# *INTRODUCTION: SOME USES OF EXPERIMENT*

## PART I: INSTRUMENTS IN EXPERIMENT

The development of experimental science has been accompanied by a spectacular growth in the range of instrumentation which experimenters use. It has become a commonplace to refer to the significance of this growth in the interpretation of experiment. This is where our examination of the uses of experiment begins, with an examination of the ways in which instruments function in the experimental workplace. The riches of scholarship in the history of instruments provides an indispensable resource for the analyst of experiment. Many contributors to this book pursue this approach: Jim Secord and David Gooding both describe electrical devices of British experimenters in the 1820s and 1830s; Peter Galison and Alexi Assmus document the development of C.T.R. Wilson's cloud chamber; John Krige examines the British decision to help fund the particle accelerators at CERN; Allan Franklin reports on a wide range of devices employed in contemporary physics. It is clear from these cases that the uses of experimental instruments are many and various.

Yet it is hard to construct a taxonomy which adequately charts this variety and displays the forms in which instrumentation appears in experiment. This is partly because the relative priority of instruments in the experimental enterprise seems problematic. Some cases suggest that the experimenter's tactics are dictated by instrumental capacity, and reliance on instrumental testimony seems a precondition of experimental endeavour. In this sense, the experimenter is constrained by the instruments used. Other cases, however, indicate the constructive role which the experimenter plays in making any device count as an instrument precisely by using it in a trial and working to establish its reliability. Furthermore, several chapters in this volume attend to the process through which phenomena are realised in and by instrumental manipulation. In Gooding's account of the electromagnetic explorations by Michael Faraday and Peter Barlow, or the examination of C.T.R. Wilson's condensation physics by Galison and Assmus, the emphasis is placed upon the activity of the experimenter in establishing the character of an instrument and of the effects produced with it. Barlow's magnetic compass or Wilson's cloud chamber had no self-evident character: their users had to work hard to fix what that meaning might be, and this fixity was revised by others. This activity is prior to, and enables, experimental efforts to model, imitate and measure phenomena.

*Marketing and the experimental network*

The three chapters in Part I examine the active role of the experimenter in interaction with instrumentation and display the range of such interactions available in the history of experimentation. Willem Hackmann classifies the instruments of the early modern period with respect to this kind of activity. He discriminates between 'passive' devices used for measurement and observation and 'active' instruments such as the 'philosophical' air pump and electrical machine, whose purpose was to produce novel phenomena for the experimenter. This classification suggests further considerations of importance for the relation between instrument and experimenter. More than one community was involved in the production and use of instruments: communities of makers and salesmen interacted with experimenters in complex ways, and this market-place was a site of key importance for the establishment of the authority and repute of the various devices of experimental science. In his commentary on Hackmann, for example, Jim Bennett argues for the categories of the early modern instrument market, where divisions between 'active' and 'passive' devices are less visible, and where contemporaries constructed their own taxonomies of the instruments on sale. As both Hackmann and Bennett demonstrate, the dissemination of devices such as the electrical machine, the barometer or the telescope to a wide audience of customers and clients involved the establishment of reliable means of assessing instrumental performance and definition. The public standards to which these instruments were subjected had important effects upon the ways experimenters behaved and, in particular, upon the demands they could expect to make of their tools. Furthermore, the creation of markets for instruments was accompanied by standardisation of techniques and artifacts, a development of great significance for the survival and growth of experimental communities capable of communicating among themselves.

### *Standards and models*

A second feature of experimental instrumentation discussed here is also related to these issues of reliability and standardisation. In his examination of Newton's experiments with glass prisms, Simon Schaffer employs the term 'transparency' to describe the attribute an instrument possesses when it is treated as a reliable transmitter of nature's messages. Schaffer shows that devices do not appear in the laboratory as ready-made 'instruments'. Considerable work is necessary in order to establish the character of an object as an instrument, and this process is accompanied by efforts to win the assent of a community to the object's reliability and transparency. The example Schaffer chooses is that of the common glass prism, a mundane device whose use became emblematic of Newtonian optics. He argues that the Newtonian prism became 'transparent' only when agreement had been reached about the tenets of Newtonian optics. Closure in theoretical dispute was accompanied by the appearance of a new commodity: the marketable English clear glass prism. Bennett develops this thought by pointing out the similar processes which accompanied the establishment of the 'transparency' of the giant telescopes of William Herschel and the Earl of Rosse. By documenting the troubles of identity and replicability which bedevilled the dissemination and status of the prism and the telescope, these authors argue for an account of the use of instruments which recognises the role of the credibility and reputation of the makers of instruments and their users.

