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Experiment and the Making of Meaning

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Human Agency in Scientific Observation and Experiment

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Preface

> ... the topic of 'meaning' is the one topic discussed in philosophy in which there is literally nothing but 'theory' – literally nothing that can be labelled or even ridiculed as the 'common sense view'.
>
> Putnam, 'The Meaning of Meaning'

This book explores some truths behind the truism that experimentation is a hallmark of scientific activity. Scientists' descriptions of nature result from two sorts of encounter: they interact with each other and with nature. Philosophy of science has, by and large, failed to give an account of either sort of interaction. Philosophers typically imagine that scientists observe, theorize and experiment in order to produce general knowledge of natural laws, knowledge which can be applied to generate new theories and technologies. This view bifurcates the scientist's world into an empirical world of pre-articulate experience and know-how and another world of talk, thought and argument. Most received philosophies of science focus so exclusively on the literary world of representations that they cannot begin to address the philosophical problems arising from the interaction of these worlds: empirical access as a source of knowledge, meaning and reference, and of course, realism. This has placed the epistemological burden entirely on the predictive role of experiment because, it is argued, testing predictions is all that could show that scientists' theorizing is constrained by nature. Here a purely literary approach contributes to its own demise. The epistemological significance of experiment turns out to be a theoretical matter: cruciality depends on argument, not experiment. Meanwhile, empirical access to nature and the predictive success of science remain mysterious.

Philosophers of science like mysteries less than scientists do. Some affirm instead the social and cultural basis of the reality referred to in scientists' talk. From this they infer the constructedness of that reality. This pursues the linguistic turn to its methodological conclusion. Philosophy of science becomes a form of literary criticism. Others assert the reality of the natural world to explain the fact that scientists' talk often refers to things and processes in the world, and to preserve the idea that scientific theories approach closer to the truth about nature. Both responses caricature science by their neglect of its practical, procedural, inventive, and informal aspects. The literary residue of science is of course where concepts, arguments and demonstrations are found. It seems self-evident that the radical thrust of a thought-experiment by Einstein or the elegance and generality of Schrödinger's wave mechanics are purely intellectual achievements. This does not make science a purely verbal activity. To treat its most elegant theoretical achievements as if these epitomize the whole of science demeans the intellectual dimension as well as the others, because it fails to address the problem of how embodied intellects grapple with a real world. Besides its philosophical interest, that problem is important to science education as well.

Bacon and Galileo wanted a new science which was practical – by contrast to the old sciences which they characterized as literary because their sole authorities were texts. It is ironical that while many philosophers admire science because it *is* empirical as well as rational, philosophical practice confines it to the literary view that Galileo rejected. Scientists' own accounts of their work do of course emphasize the creativity of intellect and the rigour of argument, and they usually subordinate material practices to intellectual ones. These narratives are accessible to philosophy whereas the tacit and pre-verbal context in which observers get to grips with the phenomenal world, is not. To be sure, we can understand the meaning of arguments in research reports without delving into the practical context from which some anomalous new possibility has emerged. Nonetheless, an explanation of the persuasiveness of these literary forms and what their success has to do with experiment, will remain elusive as long as the enabling role of experiment is ignored. Every scientist since Galileo has known that experiment is a demonstrative resource. Is experiment no more than a means of invoking a constructed reality as a rhetorical ally? An affirmative answer makes sense only if, assuming that the only world that matters is a world of words, we consider only what scientists say and write.

Many creative activities in our culture influence imagination and shape experience, so it is hardly surprising that science should do so. I find it implausible that an activity construed as literary and cerebral should have power over nature as well. The importance and elegance of reasoning and argument in science is not in question. The problem is that the traditional divorce of mind from body and thought from the world leaves the influence of intellect – even within science – mysterious and surprising. The dualism and epistemic individualism that have dominated modern philosophy since Descartes and Berkeley render it incapable of addressing the problem of empirical access. Modern philosophy cannot understand how what scientists do gives them power over nature as well as our imaginations. It lacks a plausible theory of observation.

This book proposes an alternative view based on aspects of scientific work largely neglected by modern, especially analytical, philosophy. These are the agency of observers and the way their observation of nature is mediated by their interactions with each other, with their instrumentation and with the material world. Empirical access is a cognitive and a social process. By *cognitive* I mean that it has to do with apprehending and articulating experience in a general way. This must include, for example, an observer's proprioception, because that enables the sensorimotor learning on which the representation and communication of new experience depend. I shall show that experimenters learn by engaging a real and often recalcitarant natural world. The view must be *social* because what they learn owes its significance and often its very expression to encounters with other, often recalcitrant, observers. Establishing the existence of a phenomenon presupposes learning how to communicate the possibility of experiencing it. The social and the cognitive are often interdependent because making experience intelligible is an active process in which observers often need to make sense of their own behaviour in relation to phenomena in order to communicate it to others.

I want to show how scientific observers make what Quine calls a semantic ascent. 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.

To push at the boundaries of language in this way is to challenge some familiar epistemological assumptions. These are expressed in the individualistic and mentalistic view of the scientist as a knowing subject. This view takes the primacy and autonomy of individual experience for granted. It assumes that the only agency that really matters is reasoning – the rule-guided manipulation of symbolic objects – and insists that scientific reasoning must resemble formal argument. These assumptions are hardly controverted by the narratives to be found in histories, in science textbooks or even in research papers. With the clarity of hindsight these accounts emphasize the role of declarative knowledge and verbal argument. Experiment produces natural facts that test predictions. Human agency disappears from such accounts so that nature and logic can be centre stage. But these are birds-eye views, drawn after the terrain has been explored and the route mapped out. They do not explain the provenance of experimental results but instead construct arguments that bring experiments into an evidential relationship to hypotheses. They pass over the work which brings the 'two worlds' together. Such work is the main subject of this book. Philosophers rarely stray beyond the ordered, reconstructed world of these narratives and the histories based on them. Their caution reinforces a false philosophical view of the relationships between observers, their theories and the world. Philosophers of science need to recognize that the structure of evidential argument is worked out during research and that this process includes writing structure into experimental narratives. Constructing narratives also requires know-how not recognized by formal methodology.

