that our belief in the reality of what we see is strengthened by the possibility of *intervening*; the predictable manipulation of a phenomenon gives added credence to its reality. In a microscope our success in observing a person-made calibration grid gives us faith in the verisimilitude of the enlarged images of hitherto

[193]Toulmin, 'History, praxis and the "third world"', p. 665.

[194]Ibid., p. 668. The same distinction is evident in Toulmin's *Human understanding*.

[195]See also Franklin's *The neglect of experiment*, where a realist interpretation of experiment commits him to the dualist conception of language.

[196]Reprinted in Hacking, *Representing and intervening*, pp. 186-209.

unseen objects. Third, in addition to practice, an understanding of the physical principles behind the apparatus inspires further confidence in the reports we glean from it. These arguments all point in one direction: scientific practice makes use of theoretical entities (whether 'observable' or not) as tools not for thinking, negotiating, or writing, but for *doing*. At the level of action, 'when we turn from representation to intervention, to spraying niobium balls with positrons',[197] social inputs and the supposed language-ladenness of facts are entirely irrelevant. Scientists systematically manipulate and do things with many of the entities they postulate. These entities and their properties are manifest in practice and independent of any linguistic input to their discovery. Our metaphysical theories should reflect these facts.

The idea that intervention testifies to the reality of things is novel and potentially fruitful (Gooding sought his empirical basis in engagements with nature too), but, as I shall argue, ineffective as a weapon against constructivism.[198] Hacking begins the article on microscopes with the reminder that one *learns* to see through the instruments by actively doing, not passively looking. Of a philosopher peering down a microscope for the first time, he writes:

> Asked to draw what he sees he may, like James Thurber, draw his own reflected eyeball, or, like Gustav Bergman, see only 'a patch of color which creeps through the field like a shadow over a wall'. He will certainly not be able to tell a dust particle from a fruit fly's salivary gland until he has started to dissect a fruit fly under a microscope of modest magnification.[199]

In learning to see by doing the apprentice microscopist actively engages in making his or her own knowledge. An important part of this process of learning is the acquisition of new linguistic competences and conceptual categories — an acquisition normally guided by tutors experienced in microscopy, and the relevant texts. 'Salivary gland' is an anatomical category with which the student needs to be conversant before he or she is able to spot a dissected fly's salivary gland for the first time. Without the development of linguistic competencies nothing much relevant to scientific microscopy can be learnt or seen. Hacking doesn't identify or emphasise this aspect of learning through doing, perhaps because he takes it for granted. In any case, this unidentified aspect becomes relevant in evaluating the implications of a closely following passage:

[197]Hacking, *Representing and intervening*, p. 31.

[198]Carrier notes that if we employ Hacking's entity-realism as a guide to what exists, then *phlogiston qualifies as real* because Stahl, for example, manipulated phlogiston in order to experiment on another, more hypothetical phenomenon, namely the composition of sulfur (Stahl's celebrated sulfur synthesis was published in 1697). See Carrier, 'Establishing a taxonomy of natural kinds', pp. 400-403.

[199]Ibid., p. 189.

> The *conviction* that a particular part of a cell *is there as imaged* is ... reinforced when ... you microinject a fluid into just that part of the cell. We see the tiny glass needle — a tool that we have ourselves hand crafted under the microscope — jerk through the cell wall. We see the lipid oozing out of the end of the needle ... [etc.].[200]

The point here is that once we are able to *do* things to cells, once we are able to intervene and manipulate the microscopic world by using microscopes and microscopic artefacts, we should find it absurd to doubt that we see through microscopes, or that cells are anything but real actually existing things. Undoubtedly this *conviction*, arising in part from our having learnt to distinguish aberrations and artefacts from the objects that concern us, is justified as Hacking says. However, a stronger claim can be found in Hacking's passage, namely that the existence of cells *as cells* — the existence of their characteristics and properties — is entirely independent of the history of scientific interest in them and of actions taken to determine them. This camouflaged claim is that cells have *always* existed as conceptually imaged now — at least within the temporal constraints set by the theory of evolution. With the help of the microscope and by other means we (merely) find or discover their characteristics and properties.

Facts about learning are important in evaluating the stronger claim because, as I have said, the process of learning to use a microscope, to tell fact from artefact, etc., includes learning to see 'structures', like the structure of a salivary gland. This learning is not independent of acquiring language and conceptual categories relevant to anatomy, or to any other subject of investigation. Thurber's sketching of the image of his eyeball was of course a prank, but also intended to emphasise that the inexperienced microscopist is likely at first to see anything or nothing.[201] The linguistic skills of the *experienced* microscopist have not fallen out of the picture, they have become as transparent as any other skill that he or she has mastered. All that is left — and all that appears in the published report — is, as Hacking puts it, the 'structure in the specimen in essentially the same two- or three-dimensional set of relationships as are actually present in the specimen'.[202] The use of microscopes exploiting different physical

[200]Ibid., pp. 189-190, my emphasis. Cf. Hacking, 'The participant irrealist', pp. 277-278.

[201]You look at an electron-microscope micrograph for the first time. You might find that the shapes have an aesthetic appeal. You might assume that the micrograph must be of *something* for someone to have made it and kept it. But until you are told *what* the micrograph is a micrograph of, you might also reasonably believe that it is of anything, or nothing. Language must be worked into the mute image, as it were, before that image is epistemically accessed — albeit superficially — by you the beginner. For the scientist, a much more complicated language has been integral to the understanding of such images ever since the early stages of his or her apprenticeship.

[202]Hacking, *Representing and intervening*, p. 208.

principles to reveal identical structures does not change the fact that the recognition of structure, and the recognition of structural identity underlying images, presupposes a specialised and specific linguistic training. This training, insofar as it leads to the recognition, sustenance, development, and creation of new ontologies in world A, is training *in prescription* primarily, and *in description* only secondarily.

Hacking may seek to counter this constructivist retort with a variation on what he calls 'the argument of the grid'.[203] It runs as follows. A familiar artefact is introduced into an experimental device just as if it were an unknown object of scientific interest. We examine the output of the device and we recognise the exact characteristics of the thing that we have made. It follows that when, under identical circumstances, we see the structure of an unknown object not of our own making we must be seeing the structure-in-itself, not a social construct. In the case of microscopes, familiar artefacts of the kind in question exist commercially. They are called 'grids' and are produced following well understood principles of photographic reduction, in this case of fully visible grids consisting of lettered squares drawn with pen and ink.[204] So confident are we of what the tiny grids should look like that we can use them to remove aberrations from various types of microscope. If, then, microscopes can deliver, unaffected, structure pre-fabricated by us, *why can they not deliver structure as it exists pre-fabricated in nature?*

One problem with the argument of the grid is its assumption that 'asocial' explanations can be given of our ability to recognise structure in such objects as chessboards, billiard balls, etc. The constructivist conception of language is as relevant to childhood learning as it is to scientific discovery. Accordingly, our ability to recognise, for example, structure in a chessboard cannot be understood apart from our efforts earlier in life to conceptualise the world around us using, among other things, the linguistic resources that human society made plentifully available to us.[205] My extension of constructivism to early learning (which I shall not try to defend here) does not entail that everything is in the mind — it *does* entail that the argument of the grid cannot provide support for a language/world disjunction by collapsing recognition of the unfamiliar into recognition of the familiar.

[203]Ibid., p. 202.

[204]Ibid., pp. 202-204.

[205]'Duck-rabbit' type drawings, rather than chessboards, more often serve to remind us of our own contributions to our experience.

Even when the scope of constructivism is limited to science, the conclusions of the argument of the grid can be shown to be untenable. Consider what I call the 'divided room' situation. I am sitting in a small room divided by an opaque partition. I am facing the partition, on the other side of which are two objects. My hands control two rods that penetrate the partition. By moving the rods, my task is to determine the shape and relative size of the objects on the other side. The rods function as very rough extensions of my hands. After a lot of fumbling around I decide that the two objects are right-angled isosceles triangles in shape, and identical in size. I decide to test my supposition by joining the two, so that the hypotenuse of one meets that of the other (or at least that is how I believe I've joined them). I now fumble around some more, and, indeed, the new object seems to me square-shaped. I then proceed to make *use* of the objects to carry out more complex tasks (perhaps involving newly-found objects), and I soon take it for granted that the original two objects are of precisely the triangular shape I decided upon initially, and of identical size. Is action in the divided room a good analogue of scientific inquiry?

