PHILOSOPHERS AND ARTISANS:
THE RELATIONSHIP BETWEEN MEN OF SCIENCE AND INSTRUMENT MAKERS IN LONDON 1820-1860

by

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A Thesis presented for the Degree
of Doctor of Philosophy

Unit for the History of Science,
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May 1991

To my Mother and Father

CONTENTS

ABSTRACT

This thesis examines the changed status of the instrument maker in the London-based scientific community of the nineteenth century, compared with the eighteenth century, and seeks to account for the difference. Chapter 1 establishes that the eighteenth-century maker could aspire to full membership of the scientific community. The following chapters show that this became impossible by the period 1820-1860.

Among reasons suggested for the change are that the instrument maker's educational context to some extent precluded him from contributing to scientific innovation, and also the changed market for his products in an industrial Britain required that he devote more time to his business, thus decreasing the time available to pursue new developments. However, the decline is attributed mainly to the tendency of the scientific community to refine its own criteria of membership, in an era in which its self-consciousness as a distinct group increased, and its members articulated claims to status in terms of their value to the State.

This ideology and its consequences are analysed in a number of studies. Chapter 2 deals with the burgeoning of collective identity in the context of the Royal Society, while the next four chapters study individual members of the scientific elite - Wheatstone, Babbage, Airy and Faraday, and their relationships with instrument makers. The studies demonstrate that the philosopher recognised the artisan's work as important, but not as vital as his own, and not classifiable as scientific work. As an institutional manifestation of the motives of the leading philosophers, the B.A.A.S. is the focus of Chapter 7. The final case study centres on the maker's tactics of self-promotion in business terms, thus linking more fully the factors at work in ensuring the rise of the philosopher and the decline in status of the artisan in the scientific community.

PREFACE

The following thesis represents the result of a period of full-time research, which I began in October 1988, into the history of nineteenth-century British science. The work was carried out in the Unit for the History, Philosophy, and Social Relations of Science at the University of Kent at Canterbury, and was aided by a studentship from the Nuffield Foundation, administered through Professor Maurice Crosland, for whose initial confidence in me and subsequent encouragement I am most grateful.

Many individuals have contributed to the work in diverse and invaluable ways, and I owe all the following a considerable debt of gratitude. In partiular, without my supervisor, Crosbie Smith, who provided a limitless source of erudition on which to draw, and my father, William Frederick Ginn, who devoted considerable effort to advice on matters of presentation, the finished product would have been much inferior. My mother, Maureen Ginn, also suggested various improvements, and Neil and Ian Higginson contributed in a variety of ways to the ease of completion of my research, not least of which was their willingness to transport me to and from the University on a daily basis.

I should like to record my thanks to the staff of all the libraries which I had occasion to visit, and who often acted well beyond the call of duty. The staff of the University of Kent Library had to endure the greatest number of obscure requests and dealt with all of them most efficiently. In particular, the Inter-Library Loans Division demonstrated great resolve in

tracking down many old books which most people would have assumed unobtainable, and the librarians of the Special Collections Section facilitated the consultation of rare volumes of material in the library here. The British Library , both its Printed Books Room and its Manuscripts Room, was visited on numerous occasions, as was the Science Museum Library, whose staff fulfilled many unreasonable requests for large numbers of nineteenth-century periodicals in all probability never before read. Manuscript collections of individual men of science and instrument makers were consulted at the discretion of the librarians of several institutions: King's College, London (Charles Wheatstone), the Royal Institution (Michael Faraday), the Institution of Electrical Engineers (William Fothergill Cooke), and the Rose Lipman Library, Hackney (Louis Paschal Casella), to all of whom, again, my thanks. The completion of this work in 1991 has been unusually topical in that it coincides with the celebrations of the bicentenary of the births of Faraday and Charles Babbage, both of whom are included as extended case studies in the following pages. The staff of the Library of the Royal Society of London provided advice on eighteenth-century manuscripts and instrument makers during my trips there, for which I am grateful.