It is important to understand the processes by which scientists engage the natural world and construct evidential arguments about it. The recovery of know-how is essential to cognitive science and computational approaches to discovery. Recent attempts to build artificial discovery programs highlight the incompleteness of our present understanding of *natural* intelligence – the use of experiment to create new possibilities for experience which scientists and inventors develop as theories and realize as technologies. For scientists, uncertainty is one of the doorways into the exploration of possibilities. By contrast, much of philosophy of science has been in such retreat from uncertainty that it has little to offer the student of creative processes in science and technology. The examples in this book recover something of the *process* of observing, experimenting and theorizing about experiment. The first part discusses the experimental development of a new way of thinking and talking about a novel phenomenon, electromagnetism. I show how, by making new images, objects, words and practices, scientists such as A. M. Ampère, J. B. Biot, Humphry Davy, and Michael Faraday brought new perceptual information about the world into their thought and talk about nature. The concepts they introduced also affected the existing frameworks of concepts and practices. Their attempts to make sense of this new phenomenon led to a radical alternative to traditional ways of imaging and theorizing a large group of natural phenomena – electric and magnetic forces.

The six chapters of Part One emphasize the role of interpretative techniques in extending the capacity of language to describe and explain new domains of natural phenomena. Where Part One focusses on the practical 'stage-setting' for verbal and pictorial representations, Part Two explores how phenomena move from the context of experimental practice to the context of argument, changing theory as they do so. In chapter 8 I extend the analysis first to test theories with large, complex experimental systems and then to thought-experimentation. This shows both thought- and real-experimentation share important features I identify as participation and the refinement of practice. Chapter 9 traces the construction of a thought experiment in the pages of Faraday's laboratory diary, showing how this enabled him to reinterpret the meaning of some fundamental experiments. I sketch the interplay of practice, theorizing and metaphysical categories in chapter 10. These two chapters show Faraday, one of the worlds greatest experimentalists, articulating experiments into evidential arguments. With the transformation of exploratory experiment into demonstrative ones comes a change in the status of techniques, phenomena and in the reality of effects, relationships and entities. Statements about these empirical facts become meaningful in a broader sense – they have been made part of a cultural repertoire of facts, arguments and theories.

My approach to the practice of observational science involved repeating observations and experiments to recover the contingency and uncertainty of real experiment, and to bring out the tacit knowledge and skill concealed behind the laboratory records. I represent these aspects in maps of experimental procedures. This approach shows the traditional epistemology of observation to be mistaken. What should replace it? Tradition has it that observers are passive receptors of stimuli from an external world. This means that they must manipulate representations before they can manipulate objects. I argue instead that mental processes and material manipulations are complementary: the agency whereby observers construct the images and discourse that convey new experience embraces both. Where the received epistemology privileges the individual's experience of the world I shall argue that experience of the natural world must be construed within a world of other observers. Experiment emerges as a practical form of argument and this implies a different view of theory.

Theorizing – especially through thought-experiments – is often eminently practical. The force of scientific argument depends on this in ways that philosophers have failed to see. Here the distance between epistemology and science calls for a procedural turn in philosophy of science. Whereas philosophers from Hume to the logical empiricists dismissed knowing-how as irrelevant to knowing-that, I argue that their interdependence is just as necessary to defending empirical claims as it is to explaining their origins.

Looking behind the literary residue of science makes it possible to argue a modestly realistic view of the objects of scientists' discourse. The problem of reference is traditionally cast in ontological terms that reflect the dualistic view of perceptual access. The problem was therefore to show that observers are interacting with an independent world, while demonstrating that independence without relying on the very observer-world interaction that had produced the representation the observers wish to construe realistically. The problem is insoluble in this form because any means of accessing the world presupposes representations about which this same question can be asked. Access presupposes representation and there is no independent access to the observer-world relation. One response is to argue that the relationship of the world to descriptions of it must be taken as primitive. I argue a quite different view, that experimentalists engage the natural world and construe their experience to *create* the correspondence of representations to experiences. Their construals are ephemeral. Some survive and gain currency as useful descriptions of things in an observational situation. Their correspondence to aspects of the world is therefore a made relationship in which both matter and mind –or nature and culture– are implicated. How does the ontological distinction between things and their descriptions come about? Human agency is written out of experimental narratives so that the world of experience and the world of words are seen to be independent.

Study of the fine-structure of experiment implies that traditional empiricist views of science should be abandoned. The view of experimental processes developed here explains how observers confer meaning upon objects, practices, and phenomena as well as words. In chapters 7 and 8 I propose a pragmatic alternative to realism about the referents of words. This replaces correspondence as traditionally construed by a notion of correspondence as the mutual approach of the products of experimental and linguistic practice. This approach is 'asymptotic' rather than convergent. Whereas convergent realists believe that the trajectories of theories will eventually meet the world, asymptotic realism requires only that the trajectories of material and conceptual aspects of practice come sufficiently close to satisfy competent observers that their talk and thought has engaged the world. The image of a trajectory is itself a metaphor, enabling us to show that the approach of language and the world is a mutual approach brought about by an historical process. This process is the refinement of practice through action in and upon a material and social world. Mutual approach is neither arbitrary nor inevitable. The process brings about change in both verbal and material aspects of practice. What scientists take to be real in their talk depends on what they can take to be meaningful in practice.

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Having studied the literary traces of Faraday's electromagnetic experiments in manuscripts I learned about the practicalities of real experimentation through repetitions done at Bristol University and at the Royal Institution, London. I thank Jane Leigh and Susan Perrett (then students at Bristol University) for their ingenuity and enthusiasm, Rodney Hillier for making the project possible, and Professor John Ziman, who first suggested repetition of experiments from Faraday's laboratory notes as a physics project. I also thank Bill Coates and Bryson Gore of the Royal Institution for help with related experiments on electromagnetism.