An evident feature of the divided room is that it is an entirely person-made environment. The objects on the other side of the partition already have *well-established names* and are subject to well-established descriptions of a kind familiar to me. Some person has put them there. I, or somebody else, could check my suppositions about the objects simply by peering into the closed off part of the room. Assume that my suppositions are correct. If we ask: 'Were the objects triangular in shape and identical in size *all along*? Did I simply *discover* shapes that pre-existed my investigation?', the answer, of course, would be 'Yes!'. The person-made environment of the divided room guarantees that a definite answer can be given. The facility of access to objects contingently unknown to me along a path unrelated to my own efforts at examination and recognition (i.e. the fact that the objects are directly accessible) makes the divided room situation a *very bad* analogue of scientific inquiry. Yet action in the divided room is similar to looking at a tiny grid through a microscope. Under the microscope we see (and do not say that we 'discover') those lettered squares that we know pre-existed — along with their well-established names — our act of looking.

The argument of the grid is intended to work, as I have indicated, by showing that it is possible to recognise the exact structure of a familiar object, which has been turned into the object of an experimental setup, *despite* mediation of experimental devices. This recognition is then taken to settle doubts about our ability to see structures of *unfamiliar*, unknown objects as they exist in themselves, or at least as they exist apart from the mediation of artefacts of instrumentation and language. Unsatisfactory about this argument is the assumption that the process of determining something never-before-known closely resembles the process of detecting that which is known.[206] Considerations similar to those that make the divided room situation a bad analogue of scientific inquiry make the process of calibrating a microscope with a tiny grid entirely unlike the process of discovering new cell properties (structures, functions, interactions, relationships, etc.). The latter process does not involve the application of a well-established language, and is not free of uncertainty (at their inception 'discoveries' have the status of construals). If and when the discovered properties become commonly accepted, if and when they become things that scientists take and use for granted, and assuming (if they are accepted) that they are properties of a kind visible under a microscope, then they will appear as familiar as the lettered squares of the tiny grids. But no matter how strongly we *believe* in the reality of these discoveries, from a philosophical point of view we have reason to maintain that familiar scientific knowledge does not correspond to anything 'out there' independent of the language and other cultural inputs of scientists. The argument of the grid does not undermine the constructivist conception of language.[207]

The above distinction between scientific belief, on the one hand, and philosophical theses about 'the world', on the other, requires additional comment. Hacking and other realists generally fail to distinguish good or excellent reasons for belief in scientifically determined properties of the world, from the claim (common to the realisms that they advocate) that the world has those properties whether scientists have determined them or not. Consider, to begin with, three instances of Hacking's realist argument from 'intervening' and the importance he accords to *conviction and belief*:

> [i] We are convinced of the structures that we observe using various kinds of microscopes ... We are convinced about the structures we seem to see because we can interfere with them in quite physical ways, say by microinjecting. We are convinced because instruments using entirely different physical principles lead us to observe pretty much the same structures in the same specimen.[208]

[206]Blackburn writes: 'Our judgement that a cat is in the garden is made true, if it is true, by the cat's being in the garden. ... We don't, as it were, look sideways, either to other people or to systems of belief. We look at the cat and look round the garden' (*Spreading the word*, pp. 247-248). Unlike experimental science, this is a world we have grown into and need not re-explore or re-discover. Cf. Carnap's example of a group of geographers reaching a decision on the height of a mountain, in Coffa, *The semantic tradition*, p. 225.

[207]For additional criticism of Hacking's interventionism see Gross, 'Re-inventing certainty', especially p. 425.

[208]Hacking, *Representing and intervening*, pp. 208-209.

> [ii] The vast majority of experimental physicists are realists about some theoretical entities, namely the ones they *use*.[209]

> [iii] The 'direct' proof of electrons ... is our ability to manipulate them using well-understood low-level causal properties ... Determining the charge of something makes one *believe in it* far more than postulating it to explain something else ... Uhlenbeck and Goudsmit in 1925 assign angular momentum to electrons ... Electrons have spin, ever after. The clincher is when we can put a spin on the electrons, polarise them and get them thereby to scatter in slightly different proportions.[210]

There is no doubt that this really is the clincher as far as belief in the existence of entities with the properties mentioned is concerned. Do any *philosophical* conclusions follow? Realists will say that belief bolstered by successful, predictable 'intervention' supposedly shows that, (1) *scientists* are realists, (2) scientists discover independently existing entities and properties of the world, and (3) that dualism is inescapable because the pre-constructed world and language are *necessarily* disjoint. In a laboratory study of his own, Ronald Giere can be seen to be arguing for all three conclusions. Notice that in [c] below he explicitly takes himself to be *opposing* challenges to *justified belief* (and allegations of 'deception'):

> [a] It is interesting to note that the time-of-flight measurements reveal just what anyone would expect. For fixed proton energy and a fixed direction of the ejected neutron, the farther away the neutron detector, the longer the time of flight ... Again ... the time of flight increases for greater angles between the incoming proton and the ejected neutron. In these respects protons and neutrons are similar to billiard balls.[211]

> [b] The only remotely plausible scientific account of what these physicists are doing requires us, as students of the scientific enterprise, to invoke entities with roughly the properties physicists themselves ascribe to protons and neutrons.[212]

> [c] Empiricist philosophers ... would argue that ... scientists are not really justified in *believing* that there are such things as protons. Constructivist sociologists would claim that ... scientists, through their social practices, have *deceived* themselves into thinking that their own social constructs have an independent existence.[213]

> [d] [But] there can be no doubt that the nuclear physicists I have observed are realists in the sense that they *believe* something is going round and round in the cyclotrons, down the beam pipes, and striking the targets. Moreover, they *believe* this something has roughly the properties ascribed to protons — mass,

[209] Ibid., p. 262.

[210] Ibid., p. 274, my emphasis.

[211] Giere, *Explaining science*, p. 122.

[212] Ibid., p. 112.

[213] Ibid., p. 124, my emphasis.

> charge, momentum, and so forth ... few students of the scientific life would deny that most scientists are realists.[214]

As Giere suggests, these views are widely held among other realists, whether or not they choose to concentrate on experimental practice as he does.[215] It should be clear to everyone that to call scientists realists is to attribute to them a *metaphysical* view. Does Giere fully justify his attributions? Notice that when he writes that his nuclear physicists are 'realists in the sense that ...', he goes on to refer to mundane, scientific, *physical* beliefs, not a metaphysical view of theirs — his experimentalists are realists by virtue of beliefs they hold as scientists. Yet the holding of ordinary scientific beliefs has never amounted to realism in *any* sense. When Giere rallies us to 'invoke entities with roughly the properties physicists themselves ascribe to protons and neutrons', it is not immediately clear what he expects us to do. Though we have little choice but to agree with scientists that entities with the said properties exist (nuclear physicists are more familiar with protons and neutrons than most of us are with billiard balls), we need not agree that their existence (as characterised) is *not* a social construction, or that it is *independent* of scientific conceptual and conventional practices. To agree or disagree with the latter *is* to take a stand on a metaphysical issue. What makes realists realists is not mere belief that protons and neutrons and all of their known properties exist as scientists maintain, but belief in addition that protons, etc., exist 'naturally' enjoying the said properties, and are objects entirely unmade by us (that Giere as a matter of fact holds the latter belief is evident in [b] above).[216]

Hacking's and Giere's arguments from intervention are perhaps sufficient to counter Giere's unnamed 'empiricist philosophers' (in [c]) — who claim that scientists are unjustified in believing all that they do about protons — but the same arguments cannot possibly support realism. They only *appear* to do so because Hacking and Giere do not clearly distinguish scientific belief from philosophical doctrine. The so-called empiricist philosophers do not do so either — their claim, one suspects, is not primarily that scientists are not justified in believing hard facts about protons, but that they are not justified in believing *hard facts*. That is, empiricist philosophers believe that because

[214]Ibid., my emphasis.

[215]See, for example, McMullin, 'A case for scientific realism', and Franklin, 'The epistemology of experiment' (which is all about 'rational belief').

[216]Cf. Fine: 'To be sure we do have reason to believe that there are molecules and atoms ... The reasons are embedded in the various overlapping and ever-open practices that constitute the judgement of those claims by the community of concerned scientists. They are good reasons, in some cases the best we are likely to find in support of *any* belief. Of course to see such grounds as sufficient for belief in the truth of the claims is a far cry from realism' (*The shaky game*, p. 171).

realism is untenable (the world is epistemically inaccessible), scientists ought to be anti-realists — they cannot justifiably believe or know that there are such things as protons. Hacking and Giere, by contrast, think that because scientists *can* justifiably believe or know that there are such things as protons (they use them daily), they ought to be realists and therefore realism is tenable. The parties move (unjustifiably) from scientific knowledge and belief to philosophical doctrine, and vice versa.