Replication and credibility are both key terms in the history of instrumentation and in the analysis of experiment. Many of the contributors to this volume examine the problems of making experiments reliable, and of establishing claims to reliability with fellow experimenters. Alongside the standardisation produced by instrument markets and the local achievement of reliability, an important aspect of this process is the production of phenomena before appropriate audiences. Hackmann charts the development of themes of analogy and modelling in the demonstrations of experimental natural philosophy, indicating the manifold ways in which experimenters devised instruments which purported to model nature. He describes models of electric fish and storm clouds, plant physiology and falling bodies. An enormous theoretical investment is involved in experimenters' claims that such devices are adequate simulacra of the real world. This seems to be a quite general feature of confidence in instrumental tactics. Measurement devices, optical instruments and philosophical machines all involve the more or less tacit claim that they successfully model and represent nature. Galison and Assmus demonstrate that in Wilson's late nineteenth-century programme, the cloud chamber was designed to make models of meteorological events in the laboratory. They describe this as the 'mimetic' approach in nineteenth-century science. Hackmann shows that mimesis was a common aim in eighteenth-century natural philosopy, and Gooding indicates that this imitative enterprise was also a striking feature of the research of the London electromagnetic network, especially when it sought to map geomagnetic phenomena in the 1820s. An implication of this strategy for ways of rendering phenomena and making them meaningful is explored later in this introduction.

*The invisibility of instruments*

The arguments developed by Gaston Bachelard and his colleagues in philosopy of science hint at ways in which such modelling might be analysed. Bachelard used the term 'phenomeno-technics' to pinpoint this aspect of instrumentation: effects are realised through active instrumental work, rather than recovered immediately by a passive observer from an all-powerful nature. In just this sense, instruments come to embody the theories they are used to support.[1] More recent sociology of experiment, such as that of H.M. Collins, Trevor Pinch and others, is equally forceful in its emphasis on the means experimenters use in order to save the standing of their instruments when in dispute. Devices can be calibrated, but only if the disputing parties accept that the well-known phenomenon used to standardise an instrument is an adequate surrogate for the unknown phenomenon whose character is questioned. Experimenters gain authority if their trials use instruments whose status is hard to challenge-and whose reliability is accepted. Instruments which are 'black-boxed' in this way are indispensable resources in controversy.[2]

The significance of this process is examined later in this volume by Tom Nickles and David Gooding. Here it is sufficient to note an impor-

1. Bachelard (1938), chapter 1.
2. Collins (1985), pp. 100–3; Pinch (1986), pp.212–14.

tant implication this has for our analysis of experimental instruments. An instrument's size and drama often aid the status which the device acquires: consider Van Marum's electrical jars, Rosse's telescopes or CERN's accelerators. But successful instrumentation is rendered invisible, through transparency and black-boxing. The invisibility of instruments is thus an important if paradoxical consequence of experimental achievement. Recovering the role of instruments in experiment represents an important advance in the understanding of how scientists achieve certainty.

## PART II: EXPERIMENT AND ARGUMENT

Experiments are powerful resources for persuasion and conviction. Since at least the seventeenth century, arguments that appeal to experiment have often seemed more persuasive than those that do not. In order to fulfil this role, the work of the experimenter must be transcribed and disseminated. Transcriptions of experimental activity are arguments. They are efforts to establish a particular reading of nature and its behaviour. Experimental work leaves many written traces: graphical displays, laboratory notebooks, tables of data, brief reports, lengthier and more public articles and books. Traditionally, analysts of experiment have used only the most public experimental texts, in which the work of the experimenter is presented in forms fit for consumption by a readership of colleagues. The textual deposits left by experiment are such an obvious feature of experimental life that until recently very little attention seems to have been paid to the processes by which such traces are made. An approach due to Bruno Latour and Steve Woolgar concentrates on what they term the 'inscription devices' which are used to make written material in the laboratory.[3] Inscriptions are worked up by laboratory techniques into forms which circulate inside and beyond the original experimental setting. In their initial versions, inscriptions are harder to read and more open to attack by critics. The aim of the experimenter is to transform such inscriptions to a stage where they seem capable of but one reading and become powerful weapons in argument. On this showing, public texts which purport to describe or depend upon experimental work are unlikely to involve simple representations of some original trial. On the contrary, published transcripts will need very careful interpretation if they are to be used as means of access to the experimental setting.

3. Latour & Woolgar (1979), chapter 2.

This attitude to public stories about experiment implies that they are untrustworthy guides. If publication is designed to serve a polemical or persuasive purpose, then only after agreement is accomplished will such texts receive a consensual reading, and this reading will obscure the process by which the trials in question came to acquire a settled sense. However, this salutary suspicion must not license the conclusion that celebrated public statements by experimenters are epiphenomenal to the experimental life. In fact, such texts are constitutive of the course of experimental argument and its closure. They cannot be dismissed as exercises of no import just because they are rhetorical. In his contribution, Geoffrey Cantor draws attention to the considerable efforts of historians and sociologists of science to the recovery of the experimental setting from laboratory notebooks and transcripts. What are the implications of this enterprise? One is that analysis of the process of interpretation and argument is an indispensable technique for the analyst of experiment. The chapters presented in this Part show that experimenters' writing is rhetorical and interpretative in a large number of ways.