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Bath, September 1989

PART 1

Agency in Observation and Experiment

CHAPTER ONE

The procedural turn

'Now he's seeing it like *this*', 'now like *that*', would only be said of someone *capable* of making certain applications of the figure quite freely.
The substratum of this experience is the mastery of a technique.

Wittgenstein

Logic itself has now entered a stage where 'anthropological' considerations (finitism) play an important role. Altogether the scientific enterprise seems to be much closer to the arts than older logicians and philosophers of science (myself among them) once thought ...

Feyerabend

1.1 Agency in observation and experiment

It is inevitable that language has, as Ian Hacking put it, mattered to philosophy.[1] It is not inevitable that practices – especially extra-linguistic practices – have mattered so little. Philosophy has not yet addressed an issue that is central to any theory of the language of observation and therefore, to any theory of science: how do observers ascend from the world to talk, thought and argument about that world? The importance of practice is sometimes noticed by philosophers and historians of science.[2] The more historically minded have persued it into the experimental coal-face where new experience is mined.[3] One reason for neglect of observational practice is that scientists' own accounts of experimental work support the disembodied view that philosophers tacitly endorse. Human agency is written out of these accounts; at best it appears in an appropriately deliberative and methodical role. This would seem to justify the philosophical practice of rendering observation in the passive form 'X was observed in order to ...', as if observers have eyes, minds and theories but never hands in the matter. A second reason is that concepts and theories are found in texts: they are easy to write and talk about. The literary view of scientific concepts suits the linguistic turn in philosophy and conforms to wider cultural assumptions about the priority of head over hands. Yet many scientists spend most of their time solving problems of a different order, in a manner that is procedural and involves manipulations in the material world as well as the world of concepts.

Unreflective philosophical practice allows the scientist fewer resources to investigate the world than non-scientists have. As Ron Giere points out, psychologists allow rats greater ability to map their environment than some philosophers recognize that scientists have.[4] The image of exemplary scientific practice has dominated philosophical approaches to theories of empirical access for many decades, so it is not surprising to find such high expectations placed upon the scientist. The new naturalism in philosophy recognizes that people learn by active intervention in a concrete world of objects and other people. Scientists are people. In this book I shall look at the procedural and often prelinguistic stage-setting they use to get their concepts off the laboratory bench and into their language. I shall drop an article of faith implicit in philosophical methodology, the belief that because scientists have special methods they do not also need the same ways of interacting with the world as the rest of us do.

Here the unusual and pathological can expose what is taken for granted in human perception. Observation is traditionally taken to be passive and perception is taken to be first and foremost, visual perception. This makes scientists disembodied, like Oliver Sach's patient, Christina. She retained her mental faculties, the use of her muscles and her senses but, having lost the so-called 'sixth sense' – proprioception – Christina simply did not know what she was doing in the world.[5] Richard Gregory reports a different example of the importance of doing to seeing: the case of Sidney Bradford. Blind from infancy, his sight was restored by corneal grafts after fifty years. Bradford could see immediately only those objects he had already explored by touch; prior tactile experience enabled visual perception.[6] In chapters 2 – 6 I show how agency in a material environment enabled perceptual experience that scientific observers such as Biot, Davy and Faraday reported as visual observations. The reduction of complex perceptual experience to visual experience shows that a reconstructive process is at work in writing experimental narratives. We shall see that action enabled seeing (and thinking), yet in writing their accounts, the focus on visual perception juxtaposes seeing and thinking more directly than they had been during the discovery process. Later in this chapter I establish that such activity and the experiences it produces are socially situated. First I want to grasp the nettle of reconstruction.

1.2 Discovery, reconstruction, and justification

Thinking and its interaction with the material world have a reticular, dynamical quality. This rarely survives translation into the narratives in which we learn about experiments.[7] This commonplace is important for philosophical concerns about realism, scientific method and the nature of perceptual access. It is also important for the new interest in discovery prompted by computational approaches. Philosophers and AI researchers tend to consult only published papers or text-books. In these, research processes of science are reconstructed into the linear order of experimental narratives.[8] Active, concrete processes are barely glimpsed through such texts, which do not convey the procedural, skilled aspects of theoretical and experimental work. After all, their purpose is to ensure that the contingencies and messiness of empirical work are precisely situated, if they appear at all. This makes it all too easy for philosophers to foreground the logical norms found in the methodological canons of a scientific field and to underestimate the amount of qualitative, constructive work that enables quantitative precision (– as I show in chapter 7). Encountering science in its ordered, literary mode and having further reduced this to logically-ordered verbal and mensurative activity, it is inevitable that philosophers should find empirical access mysterious.

What structures do experimental processes have? Rather than insist they conform to inductive or deductive logic, we can consider the source of the argument-structures found in scientific narratives. That scientific reasoning necessarliy reduces to logical argument cannot be taken for granted. That it has been taken for granted reflects the neglect of actual processes of natural reasoning, which is not confined to the so-called context of discovery. The main weakness of mainstream philosophy of science, from the standpoint of historical, sociological and cognitive studies, is its imposition of a sequential structure – the linear form of propositional argument – upon all reasoning. The failure of computational approaches to deliver real discovery programs – to make discoveries with data that is as 'raw' as the stuff scientists work on – is largely due to the fact that most work with the impoverished notion of discovery still favoured by analytical philosopy.[9]

The one pass fallacy

Thomas Nickles has argued that most philosophies of science are guilty of the "one-pass fallacy". The fallacy is to take scientists own narratives as realistic accounts of a single, linear 'pass' or sequence of operations, ignoring the extent to which scientists' accounts are reconstructions rather than records. Reconstruction is something that scientists "must do, consciously or not, in order to apply old results and techniques to new problems at the frontier and to model one problem solution on another".[10] They iron the reticularities and convolutions out of thought (and action) to make a flat sheet on which a methodologically acceptable pattern can be printed. Nickles argues that ignorance of the self-transforming character of accounts causes one-pass models to commit the genetic fallacy – "the mistake of thinking that its conditions of origin determine forever the character or 'essense' of a thing". In fact, the evidential and logical status of a result changes with each pass or reconstruction, so that results introduced under procedures appropriate to discovery may assume a different role in the context of justificatory logic. The case of electromagnetism shows that even when the importance of new phenomena is established quickly, the arguments that specify their evidential significance take some time to develop.