Scientists work with and within their own constructed scientific discourse. Even if philosophers could agree that operationalism or some other variety of anti-realism was the correct metaphysical view to hold about science, this would have no effect whatsoever on scientists' perceptions and pursuit of scientific knowledge. This of course does not mean that scientists and scientific knowledge are unaffected by metaphysics. As was argued in Part 2.1, metaphysics has played a formative role in modern science. But even though the rhetorics of out-thereness and objectivity have become a standard feature of scientific discourse as we know it, they represent a low-profile metaphysics that is part of everyday scientific use and not often the subject of debate. Metaphysics in science is much less salient than metaphysics in philosophy. Within world A scientists are able to make all sorts of distinctions expressing how certain or uncertain they are about this or that physical process. The expression of certainty by means of externalising rhetoric does not turn scientists into philosophical realists.[217]

Hacking and like-minded realists are wrong to believe that intervening and getting things done results in scientific conviction that is evidence *only* for realism. That conviction is compatible with constructivism (Gooding explains scientific belief as the result of the convergence of interpretations and practices), and with most expressions of anti-realism. For example, recall Latour's and Woolgar's brand of anti-realism and their emphasis on 'inscription devices': 'the central importance of this material arrangement [of inscription devices] is that none of the phenomena "about which" participants talk could exist without it'.[218] Material arrangements in the laboratory give rise to dependent observations. But the dependency here is much stronger than the

[217]Giere's unnamed 'constructivist sociologists' — also referred to in [c] — thus err in attempting to show that scientists hold false philosophical beliefs about their scientific beliefs. What they ought to be concerned with instead, and the limit of what they *can* demonstrate, is that scientists do not discover physical processes ready-made in nature, but construct them through negotiations, etc. Many realists have misunderstood what 'constructivist sociologists' aim to do. Giere is, I believe, one of those realists. See Jennings, 'Truth, rationality and the sociology of science'.

[218]Latour and Woolgar, *Laboratory life*, p. 64.

dependency of an outside view on a window. Latour and Woolgar demonstrate this at the level of vocabulary. Not only is there device-specific vocabulary ('fractionating columns', 'nuclear magnetic resonance spectrometer'), the characterisations of the substances are also device-specific or device-derivative ('fractions', 'spectrum'): 'without a bioassay ... a substance [like TRF] could not be said to exist'.[219] The emphasis here and in the following quoted statements is on the phrase 'could not be said':

> a substance could not be said to exist without fractionating columns, since a fraction only exists by virtue of the process of discrimination. Likewise, the spectrum produced by a nuclear magnetic resonance spectrometer would not exist but for the spectrometer. It is not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory.[220]

When enough scientists become *convinced* of the existence of a certain substance, argue Latour and Woolgar, they come to believe that it was there all along. This does not conflict with the claim that, for example, the bioassay begins the construction of TRF (the substance has to be constructed before it can be said of it that it was there all along). Latour's and Woolgar's anti-realism is thus not incompatible with the accurate claim that scientists strongly believe in the existence of certain entities and their properties and are well-justified in doing so. To repeat, at issue is not whether 'doing' is good grounds for scientific belief in the reality of physical objects — undoubtedly it is — but whether one can say of these objects that they are entirely independent of scientific language and practices that give them expression.

2.3.4

These considerations relate to my response to an earlier objection. Philosophers' use of history and sociology of scientific knowledge *must* be supplemented by philosophy, and in this thesis that is what I have sought to do. It is with Giere's entry into the laboratory, by contrast, that philosophy is reduced to sociology of belief. For Giere sees a philosophical thesis following immediately from the fact that belief in electrons, spin, and so on, is unassailable as far as scientists are concerned. The above considerations also contradict the claim that philosophers have rightly reconstructed scientific history realistically *because* scientists are realists. As I have said, the fact that a great amount of scientific knowledge is extremely well justified is easily accommodated within non-realist philosophies of science.

A rather different argument for a realist historiography is mentioned by Simon Blackburn in the following passage:

[219]Ibid.

[220]Ibid.

> Currently the most discussed view is that realism is the best way of explaining our scientific success, that the existence of facts explains the way in which our knowledge expands and progresses: here an explanatory role seems to carry with it an ontological commitment.[221]

Blackburn perceptively remarks that no ordinary historical explanation of success is at stake here. What is at stake is an explanation peculiar to philosophical reconstructions of science. The argument is that whatever scientists know about the world they know *because* the world is as it is. That is, the best philosophical explanation of their many fruitful beliefs and practical successes — the best explanation of the robustness of contemporary scientific knowledge — is that it is 'true', that is, corresponds to what the world is naturally like or, as John Worrall puts it, to 'the blueprint of the Universe'.[222] Many objections to the argument that scientific success can be explained by 'truth' have been raised (once-successful theories that are now considered false are an obvious point of reference for such objections), among others by Hacking and Putnam.[223] My own response is in two parts. First, arguments I considered in Part 2.1 (Latour's in particular) suggested that appeal to the reality of theoretical constructs to legitimate scientists' judgements can only be made when it has already been decided *which* constructs are real. But consensus about the reality of a construct is always the

[221]Blackburn, 'Truth, realism and the regulation of theory', p. 356.

[222]Worrall, 'Fresnel, Poisson and the white spot', p. 155. Worrall here is discussing the explanation of a theory's *predictive success*. He writes: 'it is unlikely that the theory would have got this phenomenon precisely right just "by chance", without ... the theory's somehow or other "reflecting" the blueprint of the Universe' (ibid.). Yet, predictive success has never made a good case for traditional realism. Many predictions never materialise and the rest are always subject to negotiations. Philosophers emphasising predictive success have inevitably exaggerated the importance of abstract theoretical reasoning in science and neglected the extent of preparatory work necessary to accommodate and convince others of 'successful predictions'. Worrall's blueprint of the Universe, to whose elements scientific language is attached, is, as I have argued, an unworkable image. Cf. Collins and Shapin, 'Experiment', pp. 71-72.

[223]'Worse still', writes Putnam, 'the most successful and most accurate physical theory of all time, quantum mechanics, has *no* "realistic interpretation" that is acceptable to physicists. It is understood as a description of the world as *experienced by observers*; it does not even pretend to the kind of "absoluteness" the metaphysician aims at' ('Why there isn't a ready-made world', p. 228). For Hacking's criticisms see 'Style', p. 14. Cf. Boyd, 'The current status of scientific realism', pp. 76 f.; Fine, 'The natural ontological attitude'; Glymour, 'Explanation and realism'; and Jennings, 'Scientific quasi-realism', pp. 238-239. See also Bloor, 'The strengths of the strong programme', and idem, 'Durkheim and Mauss revisited'. Bloor argues that the truth of a proposition in no way explains our discovery of it, or its acceptance by a scientific community.

outcome of an historical process. The *explanation* of scientists' judgements therefore cannot ignore that historical process. If the 'real' status of a construct is appealed to in the course of an explanation of judgements, that reality must itself be understood as a historical construct, whose 'explanation' is of a historical, not philosophical, kind.

Second, the argument is a variation on Hacking's earlier argument from intervention: it claims that our success at putting spin on electrons, for example — at polarising them, etc. — can be *explained* only by postulating independently existing electrons having exactly those properties. (Hacking's argument was that our success *compels* us to be realists about entities.) The argument supposes that the world is physically interpreted even before scientists take an interest in it (this silent 'interpretation' is present in the world's ready-made ontology). All that scientists do is discover its mute structure through experiments that bring language and world together. Accordingly, the pre-interpreted world 'explains' why knowledge of its structure enables successful interventions. I have already argued, however, that it is incorrect to say (except rhetorically) that electrons and their properties exist entirely independently of scientists' conceptual practices. The uninterpreted world B within which scientists pursue their aims cannot be sensibly populated with those very entities and properties that are the result — and can only be the result — of constructive interactions of scientists with the uninterpreted world. Rather, these entities and properties are the furniture of world A, the physical world that scientists have created and to varying degrees mastered in world B. A metaphysical explanation of the success of scientific knowledge that seeks reasons for that success beyond constructive interpretations of the uninterpreted world necessarily ignores the interface with world B at which knowledge is produced. It postulates too much in order to explain interpretative successes that constructivists like Gooding explain at the interface of scientific inquiry and the unknown without ever postulating a pre-interpreted world. It emphasises science as a process of discovery and conceals its creative aspects. It presupposes an untenable conception of language. All in all, history of science realistically construed results in impoverished history.[224]

[224]On this see Williams, 'Should philosophers be allowed to write history?'. On the realists' neglect of *achieved transparency* in the formal works of science, see Schaffer, 'Glass works', pp. 70-71. The argument that the notable advances of modern science cannot be explained but for the (realist) supposition that scientists have become progressively better at discovering mind-independent facts is open to three standard objections. First, a great number of conventional historical accounts have been required to explain the complex phenomenon that we call the rise of modern science. The supposition about scientists' greater proficiency at unearthing facts seems trivial by comparison, and it is not clear how it fits in with established explanations. Second, the argument explains nothing: Faraday was an excellent scientist by any standard (he continues to serve as a textbook model), but as Gooding has