Special thanks are due to Adam Perkins, of the Royal Greenwich Observatory, who permitted me to have access to the indexes of the George Biddell Airy archive, whilst the Observatory was in the process of its move from Herstmonceux Castle to Cambridge, and who enabled me to consult the archive itself for an extended period upon its arrival in Cambridge. The staff of Cambridge University Library Manuscripts Room also deserve my special thanks for their continued attention during this visit, in particular as I was the first person to consult the archive at its new home. In addition, I would like to record my thanks to Alison Brech, for her help and guidance on a research visit to York to consult the Troughton and Simms papers, housed there as part of the Vickers Collection, in the Borthwick Institute of Historical Research.

Several past and present members of the Unit for the History of Science have contributed to my day to day work in a number of ways, and to all of the following I am grateful: Alec Dolby, Graeme Gooday, Ana Carneiro, Ben Marsden, Yakup Bektas, Irene Crow, and William Ashworth. The production of the thesis would also have been much more difficult without the help and wisdom in word-processing matters of Arthur Page, Bernard Doolin, and Peter Gisby.

Hospitality and accommodation were provided on several visits to London by Lisa Clifton, and similar stays over extended periods were facilitated by Jivan De Silva and Chris Hawkins. Accommodation was also provided during a three-week visit to Cambridge University Library by Rob Arbuthnott, and by Jim Matthew on my trip to York. Inspiration during the writing of the thesis came from many sources: in particular I would like to thank Prefab Sprout for *Jordan: The Comeback*, The Adventures for *Trading Secrets with the Moon*, and The Brilliant Corners for *Somebody Up There Likes Me*.

Finally, I would like to express my thanks to Simon Schaffer, for his initial encouragement to embark upon the course of research leading to this volume, and to Doris and Les Hammond for their encouragement over the years.

William Thomas Ginn, May 1991.

CHAPTER ONE

INTRODUCTION

The past few years have witnessed a welcome increase in the number of historical articles dealing with scientific instruments and scientific instrument makers. In particular, a new journal, the *Bulletin of the Scientific Instrument Society,* has given a focus to work in this area, which had hitherto been somewhat neglected, or at least regarded as peripheral, by most of the other journals in the discipline of the history of science. Regrettably, the vast majority of the contributions to the new journal, and indeed in the other publications which include such work, have been made by antiquarians rather than historians. This is not to say that the studies which have been made are not interesting or valuable; rather, the point I wish to make is that they tend to be concerned with, for example, the factual details of the work of one instrument maker, or of the design of one type of instrument, and are thus somewhat internalistic in their stance.[1] Most ignore anything but the bare bones of an external context. This thesis is not motivated by an antiquarian desire to catalogue the work of a group of instrument makers. Rather, it is a contextual historical account of the place of the scientific instrument maker in the scientific community and in the wider society, and attempts to build upon a small tradition of similar

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[1] Among those dealing with the nineteenth century might be listed the following as examples of the genre: J.T.Stock, "Henry Barrow, Instrument Maker", *Bulletin of the Scientific Instrument Society,* 1986 (9), pp.11-12; J.T.Stock and P.S.Laurie, "John Dover, Instrument Maker, 1824-1881", *Technology and Culture,* 1980 (21), pp.51-5; C.Stott and D.W.Hughes, "The Amateur's Small Transit Instrument of the Nineteenth Century", *Quarterly Journal of the Royal Astronomical Society,* 1987 (28), pp.30-42.

work by historians of science.[2]

Before proceeding to a discussion of the role of the scientific instrument maker in the scientific community, it is important to define exactly what will be meant by the term *instrument maker* in the thesis. This may seem self-evident, but there are some problems in deciding whether, for example, Isaac Newton, because he could construct his own telescopes, should be called an instrument maker. I wish to have my definition mean that he should not. An *instrument maker* in this thesis is someone who derives all, or a significant part of, his living from the construction of instruments. This will generally mean that the person in question possessed trade premises of some sort as an outlet for the instruments he constructed, though this need not exclusively be the case. In any event, the definition has been framed to exclude someone such as Newton, who was sufficiently dextrous to construct instruments for his own research purposes, although he had no desire to market this skill commercially. Such an actor, a seeker of new truths of nature, for whom instruments were important because of their use as research tools, will be variously characterised as *philosopher,* or *man of science.* These terms were used interchangeably by those in the period of this thesis to describe themselves, although the former term can be seen as having a rather wider