### *Reading the 'Book of Nature'*

The ancient image of the experimenter as reader of the 'Book of Nature' already sustained the conviction that successful experiment gained authority from identification with the 'Book's' divine author. In this sense, experimental work looked like scriptural interpretation, bolstered by appropriate techniques of reading. A famous formulation of the image was that of Galileo, for whom Nature was a divinely authored book written in mathematical language. This Galilean theme was but one of many versions of the right way to interpret Nature. The chapters in this Part examine two key moments in the history of experimental writing and argument. Ron Naylor analyses the forms through which Galileo wrote about experiment, notably in his dialogues of the 1630s on the new astronomy and the new science of motion, while John Worrall examines the workings of the early nineteenth-century Laplacian régime in French physics, when experiments were described in the proceedings of committees of the Academy of Sciences appointed to award prizes to competing essays in experimental physics. The written dialogue and the transcript of the proceedings of a committee are accompanied by different rhetorics and different ways of treating experimental writing. Both depict the resolution of an experimental argument in deliberately dramatised form.

Naylor and Worrall reflect on these histrionics and their historiography. In the past, the Galilean dialogues have been scanned for evidence of Galileo's actual experimental activity. It has even been alleged that since the characters of his drama report trials in idealised or exaggeratedly accurate versions, no such experiments could have been performed. Naylor rejects such readings, and draws attention instead to Galileo's move from what he calls a 'pre-empirical episteme', towards one in which experiments could be offered as decisive confirmation of truth. The implication, discussed here both by Naylor and by Cantor, is that readers should attend to the different functions which invocation of an experiment might fulfil in different rhetorical settings. The dialogue is a particularly suggestive means of presenting trials in argument. Cantor argues that in some of the Galilean dialogues, Nature appears as an interlocutor. The process of conversion can be dramatised, the authority of Nature invoked. This is by no means always the pattern of Galileo's drama, and Naylor documents changes in the 1630s between the didactics of the dialogues on the system of the world and the demonstrations of those on the science of motion. Further, he points out the range of experimental determination which Galileo allows his actors to employ; some trials work as refutation of rival positions, others are written as the qualitative establishment of a matter of fact, and yet others seem to imply specific quantitative estimates of the behaviour of moving bodies. That there are exaggerations in Galileo's accounts of experiments is not denied: but, as Naylor shows, these exaggerations can only be understood in terms of the aims of the dialogues' author.

### *The experimental myth*

Two sets of trials described in Galileo's books soon acquired exemplary status: the isochronicity of pendulums and the free fall of bodies from a great height. In both cases, Naylor shows, Galileo confronts the interlocutors and readers of *Two New Sciences* (1638) with reports of trials of extraordinary accuracy and scale. The size of the falling weights is increased from 10 to 200 pounds, the height enlarged to 360 feet. In the pendulum trials, the different sizes of arcs described by different bobs, and their different rates of oscillation, are ignored. The effect of these moves in the Galilean text was considerable: both experiments entered the mythology of experimental philosopy and its triumph. A very similar process of transformation of written transcript into mythic drama was accomplished in the case of the history of optics analysed by Worrall. The episode in question involves the alleged response of a Laplacian committee to Fresnel's prize essay on the wave theory of optical diffraction. In the received story, a critic of Fresnel pointed out a paradoxical consequence of the wave theory, but it was then revealed that this paradoxical consequence could indeed be produced in experiment. It is claimed that the effect of this surprise was devastating: the 'white spot' observed at the centre of the shadow of a circular disc was a self-evident mark of the undulationists' victory. Worrall shows that the proceedings of the prize committee did not follow this course, and that the story of the 'white spot' has the character of a salutary fable. His concern is to establish that in this case, actors in the experimental drama were not influenced by the novelty of an unexpected phenomenon in their reaction to a successful theory.

Worrall shares an important concern with other contributors to this volume, such as Pickering, Franklin and Nickles. What is the relationship between the stories which historians can tell about the course of experimental science and the more normative lessons which philosophers teach? Worrall notes that any account of what it is to be virtuous as a scientist must respect the behaviour of exemplary scientists. However suspicious of so-called 'rational reconstructions' of the course of science, all those who study science must be aware that transcriptions of experimental practice always carry normative content. The implication is that stories about experiment and the resolution of argument are often told to illustrate rival accounts of proper conduct in the sciences.