Before moving on to consider anti-realist objections to the constructivist conception, I would like to anticipate one last objection from the realist camp. It is that the supposed language-ladenness of scientific knowledge is a glorified version of the *theory*-ladenness of scientific observation, something that realists have long come to terms with. Constructivism is just a rehash of that old thesis.[225] In response I argue that one striking difference between language-ladenness and theory-ladenness is the importance accorded to theory. Whereas advocates of the theory-ladenness of observation affirm the priority of theory, constructivism plays down its importance. In Part 2.2 I followed Gooding in arguing that in the course of low-level exploratory experimentation consensus about facts is often reached in the absence of theory. Laboratory studies suggest that the significance of theory has been overrated in other forms of experimentation too, where — if it enjoys a clear outline at all — it is often interpreted loosely or held in abeyance.[226] *Knowledge is language-laden even when there is no theory available to further laden observations.* The emphasis on theory in philosophy of science has made 'aboutness' and the dualist conception of language almost inevitable: theories removed from the exploratory and interpretative contexts of laboratories, and imagined to subsist in crisp propositions, inevitably take on the appearance of being 'about' the world. Scientists are then seen as observers of that world, who express their observations in the light of theories that supply them with language and concepts. On the constructivist account, language makes an instrumental and substantial contribution to the building of knowledge at every level, not just that of theory.

2.3.5

Anti-realists are as likely as realists to find fault with constructivism. From their point of view the proposed conception is made vulnerable through its accommodation of cultural content as an inseparable part of scientific knowledge. Anti-realists who wish to argue that constructivism is a precarious position, somewhere on the slippery slope

demonstrated Faraday's notebooks do not corroborate the claim that he discovered mind-independent facts. Third, the argument ignores the enormous changes in the social organisation of science: more scientists are working today, they are better organised, carry more rhetorical weight, and wield more technological power than ever before.

[225]On the theory-ladenness of observation see the very different accounts of Brown, 'Naturalizing observation'; idem, *Observation and objectivity*; Maxwell, 'The ontological status of theoretical entities'; and Pinch, 'Towards an analysis of scientific observation'.

[226]See, for example, Charlesworth et al., *Life among the scientists*, pp. 31 and 33; and Gooding, 'How do scientists reach agreement', p. 222.

to anti-realism, will say that if scientific knowledge is language-laden, the objects of knowledge must be mind-dependent and cannot, as a consequence, have independent existence. The construction of scientific reality (world A) entails that it is reducible to the means by which it is constructed, which is to say linguistic resources, literary inscriptions, and other artefacts of human ingenuity and biological make-up. 'Facts' exist only where there are laboratories and inscription devices. The world is invented in language and 'knowledge of the world' is no more than knowledge of phenomena and their (linguistic) representations. Nothing can be known beyond. Constructivism about language is anti-realism or idealism in disguise. There is no half-way house between realism and anti-realism.

Anti-realist objections all presuppose the dualist conception of language. That is, they distinguish language from the world, they assume that the world in-itself is epistemically inaccessible (or non-existent), and they assign to language the primary function of being 'about' phenomena (or, in extreme cases, about nothing but itself). Because constructivism contradicts this conception of language, it cannot occupy *any* position on the traditional realist—anti-realist continuum of doctrines, and so cannot be reduced to either of its extremes.[227] Since I have already attempted to show (in Part 2.2) that constructivism cannot be reduced to dualism, I shall confine my remarks here to a brief examination of the dualistic content of anti-realist claims.

If facts about the world are of central concern to realists, language about the world is the cornerstone of many anti-realists. 'Facts', writes Alan Gross,

[227]It should be evident, in any case, that my position has little if anything in common with Kantian idealism. Kant believed that reality in space and time, which was the domain of empirical science, was nothing but a system of phenomena or appearances within some minds. What was more than a phenomenon was the mind in which phenomena appear, and those 'things in themselves' that manifest themselves to the mind through the phenomena. The truly real was thus something beyond the horizon of science. Kant's answer to scepticism amounts to giving up one kind of realist account of knowledge (knowledge of the transcendental realm), while allowing for a weaker but more defensible realist account of knowledge (knowledge of the empirical realm). *This amounts to saying that all knowledge is of appearances.* By complete contrast, to the question 'What actually exists in the world independently of experience?', I answer: 'A world without ontology'. If there is an intuitive conviction that there must be something real in the world we know, my answer is that world B is that reality, even though it is not a world with real *things*, not a world with ontology (as Kant would have it). It is pointless to bemoan the fact that we cannot know world B, that we cannot know what is ultimately real. For ontologies underlie everything we know — we can only have knowledge where an ontology has gained a foothold — and world B is ontology-free.

> are by nature linguistic — no language, no facts. By definition, a mind-independent reality has no semantic component. It can neither mean, nor be incorporated directly into knowledge. Incorporation by reference is the only possibility: candidate utterances must refer to a mind-independent reality in fact or in principle, a reference earned in a manner approved by relevant epistemic communities.[228]

Gross writes of meaning that it has no independent existence in a mind-independent world. Meaning is a feature only of language. Facts are meaningful, they are expressed in propositions, and their 'nature' is linguistic. Language creates facts by establishing or appearing to establish reference to a mind-independent world. The process gives rise to the *impression* that the world has a semantic component all of its own. Does 'incorporation by reference' at least give us knowledge of the world? Not according to Gross: 'electron', 'spin', etc., refer not to actually existing things and processes but to social and linguistic assumptions:

> The causal structure of the world to which scientific terms refer is not a physical object; it is not a relation; it is not a process. Instead it is a hypostatization of a set of social and linguistic practices.[229]

As Gross sees it, what *can* be known in science is that to which language refers directly (and which is not a mere hypostatization of socially produced artefacts), namely the world of appearances:

> What is stable in science is not the posited world of physical objects, an ontology that changes as theories change, but precisely the much-denigrated world of appearances, the only world with which science must square itself.[230]

The views expressed by Gross are echoed by most anti-realists, for whom 'appearances' are frequently synonymous with the (epi-)phenomena of the laboratory environment. So Charlesworth et al. ask:

> Do the data reveal truths about reality? All we can say is that given certain kinds of instruments, raw materials and skills, techniques, ways of opportunistic tinkering to get things to work, and social relations between scientists — this is what comes out of the system.[231]

[228]Gross, *The rhetoric of science*, p. 203.

[229]Ibid., p. 82.

[230]Ibid., p. 203. According to Mach, science is a method of describing what is directly observable, our sense-impressions or sensations, in consonance with the principle of intellectual economy. To work with non-observable entities is justifiable only if they are understood to be a sort of fiction or symbolic aid. Language attaches sensibly only to what is observable. Cf. Goodman, 'The way the world is'.

[231]Charlesworth et al., *Life among the scientists*, p. 159.

Here, language is made out to be 'about' *the output of data generation systems*. (When these happen to be conceived of as 'inscription devices', language comes very close to being merely about more language.[232]) The emphasis on experimental appearances is not unexpected given that anti-realists consider natural reality to be inaccessible yet maintain that language functions primarily referentially. Its direct objects of reference — and all that can be talked about — are laboratory 'observables'. So, Bruce Gregory confirms that:

> physics is only indirectly about the world of nature. Directly, it is talk about experimental arrangements and observations. Given a particular experimental arrangement, physicists can predict the outcome of certain measurements ... What is not given to physicists by nature, but rather is invented by them, is what they *say* about these outcomes, the language they use to talk about nature.[233]

Although Gregory is not entirely wrong in saying that the language of physics is not found ready-made but is invented (unlike Gooding he does not explore the *contexts* of its invention), he distinguishes physics from the 'world of nature' along dualist lines. He fails to see the distinction between the scientifically constructed natural world and the uninterpreted world, which in my view co-exist without the former in any way being 'about' the latter. Rather, Gregory's world has a pre-given — or at least *some* — structure, but the language of physics that purports to be about it necessarily (in his view) fails in its task.[234] It is from this idea — that language necessarily fails to reveal the true structure of the world — that Gregory's anti-realism stems. For although language is our only available means of expressing knowledge of the world, all that it can successfully be about is the world of experimental arrangements and observations. Of the world at large we can know nothing:

> The lesson we can draw from the history of physics is that as far as we are concerned, what is real is what we regularly talk about. For better or for worse, there is little evidence that we have any idea of what reality looks like from some absolute point of view.[235]

[232]For Latour and Woolgar representations are the reality-stuff of science — they express 'objective knowledge' just as they become cut off from the messy activity of the laboratory. For more on the thesis that scientific talk is about further talk see Lynch, 'Extending Wittgenstein'.