Grasping the nettle of reconstruction means recognizing that all accounts – even those made as the process went on – involve an element of reconstruction. Science is both reflective (moves are considered and criticiced) and recursive (the meaning of earlier steps may change as a result of later moves and their outcomes). Reconstruction is needed to produce an account ordered enough to enable further action, the communication of what is going on and the redefinition of problems. This has already occurred to some extent in the accounts we find in manuscript notes, records, correspondence, and drafts of papers. This sort of accounting involves a type of reconstruction I shall call *cognitive*. It is inherent in the process of generating accounts that make experimental behaviour intelligible to any of the actors involved. I discuss examples in chapters 2 and 5. Cognitive reconstruction can be distinguished from *demonstrative* reconstruction that scientists do to generate arguments from accounts of particular experiments. These two activities are interactive: for example, Lavoisier's drafts of his papers on respiration show that concepts are being articulated alongside arguments. Although the need to develop an argument may become more important than the need to describe, no clear divide between discovery and justification can be discerned in the successive versions from early drafts to final report.[11] Writing up is part of the discovery process. We can distinguish this sort of development (though not too sharply) from *methodological* reconstruction of an account. Here the primary concern is to make the evidential argument conform to the methodological canons of a particular experimental discourse. An example is Millikan's creative selection of data in his oil-drop experiments.[12] Medawar's well-known complaint about the fraudulence of scientific papers identifies other reconstructions of this kind.[13] He argued that the explicitly inductive form of research papers distorts the discovery process. Medawar took this to be deductive and fallibilistic. However, Ampère's reconstruction of the history of his electrodynamic experiments shows that this can be rhetorical in intent without favouring an inductive methodology (see chapter 2). A more extreme example of *rhetorical* reconstruction would be Galileo's exaggeration of numerical values in his account of the famous leaning tower experiment.[14] Another type of reconstruction streamlines the discovery process to make every step as transparent and self-evident as possible. This enables the exemplary demonstrations of science texts, which are obviously not intended to describe the vicissitudes of actual research. Rewriting (once) actual examples has a didactive as well as a demonstrative role, so I call it *didactic* reconstruction. This enables the dissemination of what Kuhn called exemplars.[15]

Textbooks and monographs are the main source of the image of experimentation that dominates analytical philosophy of science. This made it easy to formalize scientists' deliberations, bringing them into line with philosophical theories of scientific method and argument. I call this *philosophical* reconstruction. It presupposes – but usually fails to recognize – the other five kinds of reconstruction (see Table 1.1). These reconstructive activities are loosely characterized. I make them to situate standard philosophical views in relation to more naturalistic approaches to science, so as to re-open the issue of the *structures* that scientific processes are thought to have.

Table 1.1. Six kinds of reconstruction

	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

Many of these are logical or in principle formalizeable. If such structures are made rather than given, then they can be recovered empiricially. We do not have to invent logics *a priori* for processes of discovery. This book therefore focusses on cognitive, situational, procedural, and conceptual sources of some experimental narratives.

Reichenbach's distinction between the contexts of discovery and justification has little regard for the process of constructing arguments: it made a divorce of convenience which implicitly justifies a highly selective approach to what scientists produce. Of course the justification of a claim can often be separated without difficulty from its 'generation' or 'discovery', but that separability reflects the conclusion of at least the first three processes in the table. It also reflects the contingent fact that the knowledge-claim is not being challenged. One of the main findings of social and historical studies of controversy in science has been that criticism goes beyond logical and empirical matters: challenges implicate experimental skills as often as they implicate logical, mathematical or conceptual competence. In practice this means that scientists recover and re-evaluate skills, procedures and choices which may have been made invisible by reconstruction.

Philosophy and cognitive science can have little to say about the dynamics of experimental processes while they still lack modes of representing informal reasoning processes in real discovery situations. A more procedural turn can capture the reticularity and the practicality of thought and action. Here a practical problem faces philosophies of experiment. Much of what is done – especially at the frontiers of observation – involves non-verbal agency. This lies outside the traditional realm of philosophical discourse. Worse still, some of this is un-premeditated as well as pre-verbal. In the next section I venture beyond the pale, to consider how literary activities like philosophy and history of science might recognize agency in observation and experiment. I address this problem below, and in chapter 6.

1.3 The procedural turn

In this book I am concerned with embodiment which, as Dr. Johnson saw, enables us to do more than see things and utter sentences about them. Embodiment also allows the world the opportunity to correct our perceptions. As Johnson also saw, embodiment enables us to interact with an environment that has material as well as imaginary and symbolic dimensions. The manipulation of conceptual objects is often inseparable from the manipulation of material ones (and vice versa). I therefore approach observation and experiment through the more general concept of agency rather than through material embodiment.

Agents act with and upon things. The term 'action' tends be construed as intentional and deliberative: behaviour, by contrast, might be conditioned or un-premeditated. The term 'operation' carries other unwanted connotations about the attempt to ground meanings in operations that tie observation terms to experiences or quantities. I want to avoid prejudging the intentional status of certain experimental 'acts': as my discussion of reconstruction suggests, rationales for actions often emerge as the account unfolds. Particular acts are usually part of activity having a larger goal, however, even if they are verbalized, particular bits of agency need not be rationalized explicitly in terms of such goals. The term 'procedures' is not without its problems, I use it in preference to the others considered here.[16] A procedure is a sequences of acts or operations whose inferential structure is undecided.