This point is borne out by Cantor's discovery that the 'white spot' story is a myth of recent origin. Both the Galilean and Fresnel episodes provide rich material for the examination of the writing of experiment. As Cantor argues, the texts in which experimental work is invoked play crucial roles in dramatising and reproducing images of the experimental life. Key problems of the history, philosophy and sociology of experiment can be investigated through these dialogues and reports. In Galilean dialogues, an answer is offered to the question of whether shared experience generates a shared belief about the world. In the romanticised *dénouement* of Fresnel's competition, an answer is offered to the question of whether novel predictions should give preferential support to theory. No doubt these answers rely on highly idealised, if not downright mendacious, accounts of experiment. This does not deprive them of power and significance.

#### PART III: REPRESENTING AND REALISING

In chapters 7–10 the significance of experiment is shown to lie in its use in construction and articulation of those aspects of the world that scientists take to be real. Gooding shows how electromagnetic phenomena, produced on a small scale in workshops, lecture theatres and laboratories, were made visible through imagery adapted from the mapping of global, geomagnetic and meteorological phenomena. He draws attention to a network of practices and images out of which rudimentary concepts emerged, some of which became important to electromagnetic field theory. He argues that new ways of doing things led to new meanings. These meanings were at first context-dependent. They described phenomena experienced and understood by a very small number of practitioners familiar with the immediate experimental situations. They reached public awareness through public demonstrations and displays and finally gained acceptance, independent of particular practices and places, through Faraday and Thomson's theory of lines of force. Here theory was used to give wider significance to experimental phenomena. Following Fleck, Gooding argues that the acceptance of the imagery of magnetic lines and the physical concepts it implied was due in part to the fact that it articulated an explanation of practices and phenomena widely disseminated by the mid-nineteenth century.[4]

Articulation and demonstration were central to C.T.R. Wilson's work, the subject of Peter Galison and Alexi Assmus's chapter. Wilson's concern to make phenomena visible in the laboratory without transforming the appearance they have in nature affected the selection and development of his experimental methods. For Wilson, it was legitimate to use experiment to reproduce the process of cloud formation within the laboratory. However, this move from uncontrolled natural phenomena to artificially controlled conditions could be relied upon only if the experimenter's art imitated nature closely. When considering the role of instruments in experiment we have already noted that experiments which imitate nature by images or models have a long history. Galison and Assmus argue that the mimetic approach had particular importance for Wilson's selection of methods of producing, controlling and enhancing natural processes and of making them visible away from the site of experiment, for example, through photographic techniques.

Galison and Assmus's chapter also includes an illuminating account of the transmutation of the cloud chamber into the bubble chamber.

4. Fleck (1979).

Divergent interests and methodologies led to different uses of ostensibly the 'same' instrument. Here is one of the ironies of experimentation. Wilson's meteorological and holistic interests focused his attention on real clouds in the sky *and* clouds he could at first take to be equally real in the cloud chamber. J.J. Thomson and his researchers at the Cavendish Laboratory, Cambridge, were in pursuit of sub-atomic entities. This drew their attention to laboratory clouds whose artificiality was unimportant: for them, Wilson's chamber soon became a means of detecting real particles rather than a tool for producing artificial clouds. An experimental anomaly undermined Wilson's confidence in the mimetic authenticity of his clouds. This meant that he resisted the assimilation of his methods into the Cavendish tradition of analytical, quantitative experimentation. Methods he developed to preserve the real, natural status of his clouds became important for other reasons. In particular, they gave control over parameters affecting cloud formation enabling quantitative predictions to get to grips with a quantified version of the phenomena. This control licensed inferences from cloud chamber phenomena to literally invisible but theoretically desirable particles.

Whereas Gooding shows that new concepts implicit in practices led to a new theory, Galison and Assmus show how changing epistemological and theoretical rationales for the use of instruments and methods change the significance of what they produce. The 'same' device moved from one context of assumptions, skills, interests and phenomena into another. In a chapter in which mimesis features so prominently, we may also ponder the striking resemblance of their chart of the conception and disintegration of condensation physics to a Feynman diagram of the interaction of short-lived particles and fields.

## *Practices, skills and beliefs*

The first two chapters in Part III confirm that instruments and techniques remained as important in the nineteenth and early twentieth centuries as Schaffer and Hackmann showed them to be in the seventeenth and eighteenth centuries. This endorses what Hacking, Heilbron, Price and others have argued: the history (and philosophy) of theory is inseparable from the history of instrumental practices.[5] Most contributors to the present volume draw attention to the real man-made environment of skills and material artifacts with which

5. Heilbron (1979); Hacking (1983); Price (1984).