[233]Gregory, *Inventing reality*, p. 181. Cf. van Fraassen, 'The semantic approach to scientific theories', p. 112; and idem, 'To save the phenomena', p. 258.

[234]Cf. Herbert who writes that 'no matter how sophisticated our concepts, we cannot but perceive the world ... through particularly human filters. Even if we knew better, we couldn't tear off our coloured spectacles and look at *the world as it really is*' (*Quantum reality*, p. 248, emphasis added).

[235]Gregory, *Inventing reality*, p. 184.

> The stubbornly physical nature of the world we encounter every day is obvious. The minute we begin to talk about this world, however, it somehow becomes transformed into another world, an interpreted world, a world delimited by language.[236]

Our failure to obtain what Gregory calls *an absolute point of view on nature* — whose stubborn physicality we nonetheless experience every day — supports, for him, a distinction between the world of nature and the world of words. The two are epistemologically disconnected: what counts as knowledge in one is not knowledge in the other. This untenable conclusion, as with so many versions of anti-realism, results from the neglect of experimental practice. 'Talk' is in fact tied down to action and intervention in stubbornly physical processes. At the experimental interface with the uninterpreted world success depends on the inventive deployment and development of linguistic resources. This situation does not, as I have argued, produce scientific knowledge 'delimited' only by language as Gregory believes. His neglect of the stubbornly physical nature of meaning-making in experiment leads him to distinguish a language-theoretical world to which scientists are confined, from a real world which they can only desire to know. This is dualism at its most debilitating. Experiment, as Gooding notes, becomes little more than 'a means of invoking a constructed reality as a rhetorical ally' — a conclusion that 'makes sense only if, assuming that the only world that matters is the world of words, we consider only what scientists say and write'.[237]

Anti-realists who bring language/world dualism to their laboratory studies, seeking anti-realism *in practice*, likewise enclose scientists in a world of words. In the following remarks Latour and Woolgar emphasise one perceived function of language (as a test-bed of possibilities and a fine-grader, ranker, and qualifier of knowledge) at the expense of others:

> A laboratory is constantly performing operations on statements: adding modalities, citing, enhancing, diminishing, borrowing, and proposing new combinations ... [O]ther assertions can be seen to change their status rapidly, following a kind of alternate dance, as they are proven, disproven, and proven again ... These statements represent a mere fraction of the hundreds of artefacts and half-born statements which stagnate like a vast cloud of smog.[238]

Earlier in the section I argued that realists often unjustifiably project non-scientific metaphysical views onto scientists. Anti-realists have not been so bold, though popularisations of quantum mechanics (in particular) have revelled in making out contemporary particle physicists to be practicing anti-realists. Latour and Woolgar

[236]Ibid., p. 183.

[237]Gooding, *Experiment and the making of meaning*, p. xii.

[238]Latour and Woolgar, *Laboratory life*, pp. 86-87.

project their philosophical conceptions not onto scientists, but onto *stages of experimental inquiry*. They write:

> Once the statement begins to stabilise ... an important change takes place. The statement becomes a split entity. On the one hand, it is a set of words which represents a statement about an object. On the other hand, it corresponds to an object in itself which takes on a life of its own ... Previously, scientists were dealing with statements. At the point of stabilisation, however, there appears to be both objects *and* statements about these objects.[239]

Leaving aside Latour and Woolgar's correct identification of *rhetorical* practices of out-thereness, the postulation here of two stages of inquiry — the first of which finds scientists dealing in statements (*and nothing else*, besides, of course, perceptions), and the second of which finds them dealing in statements plus supposed real objects to which the statements refer — is a direct mapping of the anti-realist version of the dualist conception onto experimental inquiry. Predictably perhaps, the pages of Latour's and Woolgar's study containing these assertions are suddenly devoid of references to concrete laboratory situations.[240] The anti-realist implication that the only (genuine) stage in scientific work is that during which scientists deal in nothing but statements (and perceptions), is comparable to the realist claim that scientists deal in mind-independent entities: both are philosophical projections onto laboratory life, and both are refutable by Gooding-like analyses of experimental conceptual practice.

The anti-realist argument that everything is constructed and that there is nothing 'out there' besides appearances that corresponds or could be known to correspond to our scientific conceptions of what there is, is a confusion traceable to the dualist conception of language. Once we realise that there is no room for 'aboutness' in the experimental process (or that it is at best a rhetorical repertoire), we have no *reason* to ask the question, 'What are electrons conceptions *of*?'. To wish that the world could be known exactly as it is, is not so much a wish for the incredible, it is a wish for the incomprehensible — to despair as anti-realists have about uncontaminated natural knowledge, not a trace of which can be had, is to be in the grip of an entirely wrong-headed conception of knowledge. Nelson Goodman's recommendation that we come to terms with the idea that there is 'no such thing as the real world, no unique, ready-made absolute reality apart from and independent of all versions and visions',[241]

[239]Ibid., pp. 176-177.

[240]See ibid., pp. 176 f.

[241]Goodman, *Of mind and other matters*, p. 127. Cf. Barnes: '"Reality" does not mind how we cluster it; [it] is simply the massively complex array of unverbalised information which we cluster. This suggests that different nets stand equivalently in relation to "reality" or to the physical environment' ('On the conventional character of knowledge and cognition', p. 33).

should be complemented (and thereby considerably moderated) by the idea that there *is* such a thing as the uninterpreted world B within which scientists are constructively engaged daily in producing their versions and visions.[242]

[242]Goodman argues that 'we cannot test a version by comparing it with the world undescribed, undepicted, unperceived ... While we may speak of determining what versions are right as "learning about the world", "the world" supposedly being that which all right versions describe, all we learn about the world is contained in right versions of it; and while the underlying world, bereft of these, need not be denied to those who love it, it is perhaps on the whole a world well lost' (*Ways of worldmaking*, p. 4). But this is extreme. World B, bereft of any ontology, is an *essential* underpinning of world A. It is neither 'lost', nor a world that can be granted or withheld from those who 'love' it. Love it or not, it is the world from which we build the world we understand.

CONCLUSION

UNRESOLVED ISSUES OF LANGUAGE IN SCIENCE

In *Language as calculus vs. language as universal medium*, Martin Kusch undertakes a study of Edmund Husserl, Martin Heidegger, and Hans-Georg Gadamer. While Kusch's general aim is to compare a particular historical succession of philosophical perspectives on language, it is noteworthy for the purposes of this thesis that the three continental philosophers he discusses expressed and exploited dualist assumptions and metaphors on the occasions when their philosophies of language merged with their philosophies of science. The dualism to be found in their writings changes character from one generation to the next (Husserl taught and exercised a powerful influence on Heidegger, and he, in turn, taught and influenced Gadamer), but the language/world disjunction is preserved intact. In the following few pages, drawing on Kusch's comparison, I illustrate briefly this case of dualism outside the analytic tradition.

Husserl advocated a correspondence theory of truth (although 'correspondence' is not a term whose meaning Husserl ever clarified). Writing in 1898, he insisted that 'one truth' is dependent on 'one world':

> We will thus not accept the unclear talk of different regions of experience, of different 'worlds' (*universes of discourse*), that treat the existence or non-existence of the same object differently. The 'world' of myth, the world of poetry, the world of geometry, the real world, these are not equally 'worlds'. There is only *one* truth and *one* world.[1]

Husserl also held that natural language speakers, because their thinking is so intertwined with language, experience something of a picture relation between words and things, projecting language onto objects they encounter.[2] Husserl's belief that true knowledge presupposes one and only one world, together with his belief that strong links exist between words and things, are elements of a theory of truth which quite clearly is founded on a dualist conception of language: words are ontologically distinguishable from — but can have a correspondence to — things, and truth is possible because there is one and only one physical world, delimiting dramatically the freedom of correspondence of words to things.

When science is singled out for discussion the dualism underlying Husserl's theory of truth is especially prominent. He draws a distinction between what, on the one hand,

[1]Quoted in Kusch, *Language as calculus*, p. 38.