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[2] Of particular relevance to this thesis is J.A.Bennett, "Instrument Makers and the "Decline of Science in England": The Effects of Institutional Change on the Elite Makers of the Early Nineteenth Century", in P.R.De Clercq (ed.), *Nineteenth Century Scientific Instruments and their Makers,* (Amsterdam, 1985), pp.13-27. Also W.D.Hackmann, "The Nineteenth-Century Trade in Natural Philosophy Instruments in Britain", *ibid.,* pp.53-91.

meaning, as it may include moral and political philosophers, who would not have been seen as men of science.

The distinction between men of science and instrument makers, as I have defined them, it is important to note, is not an artificial historical construct, nor indeed should it be regarded as arbitrary. The difference between someone for whom instrument making was a career, and those for whom it was merely a skill useful in their chosen life's work, is a fundamental difference. I have included the proviso that it may only be necessary for a man to derive part of his income from instrument making for him to fit my definition of *instrument maker* because historical examples exist of instrument makers with other fields of interest and sources of income. These examples generally come from the eighteenth century and at this stage need only to be indicated in passing. For instance, Benjamin Martin was a famous maker who possessed his own shop in Fleet Street from 1756, and sold a wide range of optical, mathematical, and philosophical instruments which he either made himself or bought from other makers, but Martin also made a living as an itinerant lecturer, and as an author of popular textbooks.[3] Such a character was not a feature of the nineteenth century scientific community, although there were some instrument makers in the new century such as Francis Watkins, who tried their hand at writing popular textbooks, in Watkins' case on electricity (it was electrical instrumentation for which his firm of Watkins and Hill were

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[3] J.R.Millburn, *Benjamin Martin. Author, Instrument-Maker, and "Country Showman"*, (Leyden, 1976), provides extensive details of his career.

particularly renowned).[4]

On the other side of my initial definition of instrument makers comes an actor such as Charles Wheatstone, who started life as an instrument maker (albeit a *musical* instrument maker) and who was able to make his own instruments for philosophical purposes throughout his career. However, he did not make his living from the sale of instruments, and devoted his time to scientific work, with the institutional base of a professorship at King's College, London.[5] In spite of his beginnings, he is thus characterised as a *man of science*.

1. Men of Science and Instrument Makers: "Philosophers" and "Artisans".

Having outlined the two groups with which I will be dealing, I shall give in this introduction a summary of the general historical problem to be addressed in the thesis. It concerns the apparent change in the status of the scientific instrument maker in the English scientific community between the eighteenth century and the first half of the nineteenth century. In this introduction the premiss will be established that in the eighteenth century, England, and in particular London, was the acknowledged centre of world instrument making, and its exponents were active not only in the construction of the instruments, but also in the introduction of new principles into their designs. In

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[4] F.Watkins, *A Popular Sketch of Electro-magnetism, or Electrodynamics*, (London, 1828).

[5] B.Bowers, *Sir Charles Wheatstone FRS, 1802-1875*, (London, 1975), provides details of Wheatstone's life, as does Chapter 3 of this thesis.

short, the top instrument makers were just as much researchers and original thinkers as they were artisans and retailers. The main body of the thesis will attempt to show that this was no longer the case by the nineteenth century, and that by then the instrument maker was little more than a skilled craftsman, having lost his central role in the scientific community. In addition, the thesis will attempt to provide answers as to *why* this change in status occurred.