The term *procedure* connotes two complementary, interactive aspects of a single process: on the one hand there is the manipulation of objects, instruments and experience and the manipulation of concepts, models, propositions and formalisms on the other. Although in the first few chapters I shall emphasize the first aspect I should stress that procedures involve verbal and symbolic representations of objects of experience and of learned ways to manipulate them. Procedures are developed and refined over a period of time. During that time explicit rules or protocols may be established (e.g., as in calibrating a measuring instrument or cleaning a sample for carbon dating).[17] Procedures are also intellectual and many can be machine based: sorting, mathematical integration, comparing. In this book the emphasis is on how experiments begin: the invention of experimental procedures in response to novel situations. The term 'procedural' therefore connotes know-how, knowledge that may be fluent in that it is skilled, yet is non-declarative, because it is not yet represented as a set of instructions.

As for experiences elicited by such procedures, novel experience is necessarily non-declarative. I show in chapters 2 and 5 that representations of new phenomena require the construction of new procedures. We are familiar with the crucial importance of the calculus as a method of representing physical process. The invention of qualitative procedures and the skills needed to use them has been no less important to the development of natural science. So far, there is little history and even less philosophy of how maker's knowledge has enabled scientific knowledge.[18]

Theory, observation and experiment

According to the received philosophical view, natural phenomena are bounded by theory. I shall argue that natural phenomena are bounded by human activity. Reasoning and theorizing are essential to this, but they are not sufficient. Analytical philosophy views the relationship between theory and experiment as a logical relationship between propositions. So experiment must be a means of generating observation statements which bear a logical relationship to statements derived from theory. As purposive and rational activity, experimentation must be informed by an internal narrative which has the form of a logically connected string of propositions. The same rationalist assumptions inform logic programming.

This focus on explicitly represented knowledge implicitly proscribes consideration of the *other sorts of stuff* with which science is made: instruments are invisible or feature at best in a subsidiary way, as merely practical means to theoretical ends; observers' agency figures not at all. This has not mattered to philosophy because only outcomes (hypotheses, observations) have any epistemological import. Inductive and finitist approaches that model the dynamical character of observation also retain a passive view of the observer. Hesse's learning machine possesses receptors and processes that allow self-correction of inductions, yet the machine operates in predefined ways on passively received input.[19] It learns within the parameters specified by the model of its environment, but it cannot explore by acting on that environment. I argue in chapter 8, neglect of agency in the nitty gritty of empirical access makes idealist explanations of consensus about nature (nowadays defended by sociologists rather than philosophers) more plausible than they otherwise would be.[20]

If we drop the assumption that ratiocination is the only sort of agency worth recognizing then we can look at how reasoning interacts with other activities. We get a very different picture of the relationship between the world and our representations of it. That relationship is based upon our agency in the world. To draw attention to the context of action from which scientists' talk and thought about the world emerges I shall represent agency in experimentation directly. I do this with diagrams called *experimental maps*. In this section I introduce a notation for constructing experimental maps. I shall later use it to map several sorts of empirical activity, including exploratory observation and the interaction of observation and construction of instruments (chapter 5) and the experimental testing of quantitative theoretical predictions (chapter 7). The interaction of hand, eye and mind in the fine-structure of observation is represented by two examples of short and relatively simple pieces of exploratory observation in the early stages of electromagnetism. This work, by J.B. Biot (chapter 2) and Michael Faraday (chapter 5) highlights the interaction of observers' manipulations of objects, percepts and concepts. Testing is illustrated by the first few months of Giacomo Morpurgo's search for quarks, a large-scale, long-term project to test a precise theoretical prediction using a complex experimental system. This example emphasizes the relationship between experimental technology, instrumental practices and theory. It shows how the incompleteness of theory and nature's recalcitrance necessitate a process of learning how to build and run a working experimental system.

In chapters 6 and 7 I show how these examples substantiate the thesis of this chapter, that human agency is essential to both exploratory observation and experimental testing. They illustrate how effects which are local, particular and lacking explicit theoretical significance, nonetheless shape the development of experiment and so enable the production of the empirical results that can be marshalled into evidential argument. The distinction between observing and experiment is therefore called into question: this, too, may be an artefact of the reconstructed character of retrospective accounts. However, to start with I shall use the received philosophical distinction between the conceptual world of 'theory' and the material world in which 'observations' are made, and I shall begin with the received methodology of theory-testing. This is because it is easier to understand a notation introduced to represent a familiar view. Second, this gives us a representation of the mistaken view to compare to the procedurally-explicit view developed in later chapters. Third, it is instructive to see how the distinction between conceptual and material problem-spaces breaks down in practice. As we unravel reconstruction and move into the detail we soon reach a level of analysis at which the dualistic ontology underlying the distinction between 'theory/concept' and 'experiment/material world' becomes untenable. The self-evidence of the ontological status of any particular, novel objects of manipulation proves to be a retrospective matter.

The problem of mapping activity in a four-dimensional space onto the two dimensions allowed by the printed page is not unlike the representational problems that scientists have to solve. Experiments are recorded and described with a set of concepts embodying established representational conventions. When perplexingly novel phenomena are seen or novel techniques invented, it is not obvious how to translate the novel into the familiar, nor is it obvious that what is effected *is* a translation. (I argue in the following section that here too, negotiation between observers is essential). To start with, I shall depict theoretical sorts of representations one way and observational sorts of representations another: circles denote concepts (mentally represented things) and squares denote things taken to be in the material world (bits of apparatus, observable phenomena). In the analytical tradition the significance of experimental results (observations) is purely evidential (or *epistemic*), and this is a logical relationship. This is why so little needs to be said about the process of observation and experiment as distinct from the logics of inductive support and testing.[21]

Table 1.2. Basic hypothesis testing schema.