[2]For further explanation, see ibid., p. 65.

he sees as our usual everyday way of living and relating to a pre-scientific and pre-theoretical world, and, on the other hand, the world of science. The former, he argues, we do not encounter from the outside or objectively; rather, we live within it.[3] 'Thus', he writes, 'men as men, fellow men, world — the world of which men, of which we, always talk and can talk — and, on the other hand, language, are inseparably intertwined; and one is always certain of their inseparable relational unity, though usually only implicitly'.[4] By contrast, when it comes to doing science it is possible to step outside the 'relation' of language to the world and observe it, and if necessary modify it. In science, where the determination of truth in Husserl's view is of primary importance, the 'inseparable intertwinement' of language and world allowed for in ordinary everyday affairs is unravelled and the two sides become parties to a relation — the indistinguishable thus become distinguishable.[5]

Heidegger retains his teacher's ontological distinction between language and world but claims that because 'we' (ordinary people) cannot grasp the world independently of what he calls our 'historical language', language and world cannot be abstractly separated.[6] Heidegger's adoption of this position is a consequence of his broader criticism of Husserl's phenomenology. He argues that Husserl presupposed a subject-object distinction — an intentional relation between subject and object — that forced him to conceive of the world as object-like, as something transcendent and inessential to the (so-called) 'Being' of the subject. On the contrary, Heidegger argues, the traditional dichotomies between the subject and the object should be undercut by starting not from some particular object and the way it is perceived but from an appreciation of the world as the 'universal medium of meaning'. Thought of in this way the world will be better understood not as an object but as an 'unthematic whole',

[3]Ibid., p. 81.

[4]Ibid., p. 118.

[5]Ibid., pp. 109 f.

[6]For the phrase 'historical language' see ibid., p. 153. In Kusch's view, Husserl and Heidegger held exactly opposite conceptions of language: what he refers to as the conceptions of language as calculus and language as universal medium. According to the latter conception (which Kusch attributes to Heidegger), 'one cannot as it were look at one's language from the outside and describe it ... The reason for this alleged impossibility is that one can use language to talk about something only if one can rely on ... a given network of meaning relations obtaining between language and the world' (p. 3). In other words, the relation between language and the world cannot be expressed. By contrast, the conception of language as calculus (which characterises Husserl's philosophy) does not limit us in this way: language is a tool that can be manipulated and reinterpreted and used to discuss (in language) its very own semantical relations to the world. See pp. 130-132.

in and by which a human being lives. The world's nature as the universal medium of meaning makes it impossible for us to step outside it.[7]

Obviously these few remarks greatly simplify Heidegger's views — as earlier ones simplified Husserl's — but they illustrate a desire found in both philosophers to do away with crude distinctions between language and world, between words and their meanings, and between the perceiver and the object of perception. The distinction is never *actually* done away with. In Husserl's case it resurfaces, as we saw, when he talks about science. Heidegger returns to the distinction as follows:

> It is also a matter of fact that our simplest perceptions and constitutive states are already *expressed*, even more, are *interpreted* in a certain way. What is primary and original here? It is not so much that we see the objects and things but rather that we first talk about them. To put it more precisely: we do not say what we see, but rather the reverse, we see what *one says* about the matter.[8]

This remark may give the impression that the world just *is* in the way that we choose to talk about it, but from where Heidegger (*qua* philosopher) stands the view is broader and takes in another dimension. The philosopher can see things the way we ordinary people can, but he or she can also see *more*. The fact that ordinary folk (or, to put it loosely, what Heidegger calls *Dasein*) cannot sidestep the language they speak (for it determines the way they understand themselves and their world), to reach a world existing 'an sich', is something *Heidegger* recognises, not ordinary folk or *Dasein*. Heidegger's conception of language, at least from his privileged point of view, is thus recognisably dualist for it includes the notion of a world in-itself being referred to by an otherwise detached language. The view is privileged because it is not attainable by most, to whom it never occurs that there is any disjunction of language and world. But, Heidegger argues, it is an essential ingredient of a correct and complete philosophical view, and is therefore attainable by some philosophers at least.

Husserl expresses a dualist conception of language coupled with the idea of truth as correspondence. Heidegger retains that dualism to ensure (along, perhaps, with an air of sophistication) that the philosopher stands over and above ordinary people in vision. Third down the line, Gadamer, in *Truth and method*, writes that 'we cannot observe a language-world from without ... for there is no point of view outside the experience of the world in language from which it could itself become an object'.[9] Here, the world

[7]Ibid., pp. 154-5. Of the four-hundred-odd pages in Heidegger's *Being and time* the chapter on language is not quite seven pages long.

[8]Kusch, *Language as calculus*, p. 185.

[9]Ibid., p. 247.

appears to us through language, but the world does not appear as an object that can be *picked out* by language. 'We speak from the centre of language',[10] writes Gadamer, hence we are never able to take a standpoint outside of its limits. His treatment elsewhere of language as a *picture* is supposed to capture the inseparable unity of language and world.[11] The unity of 'representation and what is represented' he considers primary, whereas any distinction between the picture and that which its is a picture of he considers merely secondary. 'The word' for Gadamer is so intimately related to 'the thing' that he speaks of 'a language that things have'.[12] Put thus, there would appear to be little room left for a genuine disjunction of language and world. Yet Gadamer writes:

> The word is not just a sign. In a sense that is hard to grasp it is also something almost like a copy ... The word has a mysterious connection with what it represents, a quality of belonging to its Being. This is meant in a fundamental way; it is not just that mimesis has a certain share in the creation of words.[13]

The supposition that 'the word has a mysterious connection with what it represents' may seem obscurantist, but Gadamer is merely making explicit the mystery surrounding the word/world connections in the writings of Husserl and Heidegger.[14] Thus, in the progression from Husserl to Heidegger to Gadamer, dualism appears: *first* as an assumption associated with a philosophical thesis about truth, *next* as an assumption securing an extra-ordinary perspective for philosophers, and *finally* as an assumption that defeats or transcends human understanding. The dualist conception of language is not once treated as an inconvenience, a philosophical artefact to be discarded, or at least defeated by an alternative philosophical account of language and world. Are continental philosophies of science afflicted by dualism to the same degree

[10]Ibid., p. 248.

[11]Ibid., p. 251.

[12]Ibid., p. 256.

[13]Ibid.

[14]Gadamer's views (and perhaps even Heidegger's) on this matter are comparable to Frege's. The latter's arguments to the effect that we cannot attach any clear meaning to the idea of a correspondence between a real thing and some meaning, were linked to his belief that we cannot step outside language to compare language to the world. Obviously 'language' and 'world' were thought by Frege to be somehow distinguishable even though he considered the distinction useless. See ibid., p. 66. For another obscurantist construction of the language/world disjunction, see Körner who writes that according to Cassirer the philosophical analysis of symbolic representation shows that 'in any symbolic representation two moments, the symbol and the symbolised, are united into an essential unity yet stand in polar relationship to each other' ('Ernst Cassirer', p. 45).

that Anglo-American philosophies are? This is a question that goes beyond the scope of this thesis and must be left, for the moment, unresolved.

Also beyond the scope of this thesis is first-hand knowledge of the linguistic practices of working scientists — knowledge that can only come from exhaustive historical research or, more likely, participation in and observation of laboratory life. Taking a more traditional philosophical approach, in these pages I have attempted only to survey, re-interpret, and adapt to my purposes the first-hand knowledge of others. The critical examination of philosophical assumptions about language in science need not lead philosophers into laboratories, real or archival. Historians and sociologists are going that way themselves in increasing numbers, and their findings — though not free of philosophical assumptions — should help philosophers of science to gain proximity to their subject. There is an opportunity here to develop what Knorr-Cetina calls an 'empirical theory of knowledge', a theory that does not ignore the complexities of scientific research and does not involve simplistic distinctions like that between realism and anti-realism.[15]

An 'empirical' theory must take into account the relatively inconspicuous metaphysical content of everyday science. 'If', as Gooding urges, 'philosophers want to understand how a scientist or a group of scientists use experiment to make talk refer to the world and to inform scientific argument, they must pay as much attention to embodied, empirical practice as they have to theorizing about theories' — together with historians, philosophers need to examine the private history of experiment 'in order to understand how practical activity and its results are turned into persuasive arguments and experimental demonstrations'.[16] Hacking's writings are seminal contributions to an

[15]See Knorr-Cetina, *The manufacture of knowledge*, p. 3; and idem, 'Tinkering towards success'. Cf. Rorty's call for a *pragmatist* conception of knowledge, *Philosophy and the mirror of nature*, p. 11; and Shapin's 'instrumental model' of sociological explanation in 'History of science', pp. 196-198. See also the remarks of the microscopist quoted in Charlesworth et al., *Life among the scientists*, p. 118; Cantor, 'The rhetoric of experiment'; Gooding, 'How to be a good empiricist', pp. 421 f.; Latour, 'Visualization and cognition'; and Ophir and Shapin, 'The place of knowledge'.

[16]Gooding, *Experiment and the making of meaning*, pp. 271 and 137. Cf. Pickering, 'From science as knowledge to science as culture'. The functions of language shown in Table 3 (see Part 2.2) were sufficient to demonstrate the central role of language in the formation of experience. But clearly there is a need for more language-oriented research. The form it should take — tape recorders placed in laboratories? integration of sociologists into laboratories? re-enactment by philosophers of past experiments? analysis of laboratory notebooks by historians? — is less of an issue than the perceived relevance of, and will to undertake such a project.

empirical theory of knowledge in spite of their realistic leanings.[17] Besides the need for an empirical approach, the constructivist conception developed in this thesis helps focus on philosophical issues about language in science that have not been examined and remain unresolved. I discuss a number of these issues later in the section.