At this stage I would like to consider three possible reasons for the change in the instrument makers' position in the scientific community. Firstly, the explanation might be given that science was becoming "more complicated". As a reason why an instrument maker could no longer contribute to scientific research this is largely devoid of any explanatory power. After all, the period between say the years 1780 and 1820 was not the only period in which science became "more complicated". Thus there is no particular reason why the instrument maker should cease to be a contributing partner in the scientific community in this period rather than for example in the period 1740-1780, when we would expect science to have made similar strides in "complexity" to those of the later period. However, although I do not wish to place much stress on this as a contributory factor in the change of status of the makers, it is useful to consider it in bringing to light the idea that the innovative element in the design of new instruments in the nineteenth century required a level of knowledge which the typical education of the instrument maker did not provide. In other words, if this first explanation is framed initially in terms of the instrument makers' educational context, rather than merely in terms of science's complexity, then it can be admitted to account in part for the decline in their status. In the eighteenth century the technical education of a maker was sufficient to keep him at the forefront of attempts to refine instruments, but in the nineteenth century new instruments were designed which required a level of theoretical, indeed mathematical, knowledge which the artisan's education did not provide. The innovative element was provided by the philosopher, the seeker of new principles, and the instrument maker was *instructed* as to how to carry out the new, sophisticated design. Thus the notion that the increasing "complexity" of science put prosecution of research out of reach of the instrument maker, and had as one of its consequences a loss in status for the maker in the scientific community, has at least a certain validity, if qualified by reference to the educational backgrounds of philosophers and artisans.

The second reason emphasised in the thesis is more historically specific than the first and concerns the position of the time period with respect to the Industrial Revolution. In the eighteenth century the instrument maker was primarily an individual *craftsman* constructing all his instruments by hand. This individual element in the work was often manifested further in a desire in the craftsman to provide improvements in the principles of his instruments and to design new ones; such improvements being pieces of scientific research in their own right. The Industrial Revolution created a new set of pressures for the maker to respond to. Not only did new labour-saving devices appear to remove some of the individual elements of craftsmanship, but makers, in an expanding market which demanded, amongst other uses, large numbers of observing instruments for Empire-building voyages, had, in order to survive, to increase the efficiency of their businesses in the face of home and foreign competition. Greater numbers of workmen became employed in instrument making. Where an eighteenth-century maker would have operated by himself, a typical nineteenth-century maker might have employed half a dozen workmen. Needless to say this had the effect of further decreasing the individual element in instrument making. Essentially, the Industrial Revolution gave rise to new economic pressures, under which the individual craftsman interested in the development of instruments was turned into a businessman and manager who only performed a proportion of his own work. These pressures were thus partly responsible for the removal of the instrument maker from centre stage in the scientific community.

The reason for the decline in the status of the instrument maker which this thesis seeks to emphasise particularly, however, is again historically specific, but this time concerns the status of the men of science themselves. I wish to argue that the early nineteenth century was a period in which the philosopher, or man of science, sought aggrandisement in British society.[6] The philosophers wished to demonstrate the importance of their

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[6]This notion of "aggrandisement" I derive from J.B.Morrell, "Professionalisation", in R.C.Olby, G.N.Cantor, J.R.R.Christie and M.J.S.Hodge (eds.), *Companion to the History of Modern Science,* (London, 1990), pp.980-9.

activity to the State and to mankind in general, and to construct a position for themselves as an elite group in society. This goal had as a corollary that other groups were excluded from the elite group. This thesis argues that instrument makers happened to be one excluded group. Although these three interrelated factors which I have postulated were all operative in determining how instrument making and the original work done by makers changed, then, the thesis will show that the main factor at work in excluding the makers from the scientific community was the way in which that community came to be defined in the nineteenth century by those controlling it.

2. Instrument Making in the Eighteenth Century.

Before proceeding to discuss the nineteenth century relationship between philosophers and artisans, however, it will be necessary to establish that there really *was* a change in the status of the maker in the scientific community. That is, it must be established that in the eighteenth century the elite maker was a full contributor to the scientific enterprise. I propose to establish this by a consideration of the top London makers in the latter part of the century, a field of study in which some good material already exists,[7] before going on to the rather more neglected area of the makers' position in the first half of the nineteenth century.[8]

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[7] Most notable in recent years has been R.Porter, S.Schaffer, J.A.Bennett, and O.Brown, *Science and Profit in 18th-Century London*, (Cambridge, 1985).