1.	Derive hypothesis H_1 from theory T_1	
2.	Derive observable O_H from hypothesis H_1	
3.	If [setup A] then O_H. (Counterfactual)	
4.	Realize setup A	
5.	Observe result O_1	
6.	Compare O_1 to O_H	
7.	If $O_1 = O_H$	then H_1; H_1 implies T_1
	If O_1 not $= O_H$	then not H_1; not H_1 implies not T_1

On a deductive model the familiar theory-observation relationship as set out in Table 1.2 would be mapped as in Figure 1.1. Here an hypothesis H_1 is derived from theory T_1, where H_1 implies observation O_H. A real-world possibility is imagined in which O_H occurs in a material situation realized by setup A. When this is realized, a result O_1 is observed. Comparison of O_1 to O_H shows whether the results supports the original theory (via the hypothesis). This simplified narrative can of course be elaborated through the sophistication of methodological rules. However, such sophistication does not rescue the received view from the objections I develop here.

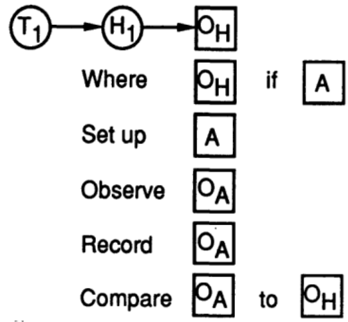

Figure 1.1. Relationship of observation to theory on the standard view.

Many statements in theories are about entities that might exist in some world – say, of mathematical or formally-defined objects, of theoretically imaginable objects or of physical objects. They are based in turn on other claims containing information about such worlds. We can combine the squares and circles to represent the ontological duality (or ambiguity) of the entities in play. For example, there are mental representations of things taken to be in the real world, but which enter discourse only as interpreted through a complex of theories. A schematic model of an electroscope or microscope represents our understanding of an actual class of instruments, so these would appear in the map as a square (material artefact) inside a circle (concept or model of artefact): see (figure 1.2a). A model of an hypothetical, possible but as yet unconstructed instrument would be shown as a square inside a circle (figure 1.2b). Uninterpreted traces on bubble-chamber photo would also appear as a square (an event in the world). However, after interpretation as the tracks of elementary particles – a learning process with a history of its own – the square representing these traces would appear inscribed within a circle, indicating its theory-ladenness.[22] Similarly, a representation or model of a putatively real entity or mechanism (such as a bacterium or an electron) would have the same composite form, until such time as it is realized (or made actual), when it could be represented simply by a square.

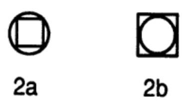

Figure 1.2. A. Object in the conceptual world.
B. Object in or taken to be in the material world.

Realizing means observing in manner that is generally accepted as 'direct'. Ryan Tweney argues that even apparently direct observations yielding representations we now accept as reasonably accurate – such as Hooke's drawing of a louse – involved many manipulations of the object under the microscope to build-up a composite model from successive, discrete "scans".

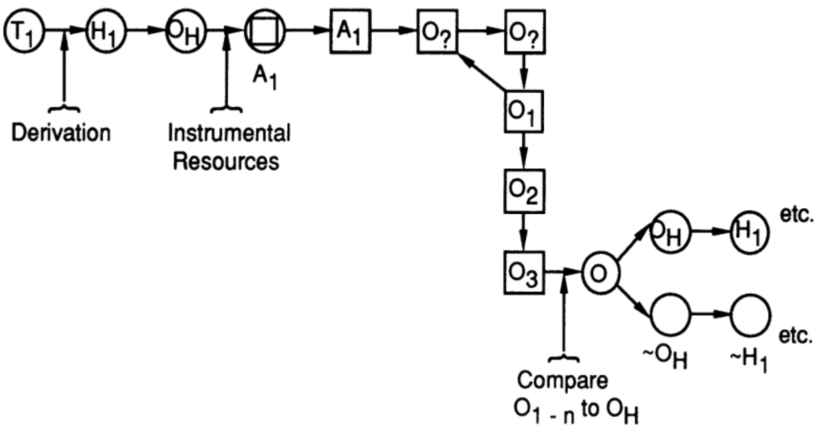

Figure 1.4. Sequence representing repeated procedures and the use of resources.

The brackets indicate when resources are drawn upon to enable operations or processes. The importance of resources drawn from beyond the immediate context is well-documented. I discuss it further in chapter 4. Such resources include mathematical or logical procedures (used in deriving H_1 from T_1); technological precedents for a proposed device or component of a system; a theory of the instrumentation or some situated design work to generate a viable piece of apparatus A_1; manipulative skills and specialized techniques; verbal and pictorial images; computational or other representational procedures which enable (say) the comparison of numerical output or observed phenomena $O_{1\text{-}n}$ to what was predicted by H_1, and so on. Comparisons involve similarity judgements involving consensus-seeking processes that I shall not attempt to map here. The decision to select a particular resource often indicates that heuristic considerations are in play. The development of this point is also beyond the scope of this chapter.

The experimenter's space

The maps show experimentation as a play of actions and operations in a field of activity, which I call the *experimenter's space*. Experimental maps represent the structure of discovery and construction in this space. As we shall see, this space should not be identified with any particular *place*. Steve Shapin and Simon Schaffer have shown that the place of experiment – meaning the social identity conferred by its physical setting – was of crucial importance to the acceptance of its demonstrative status as part of the mechanical philosophy.[26] Since the scientific revolution, however (and especially in this century) the power of experiment has been identified more with laboratories as repositories of scientific method and technique and less with particular individuals and settings. Latour's study of how Pasteur established the potency of an anthrax vaccine by moving between the laboratory and the field shows how experimenters may expanded their space to include the world outside the laboratory and, eventually, the whole of society.[27] Only in textbooks can an experiment be identified with the immediate, physical setting of its apparatus and the intellectual space in which it is represented and reported.