How does language accomplish the work of science? Does it represent pre-existing objects in nature and their relations? Do scientists as they discover new things invent new words and use them to represent relations in the real or phenomenal world? Do they use language to transcribe observations and phenomena of the laboratory? Is the performance of an experiment accompanied or followed by the epistemically secondary task of deciding on its linguistic representation? In Part 1 of this thesis I looked at common philosophical assumptions about how language functions in science. In particular I looked at expressions of language/world dualism, where what is typically assumed is that scientists discover (or imagine they discover) aspects of the physical or phenomenal world. The primary function of language is to give expression to those aspects.

In Part 2 I looked at historical studies of the development of scientific writing, formal narrative, and argumentation. I presented evidence there that the adequacy and persuasiveness of scientific language has to be understood as a historical development. Language is related to conventional, rhetorical, metaphysical, and social aspects of scientific culture. Reaching consensus about observations and beliefs has always depended on reaching consensus about how to *express* (how to talk about) observations and beliefs: 'solutions to the question of what was the correct language for scientific intellectuals were simultaneously solutions to the question of what natural reality was like' (Shapin).[18] The fierce epistemological and simultaneously political disputes mentioned by Schaffer were signs of a struggle to establish 'proper' languages for natural knowledge. In our own days (as in days prior to ours), the language of a published experimental report is what it is to use scientific language correctly, to be competent in this or that domain. When language ceases to be regarded as especially problematical it becomes *ipso facto* transparent, or appears 'naturally suited' to the task at hand. Transparency is always an *achieved* result in the face of opposing pressures.

From historical perspectives on language I turned to the investigation of scientists' day-to-day interpretative practices. Laboratory studies have focused on the transformation

[17]See especially Hacking's thesis on the dependence of phenomena on the actions of experimenters, *Representing and intervening*, pp. 220-232; and *idem*, 'The self-vindication of laboratory science'.
[18]Shapin, 'Robert Boyle and mathematics', p. 25.

of laboratory activity into scientific facts and literary products. These studies support the idea that at higher and more public levels of expression language traditionally functions in science to espouse rhetorically the metaphysics of language/world dualism and promulgate a rhetoric of objectivity and out-thereness. Laboratory studies thus support the idea that the language/world disjunction should be regarded not as a viable metaphysics but as one among many available rhetorics. In the published scientific report objectivist discourse embodies the appearance of consensus. It is a discourse that aids, in Gooding's words, *the interpretation of the manipulation of objects*, and has the capacity and is normally employed 'to describe [objects and processes] of which it could later be taken for granted that everyone would perceive [them] in the same way' — preferably as being 'out there'.[19] Description, explanation, conviction, etc. — presumed basic by dualists — are all *made* functions historically rooted in controversy and metaphysics.

Following the preliminary historical and laboratory deconstructions of dualism, I sought to formulate a constructivist conception of language drawing on Gooding's account of the experimental construction of electromagnetic knowledge. His account dealt with uses of language at one of the most basic levels of research, where scientists engage the uninterpreted world and construe experience, fixing phenomena and making them communicable and reproducible. I argued that language should be regarded as a tool that assists the *making* of natural facts, meaning, and consensus (a view consistent with the philosophical tradition which treats language and its uses as a system of actions).[20] Human agency is implicit in that which uses of language in laboratory practice achieve. Thus mental processes and material manipulations are complementary: practice, language, arguments, interests evolve together (and so world A evolves). This interpretation contrasts with the dualist conception of the role of language as a kind of transcription device or symbolic system.

As summarised in Table 3, language contributes to the construction of scientific knowledge on a number of distinct levels. It is used to establish conventions, to conform to pre-existing conventions, to negotiate criteria and domains, to establish or question consensus about facts, to give meaning to novel observations, to impart inferential structure to narrative, and to suggest nature's unmediated involvement in the making of theory. It also 'describes and orders experience in relation to existing systems of classification and belief and enables argument about that order'

[19]Gooding, *Experiment and the making of meaning*, 87.

[20]Cf. Wittgenstein, *Philosophical investigations*, §§ 7, 11, 23, 25, 31, 491, and 505.

(Gooding).[21] It defines fields and rules of discourse to which newcomers must conform their aims and modes of expression: 'It was the hygienist movement that defined what was at stake, prescribed the aims, posed the problems, demanded that others should solve them, distributed praise or blame, and laid down priorities ... The Pasteurians translated these stakes and rules into their own terms, but without the hygienists, it is clear that very little would have been heard about them. The Pasteurians would have done something else' (Latour).[22]

The argument that scientists investigate and construct their world with language, and that as a result properties of the world cannot be sensibly separated from the linguistic resources that enabled their making, contradicts both the realist view that scientists discover independently existing properties of nature, and the anti-realist view that scientists are limited to the contents of appearances. Realists elevate the mundane and commonplace rhetoric of the language/world disjunction to a metaphysical level so as to secure the objectivity of the world. Anti-realists, for whom language reaches no further than appearances, fail to notice that language is tied down to practices and skills that engage the uninterpreted world. To move beyond these views it is necessary that philosophers of science take a 'linguistic turn', and (in Jan Golinski's words),

> relinquish the view that the language of science ... is simply a means of representation, separable from and irrelevant to the 'content' of science. Such a move carries with it the implication that [philosophers] should no longer seek to penetrate *through* the linguistic practices of scientists, to isolate ideas and conceptual structures in their minds, and should instead start to investigate those verbal and textual practices themselves.[23]

The reorientation away from mental contents and theories and towards social context and language-use has a number of proponents among philosophers. Rorty argues in defence of the 'holism' of Sellars and Quine that it is the product of a commitment to the thesis that justification is not a matter of a special relation between ideas (or words) and objects, but of *conversation* (by which Rorty means the negotiation of existing standards according to which belief is justified), and social practice.[24] Both Sellars and Quine, writes Rorty, employ arguments involving the premise that we understand knowledge when we understand *the social justification of belief*: we have no need to view knowledge as accuracy of representation.[25] Once social practices of negotiation

[21]Gooding, *Experiment and the making of meaning*, 203.

[22]Latour, *The pasteurization of France*, p. 25.

[23]Golinski, 'Language, discourse and science', pp. 111-112.

[24]See Rorty, *Philosophy and the mirror of nature*, p. 389.

[25]See ibid., p. 170.

(or conversation) replace logical practices of confrontation of words and things, the notion of the mind as 'Mirror of Nature', which Rorty finds unsuited to this day and age, can be discarded. He writes:

> When Sellars's and Quine's doctrines are purified, they appear as complementary expressions of a single claim: that no 'account of the nature of knowledge' can rely on a theory of representations which stand in privileged relations to reality. The work of these two philosophers enables us ... to make clear why an 'account of the nature of knowledge' can be, at most, *a description of human behaviour*'.[26]

Shapin's and Gooding's arguments, although not explicitly accounts of the nature of knowledge, do nonetheless gravitate towards 'a description of human behaviour'. Matters of fact, consensus, justification, conviction, meaning, and so on, are presented as circumstantial and revisable creations, not fixed elements or relationships in the metaphysics of language/world dualism. To understand their nature well it is necessary to move off the grounds of traditional philosophical argument and into the social/experimental contexts in which scientific knowledge is made. An investigation of these contexts brings us (philosophers as well as historians) face to face with 'verbal and textual practices'. The social underpinnings of scientific knowledge (which Rorty only vaguely refers to) can be found preserved in these practices. I have presented a number of reasons to dissuade anyone from wanting to 'penetrate through' them (as Golinski puts it). One reason — of philosophical as much as historical significance — is advocated, as I mentioned in Part 2, by Shapin:

> Just as the [material, literary, and social] technologies operate to create the illusion that matters of fact are not man-made, so the institutionalized and conventional status of the scientific discourse that Boyle helped to produce makes the illusion that scientists' speech about natural reality is simply a reflection of that reality. In this instance, and in others like it, the historian has two major tasks: to display the man-made nature of scientific knowledge, and to account for the illusion that this knowledge is *not* man-made.[27]

Here, what could be called a refutation of the thesis that language is 'about' the world, proceeds not from a consideration of philosophical arguments and counter-arguments, but from a kind of description of human behaviour, primarily of seventeenth-century behaviour aimed at overthrowing linguistic and narrative conventions and institutionalising new ones: the fact that twentieth-century scientific papers are rarely if ever written with the depth of circumstantial detail which Boyle's reports contained is evidence not of a contemporary language that perspicuously represents the world as it is (or as it appears to be), but of a language whose various uses are steeped in conventions, now institutionalised as part of a tradition, enabling metaphysical presuppositions about the

[26]Ibid., p. 182. Cf. Goodman, *Ways of worldmaking*, p. 170.