[8] The articles collected in De Clercq, *op.cit.* (note 2), represent much of the available scholarship, though many deal with the situation abroad.

A three-fold division of the instruments produced by makers had emerged by the early eighteenth century. The typical maker might describe himself as "Mathematical, Optical, and Philosophical Instrument Maker", a distinction which would persist into the nineteenth century. Many makers were characterised more simply as devoting themselves to one of these specialisms. "Mathematical" instruments were aimed at the solution of *practical* problems of astronomy, navigation, or surveying, and included quadrants, sextants, and so on. "Optical" instruments such as the telescope and microscope were also in one sense practical, but in addition aimed at a theoretical understanding of the world. "Philosophical", or "Natural Philosophical" instruments not only included those for passive observation, but also for active intervention with nature by creating new effects, as in air pumps, or electrical machines.[9]

The three-fold division in instruments is not of vital importance so far as this study of instrument makers is concerned, as most makers dealt to some extent in all three classes. It is important to realise, however, that the *markets* for the different types of instrument were not the same in the eighteenth century when the division first appeared. Instruments such as air pumps and electrical machines, i.e. philosophical instruments, tended to be attractive to private gentlemen as status symbols. So did telescopes and microscopes. A rather different market existed for the mathematical instruments, which

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[9] J.A.Bennett, "The Scientific Context", in Porter, Schaffer, Bennett, and Brown, *op.cit.* (note 7), pp.5-9.

were required by those needing to solve practical problems. In addition, large telescopes and other large instruments were constantly required by three metropolitan institutions - the Royal Observatory, the Board of Longitude, and the Royal Society.[10] A commission from one of these institutions was very important to a maker in terms of the prestige it could give him, and original work done on such instruments established makers as key members of the scientific community. The popular market, while financially lucrative, was not as significant in increasing a maker's scientific status - the elite makers were those who made the best sextants and telescopes, not those who made the best air pumps. This theme will be implicit throughout my discussion.

Precision instrument making in the eighteenth century could thus be a form of aggrandisement - it enabled its exponents to become members of the established scientific community. Many of the top makers became Fellows of the Royal Society. Of those fitting the initial definition given in this thesis of *instrument maker* can be listed James Short, John Senex, George Graham, John Dollond, Edward Nairne, John Ellicott, John Whitehurst, and Jesse Ramsden.[11] Many of the other makers in this period in London were accepted as being amongst the finest in the world, though for one reason or another they did not become Fellows of the Royal Society as a reward for their work. Among this latter class we could name John Bird, Jonathan and Jeremiah Sisson, George

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[10] *Ibid.*, p.8.
[11] See the List of Fellows in *The Record of the Royal Society of London*, (London, 1912).

Adams, Benjamin Martin, John Troughton, Peter Dollond, and John Cuthbertson.[12]

Rather more remarkable than the number of makers who became honoured as Fellows of the Royal Society (and two of those named, Ramsden and John Dollond, received its highest accolade, the Copley Medal) is the sheer wealth of papers published in the *Philosophical Transactions* by these men: such papers would have been seen as genuine pieces of scientific research, and some represented considerable expenditure of time for men who were engaged in running their own businesses and constructing their own products for sale as well. In the period from 1730 to 1800 can be found in excess of 80 papers in the *Philosophical Transactions* by Fellows who were instrument makers.[13] Neither did the lack of a Fellowship preclude publication: Peter Dollond published 3 papers though never emulated his father's achievement of a Fellowship and the Copley Medal. Most prolific of the contributors to the journal were George Graham, with 23 published papers, and James Short, with 32. By considering the careers of some of these makers it will be possible to establish a perception of the position of the

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[12] For biographies of these and other makers active in the eighteenth century, see E.G.R.Taylor, *The Mathematical Practitioners of Hanoverian England, 1714-1840,* (Cambridge, 1966); M.Daumas, *Scientific Instruments of the 17th and 18th Centuries and their Makers,* (London, 1972); H.C.King, *The History of the Telescope,* (London, 1955).