The maps printed here appear in a two-dimensional field. This field ranges over several inter-connected spaces: the space of concrete manipulations, mental spaces in which exploratory imaging and modelling take place; computational spaces in which analytical procedures are carried out, the social space in which observers negotiate interpretations of each others' actions, the physical (laboratory) space in which observations are fashioned, and the rhetorical and literary space in which they are reported and put to work in arguments. Broadly speaking the lines in figures 1.3 and 1.4 denote 'actions' while the circles and squares denote things that can be communicated or manipulated (concepts, images, artefacts, etc). The notation therefore represents the interplay of theory, instrumentation and observation (as in my first example) and, at a finer level of detail, the interaction of head- and hand (as we'll see below). As I remarked earlier, the term 'action' connotes a rationale (usually including a verbally-articulated goal). To avoid prejudging the inferential structure I shall treat the lines as representing 'procedures'. The maps should therefore be read as complexes of procedures.

So far we have a notation that represents agency and its objects. What structure might such processes have? If structure is less logically-ordered than we thought, how does it emerge? The answer to this question requires a final piece of preparatory work: to represent choices or decisions that determine the direction taken at any particular point.

Choice, pathways and structures

From the discussion of reconstruction it is obvious that the notion of 'the actual' pathway of an experiment is a chimera which language can express but which the reticularities of thought prevent us from accessing. The reflexive, reticular character of actors' thought reconstructs the real-time proceses of discovery and invention. I want to emphasize that the maps represent possible pathways. The plausibility of a given map depends on how well it interprets the information available to historians – including the repetition of experiments, surviving instrumentation, contemporary notes, knowledge of contemporary practices, retrospective accounts, and so on. As we shall see, maps show the existence of alternative routes that were not perceived, perceived but not pursued, or pursued to a dead end and subsequently forgotten.

When comparing a (relatively) unreconstructed laboratory record with a published narrative describing the same experiment we would expect to find not only that the actual sequence of procedures has been reordered but also that certain (contingent) choices have been promoted to decisions. I distinguish choices which are made on the basis of incomplete information or a partial understanding of a situation from decisions which are made on a rationale that is thought to be reasonably complete at the time. Let choices be represented by white triangles; decisions by black ones (see figure 1.5). Because experiments don't usually work and getting them to work involves exploring dead ends, we need to show the recalcitrance of instrumentation as well as of nature. One of my main concerns is to identify the role of perplexing results. These are important to explaining how experiment contributes empirically relevant results. We need to see how a sequence fails at any point. The failure can be conceptual (say, a derivation by argument of a model or hypothesis from theory) or practical (a setup proves impossible to construct or to operate, fails to behave as expected, or produces unexpected output, or an observer lacks the dexterity needed to carry out procedures). I indicate un-anticipated outcomes by terminating the action-line with a T or a reversed arrow-head.

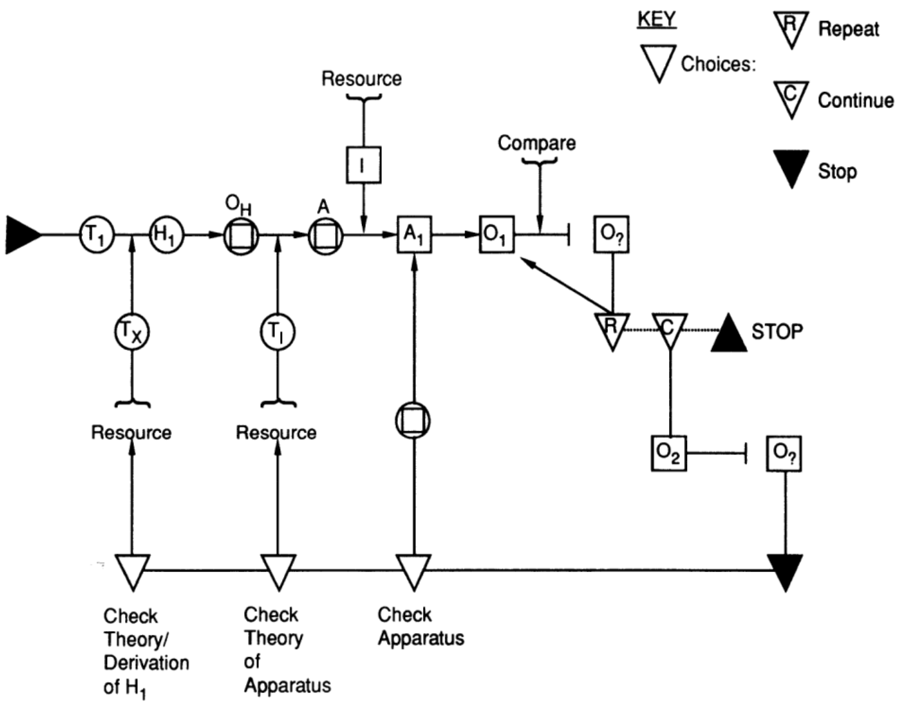

Figure 1.5. Representation of a distinction between choices and decisions.

This situates decisions, such as a decision to abandon some line of reasoning or experimental stratagem, as responses to certain kinds of outcome. There are many more levels of embeddedness of choices and decisions than the two represented here. Demonstrative and philosophical reconstructions make experimental pathways look like decision trees. These illustrate the existence of another aspect of observation and experiment that disappears from observation reports and which few philosophers have taken seriously: the social dynamics of observation.[33]

An adequate representation of observation and experiment should display the social dynamics of observation as well as the cognitive processes highlighted here. This would introduce a degree of complexity that exceeds the capabilities of experimental maps as static, two-dimensional images. I return to this problem in chapters 6 and 7 where I map specific experiments in detail.