[27]Shapin, 'Pump and circumstance', p. 510.

expression of experimental experience, observational evidence, narrative argumentation, consensus, etc., *to be rendered invisible.* Within this influence of tradition lie, I believe, suggestions for future philosophical research or, to put it otherwise, some unresolved philosophical issues of language in science.

Briefly put, the unresolved issues arise from the possibility that language *imposes constraints* on scientific thought, action, and the expression and pursuit of scientific knowledge.[28] Issues of linguistic/literary constraint can be seen as the flip side of issues I have been concerned with so far (my main concern has been with how language complements and *enables* scientific thought, action, etc.). The idea that language *constrains* the ways in which we think and act (besides, of course, influencing what we think and do), is a relatively old one. Unlike many philosophical ideas it even enjoys a degree of popularity, rooted in the dictum that 'the message' is not neatly distinguishable from 'the medium' (or from linguistic codes). Milman Parry, Eric Havelock, Frances Yates, Marshal McLuhan, Elizabeth Eisenstein, Walter Ong, Noam Chomsky, and Jack Goody (to mention the most influential) have all in different ways elucidated and defended the proposition that changes in non-abstract technologies of language and communication (oral, written, printed, etc.) have caused major changes in abstract thought, marking transitions in consciousness, metaphysics, rationality, representation, etc., from the ancient to the classical, the classical to the medieval, and the medieval to the modern world.[29] More recently, historians of science, such as Thomas Broman, Lisa Rosner, Ludmilla Jordanova, Michael Mulkay, and Charles Bazerman have argued that a close relationship exists between formal structures of written 'genres' and their intellectual contents: genres of writing and scientific theories, argues Broman, 'develop together and become established in particular historical circumstances'.[30] (Genres are not, of course, non-abstract technologies — they are

[28]Pickering and Stephanides ask: 'We expect the material world (in experimental practice) and other people (in sociotechnical practice) to resist our designs. But what might count as resistance and thus produce the characteristic dialectic in conceptual practice is less clear. How can symbols, marks on paper, thoughts, get in our way? How can the workings of the mind lead the mind itself into problems?' ('Constructing quaternions', p. 141).

[29]See Parry, *The making of Homeric verse*; Havelock, *Preface to Plato*; Yates, *The art of memory*; McLuhan, *The Gutenberg galaxy*; idem, *Understanding media*; Eisenstein, *The printing press as an agent of change*; Ong, *Ramus, method, and the decay of dialogue*; idem, *Orality and literacy*; Chomsky, *Language and mind*; Goody, *The domestication of the savage mind*; and idem, *The interface between the written and the oral.* On linguistic/theoretical constraints affecting Faraday's work, see Gooding, *Experiment and the making of meaning*, pp. 80 and 234.

quite abstract. Yet the extension of the argument, which Eisenstein founded on the concrete technology of the printing press, to include abstract technologies or conventions of writing, seems almost inevitable in retrospect.)

Here, a parallel should be noted between efforts to explain how genres of writing 'structure' the ideas contained in them, and contemporaneous efforts (by Willem Hackmann, Derek Price, Peter Galison, Gooding, and others) to explain what Galison refers to as the 'connection between the instruments we build and the arguments and entities we construct from them'.[31] In both cases a technology (scientific language, scientific instruments) traditionally conceived of as merely instrumental is shown to impose its own *unconscious* cognitive/behavioural constraints.[32] As far as I am aware, only Hacking among philosophers of science has advanced a comparable argument, albeit with less of an emphasis on language. In an article entitled '"Style" for historians and philosophers', Hacking urges cooperation of the two academic communities to determine what he calls 'styles of reasoning' in the history of science. The results, he believes, should prove philosophically enlightening:

> My styles of reasoning, eminently public, *are part of what we need to understand what we call objectivity.* This is not because styles are objective (i.e. we have found the best impartial ways to get at the truth), but because they have settled what it is to be objective (truths of certain sorts are just what we obtain by conducting certain sorts of investigations, answering to certain standards) ... there are neither sentences that are candidates for truth, nor independently identified objects to be correct about, prior to the development of a style of reasoning.[33]

(We are reminded here of Kuhn's paradigms and of their potential, not fully articulated by Kuhn, to impose unconscious constraints on thought and action.[34]) A style of

[30]Broman, 'Reil and the "journalization" of physiology', p. 17. See also Rosner, 'Eighteenth-century medical education'; essays collected in Jordanova (ed.), *Languages of nature*; Mulkay, *The word and the world*; and Bazerman, *Shaping written knowledge*. The argument in Broman's article is suggestive but rather weak as it stands — see Zahar, 'Review of Dear'.

[31]Galison, 'Bubble chambers and the experimental workplace', p. 356. See also, Hackmann, 'The relationship between concepts and instrument design'; idem, 'Scientific instruments'; Price, 'Philosophical mechanism and mechanical philosophy'; Gooding, 'Magnetic curves'; and Pinch, *Confronting nature*, p. 212.

[32]I employ the term 'unconscious' to denote a set of constraints that scientists are normally unaware of and do not employ in their arguments. An example of a 'conscious' cognitive constraint would be the set of readings obtained from a reliable instrument in the course of an experiment.

[33]Hacking, 'Style', p. 4, emphasis added, and (for the final sentence of the quotation) p. 11.

[34]See Part 1.2.4 of this thesis.

reasoning may perpetuate a language — a genre of writing, a mode of expression — that is soon recognised as characteristic of that style. If the aforementioned exponents of *linguistic* constraints are correct, it would seem that the converse must also hold, namely, that language associated with a particular style of reasoning — because it forms a significant part of the gamut of constraints that limit what individuals under the style's influence can think, say, or do — might itself contribute to the perpetuation of that style.

How do styles of reasoning (paradigms, research programmes and so on) rise, change, and fall? Philosophers have looked at mechanisms of scientific change but have usually focused on the theory/experiment relationship and have not explored possible linguistic obstacles to change. Significant cognitive obstacles have been regarded as *theoretical* obstacles only (yet as meanings change *language often stays the same*). It would be interesting to explore the kinds of constraints operative below the level of theory. In Part 1.4 I described a field destitute of widely shared conventions relating to language and the expression of knowledge. It was a field in disarray, and it was to remain in disarray for many decades after the introduction of periodical communication. Any proposed general theory of disease had to overcome 'pre-theoretical' obstacles — *linguistic constraints* — of this kind for it to stand a chance of success. Besides specifically linguistic constraints, the possibility of scientific skilled practice in general acting as a cognitive constraint also presents an issue to be explored. I have mentioned arguments to the effect that scientific communities and concepts often develop around particular instruments rather than vice versa. It would seem that instruments and the embodied skills relating to their use may constrain scientific thought and action as much as set *theoretical* habits have been thought to (and, as I wish to maintain, set linguistic habits).[35]

This brings me to the unexplored issue of the relation of language to other practical skills, or its role in the *acquisition* of practical skills and tacit knowledge. Apprenticeship to scientific culture obviously interrelates language and other activities (the 'ideology of the textbook' is imparted at an early stage). An empirical philosophy of science that has correctly sought to deflate the role of theory in scientific practice must

[35]In the end, one could list a number of constraints that have yet to be explored and be interrelated in philosophy:

(i)	empirical:	recalcitrance of nature;
(ii)	bodily:	recalcitrance of acquired skills;
(iii)	social:	recalcitrance of colleagues;
(iv)	intellectual:	recalcitrance of 'styles';
(v)	evidential:	recalcitrance of conventions of reasoning;
(vi)	linguistic:	recalcitrance of meanings; ... etc.

be prepared to explore the workings of language at this level. An investigation along these lines is likely to throw further light on role of agency in the creation of natural knowledge. One last issue: if we think of Boyle's 'literary technology' as a kind of 'metaphysics of language', it would be philosophically interesting to explore how that metaphysics has changed over the centuries. Might differing metaphysics of language underlie some of the more notorious debates of quantum mechanics and contemporary physics?

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B. Nineteenth-century medical papers[1]

Abbreviations:

EMSJ	Edinburgh medical and surgical journal.
LMG	London medical gazette.
LMPJ	London medical and physical journal.
LMSJ	London medical and surgical journal.
LMR	London medical repository.
M-CT	Medico-chirurgical transactions (London).
MPJ	Medical and physical journal (London).

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[1]See Part 1, footnote 223, for an explanation of the format of the following citations.

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