[13] This statistic is derived from a thorough study of the indexes to the papers published in the *Philosophical Transactions of the Royal Society of London.*

leading makers as a group within the scientific community in eighteenth-century London, and this is the aim of the next two sections of the chapter.

3. The Instrument Maker and the Scientific Community in the Eighteenth Century: James Short, Benjamin Martin, and George Adams.

James Short provides us with an example of a man who must be characterised as an instrument maker, as that was his means of livelihood, and yet who had much wider scientific interests which led him to a respected position in the scientific community. Born in Edinburgh in 1710, he attended university and had an excellent record there, being urged by relations to enter the church. Instead he became the protege of Colin Maclaurin and started making specula for reflecting telescopes, by the sale of which he is reported to have made a profit of £500 before leaving Edinburgh in 1736.[14] But although instrument making was Short's trade, he left initially in order to become mathematics tutor to the son of George II in London. He set up his own shop there in 1738, dealing solely in reflecting telescopes, of which he is said to have made around 1360 in his lifetime; these could be bought "off the shelf", though larger versions had to be specifically ordered.[15] It seems certain, however, that Short only made the specula for the telescopes, contracting the work on

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[14] D.J.Bryden, *James Short and his Telescopes,* (Edinburgh, 1968); G.L'E.Turner, "James Short F.R.S., and his Contribution to the Construction of Reflecting Telescopes", *Notes and Records of the Royal Society of London,* 1969 (19), pp.91-108.

[15] *Ibid.,* p.100.

the frames and stands of the instruments to other makers.[16] Even so, it is worth noting that he carried on an interest in practical astronomy in parallel with running his business. He built an observatory in the roof of his premises and contributed observations in the form of regular papers to the Royal Society. Many of his 32 published papers in the *Philosophical Transactions* concern the observations he made in this way.[17]

Short's work on telescopes earned him the title of Fellow of the Royal Society shortly after his move to London - he was elected in 1737. His certificate of election makes it apparent that it was explicitly for his instrument making skills that he was elected:

> Mr.James Short of the College of Edinburgh, who has lately distinguished himself by his Excellent Reflecting Telescopes, and other Curious Optical Instruments, is proposed as a candidate for election into this Honourable Society, and we hereby recommend him as a person well versed in several parts of Mathematical and Philosophical Knowledge, and qualified to become a useful member of the same.[18]

Certainly Short fulfilled this promise - besides the 32 papers which were published, he communicated to the Society the work of his potential rival John Dollond on correction of

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[16] *Ibid.*, p.99.

[17] For example J.Short, "The Difference of Longitude between the Royal Observatories of Greenwich and Paris, determined by the observation of the transits of Mercury over the Sun in 1723, 1736, 1743, and 1753", *Philosophical Transactions of the Royal Society of London*, 1763, pp.158-69.; J.Short, "Observations on the eclipse of the Sun, April 1 1764", *Philosophical Transactions of the Royal Society of London*, 1764, pp.107-9.

[18] Royal Society Certificate Book, 1731-50 f.123, Royal Society Archive.

chromatic aberration in the refracting telescope.[19] This work meant that the achromatic telescope became a rival to the reflecting telescope to which Short had devoted himself. Clearly, therefore, a gentlemanly ethos and a desire to advance knowledge, rather than a mere commercial motivation to protect the dominance of his own product, pervaded Short's actions. His expertise was such that he was mentioned as a possible successor to Nathaniel Bliss as Astronomer Royal in 1765,[20] a post that in the event went to Nevil Maskelyne. Short served on the Council of the Royal Society from 1760, being particularly active in work on the transit of Venus observations in 1761, for which he not only made many telescopes, but was also on the British list of official observers (along with other makers John Bird, John Dollond, and John Ellicott).[21]

The example of James Short, then, is an important one for the purpose of this thesis - a man who made his living as an instrument maker and yet was an integral part of the scientific community, producing a significant amount of research work. Although he was the only maker ever to have been considered for the Astronomer Royalship, Short's position as a researcher was by no means unique, and other makers were able to attain full scientific status by original work. However, it is worth noting that this was not the only avenue by which someone who made a living from instrument making could be a part of the scientific

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[19] J.Dollond, "An Account of some Experiments concerning the different Refrangibility of Light", *Philosophical Transactions of the Royal Society of London,* 1758, pp.733-43.