1.5 Epistemological individualism

Controversy is essential to the emergence of new science. Historians such as Rudwick have made detailed studies showing how, for example, new geological knowledge was produced by intense and highly rhetorical debate between active researchers. His diagrammes of the Devonian controversy map out a social topology of argument.[34] Porter has shown how argument about the nature of geological science defined the emergence of geology as a new scientific field in the early decades of the last century.[35] The importance the social dimension to observation is brought out most clearly in sociological studies of controversy and by anthropological studies of laboratory life.[36] Yet philosophers still reject what goes on between observers – whether in the laboratory or the field – as irrelevant when compared to the arguments that take place between proponents of rival theories or research traditions.[37]

Philosophical objections to the sociological turn are usually couched in terms of realist and idealist theories of science. What is really at stake is the individualism implicit in traditional epistemology. The autonomy of one's own perception based on the privileged status of private experience is a hallowed assumption of traditional epistemology. The privileged status of the contents of one's own consciousness follows from Descartes' anti-skeptical argument that nothing is as indubitable as what I can introspect. What I perceive may of course be wrong but what is not in doubt is my ability to access it without distortion or error. Freud (and many others) notwithstanding, the Cartesian ideal lives on. Perception involves direct interaction with the world, but experience of the world is something individuals have independently of the experience of other individuals.

From camera obscura to shadow box

This model of perception is irrelevant not only to science but to perception in general. My argument is based on two examples: in chapter 3 I discuss an objection Wittgenstein raised against the possibility of a private language, which I call the diarist's dilemma. Faraday successfully used laboratory notes

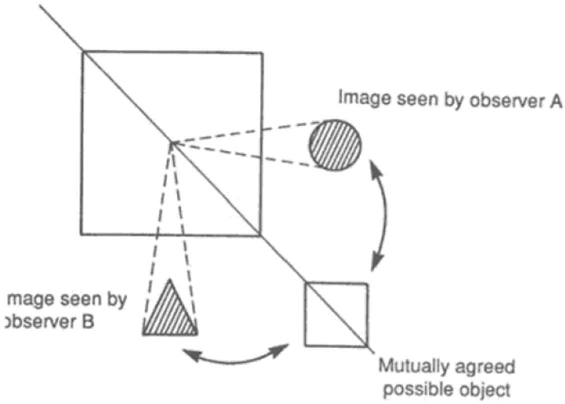

Figure 1.7. Schematic view of the shadow-box setup. Observers exchange construals of their experience and construct a possible object compatible with their different perceptions. Each observer's interpretation of his own experience is mediated by his knowledge of the other's.

and social model of observation of scientific practice, on the other. It shows how solving the problem of constructing a possible shadow-source ultimately depends on allowing another's experience equal play with one's own.

The agreement any two observers reached is far from arbitrary. It incorporates aspects of the experience of both (and in principle many more) observers and is not determined by one person's experience. (In other circumstances the negotiating skill or dominance of one observer could, of course, enable his report to determine the collective interpretation. This frequently happens in science). Nevertheless, the conclusion reached is one to which both observers agree or accede. What they agree about is a collective construct that achieves compatibility between several elements of the situation: their different experiences (and differences *between* these experiences), observers' skills (including the ability to communicate and persuade, the knowledge each brings to bear on interpreting experience (e.g., how objects project shadows). What they agree about is a construct: a set of possible objects in the box. However, such constructs are not pure artefacts: they incorporate perceptual information emanating from the shadow box. This information is useless unless it can be communicated.

Construals

I shall show in chapter 2 that the same is true of scientists. For example, similar points can be made about how scientists responding to Oersted's discovery of electromagnetism reached agreement about what they were seeing, what it might mean and, eventually, what it could or could not mean. There, and in chapters 3 and 5 I discuss the problem of making sense of aspects of a phenomenon that everyone found anomalous. Observers with quite different transformative power of novelty can be developed. It is based on a close analysis of observation and experimentation in the construction of Faraday's electromagnetic theory, from Oersted's discovery of 1820 to his qualitative statements of the relationships between electric and magnetic quantities during the 1850s. Electromagnetism is a particularly good example because it affected theory in two ways. It was readily assimilated into an established, mathematical approach to the explanation of new empirical regularities. It also precipitated a fundamentally different approach to theorizing the forces of nature, field theory. This case also shows that a wide range of observational, experimental, analytical and theoretical strategies were used during the development of classical electromagnetism. I focus on the making of images and words that communicate concepts. This is a creative and inherently social activity to which observers' agency is central: observation is a reference generating activity in which observers construe phenomena in a variety of ways. Chapter 2 argues for the idea introduced earlier in this chapter, that observers *construe* experience. Observational meaning is made in empirical situations and the supposedly mysterious correspondence of representations to entities and properties is a made relationship. The made character of this relationship supports a realist construal of the meaning of terms introduced in such situations. This provides an explanation of the historical fact that new empirical phenomena and new observational practices change the course of the history of theories.

Meaning, reference and correspondence

As a means of enabling shared experience and interpretation, construals are situational or context-dependent: they are only potentially meaningful in the way that established descriptive terms of science are. Some construals survive and become interpretations whose reference is gradually stabilized in terms of established observational practices. As interpretations they engage theoretical assumptions and problems. At this stage an interpretation may be shelved, theorized, or rejected. Either way, the agency that produced it disappears. The invisibility of human action that makes a relationship between a word or image and a bit of the world in turn makes these two sorts of thing independent. They become distinct things between which a 'correspondence' has been discovered.

The language used to describe and explain electromagnetic phenomena became conceptually explicit and more coherent. Meanings emerged in an historical process in which an operational, descriptive vocabulary was integrated into larger networks of established practices, empirical regularities and theoretical concerns. The process was not a linear one: there were frequent shifts between concrete, inventive activity and demonstration in public contexts. There were procedural descents as well as semantic ascents. However, the strength of connections between a term and its empirically specified referent gradually weakened as it was redefined, acquiring new layers of meaning. These

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