[20] Turner, *op.cit.* (note 14), p.93.

[21] *Ibid.,* p.94.

community.

Benjamin Martin made instruments initially in order to illustrate his lectures on natural philosophy, which he delivered on an itinerant basis, outside any institutional base, and by which, along with his textbooks, he achieved considerable scientific renown.[22] Martin provides us with an example of one who derived an income from instrument making, and thus fits my characterisation of *instrument maker*, but who also earned a part of his income from other sources, namely the writing of books, and the admission fees of those who attended his lectures.

Born in 1704 or 1705, Martin did not open his own shop until 1756, after many years as a lecturer and author of textbooks, although he derived an income from instrument making before that. Significantly, he also designed his own instruments and is therefore typical of the elite eighteenth century instrument makers - for example he developed a triple lens microscope.[23] Unlike Short, however, the title of Fellow of the Royal Society eluded him, and his writings tended to be of a popular nature, none being published in the *Philosophical Transactions*. This is not to say that Martin did not appreciate the honour that a Fellowship would be. In fact, he was well aware of the benefit such a title could be to him in career terms, for Millburn has uncovered a somewhat inept attempt of Martin's to secure a

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[22] Among the better known of the large number of books written by Martin are *The Philosophical Grammar*, (London, 1735), *An Essay on Electricity*, (Bath, 1746), and *Philosophia Britannica*, (Reading, 1747).

[23] Taylor, *op.cit.* (note 12), p.62.

Fellowship - in order to gain prestige for his lectures, and thus increase his income. Millburn produces correspondence to show that as early as 1738 Martin had approached Hans Sloane (President of the Royal Society), requesting that he be considered for a Fellowship, as:

> ...wherever I come, I am constantly asked, if I am a Fellow of ye R.Society? And I as constantly find it no small Disadvantage to say, No... no Man takes a greater Delight in Studies of Philosophy, nor has taken greater Pains to improve ye Science in general...[24]

Even if Martin's latter plea was accurate, his indication that Fellowship was likely to be financially advantageous to him would not have helped his quest for election to a club with a gentlemanly ethos such as the Society. In addition, his offer to make a present to the Society of his new portable air pump, if he was elected, was effectively nothing more than a bribe.[25] Whatever the opinion of his scientific talents amongst the Fellowship, errors of protocol such as these were sufficient to ensure that Martin's application never reached the stage of a formal vote, and he was forced to continue in his role as popular lecturer, textbook author, and instrument maker, without the title of F.R.S., and rather peripheral to the real elite of the scientific community, yet undoubtedly a scientific man rather than a mere artisan and instrument maker.

The instrument making shop which he established ensured for a long time a comfortable living for Martin, and he sold both his

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[24] B.Martin to Hans Sloane, 14 November 1741, MS.Sloane 4057 ff.85-86, British Library, London, cited in J.R.Millburn, "Benjamin Martin and the Royal Society", *Notes and Records of the Royal Society of London,* 1973 (28), pp.15-23, on p.17.

[25] *Ibid.*

own instruments and those of other makers. However, he took diversification in the business too far, and in 1780 he was on the verge of bankruptcy. Martin died in 1782 after a rumoured suicide attempt brought on by his desperate financial plight.[26]

The way in which Martin organised his business contrasts with his contemporary George Adams, who similarly advanced his scientific reputation and advertised his work by writing textbooks.[27] Whereas Martin tried to make his instruments as attractive as possible by making their prices as low as possible, Adams limited himself to more expensive instruments, while still retailing as vast a range as Martin, though being particularly renowned for globes and microscopes.[28] As with other top makers in the eighteenth century, Adams was noted for *improving* instruments he constructed, and for *inventing* his own new ones:

> In the construction of all the machines I have ever made, my first and greatest Care hath been to produce good Models and Drawings, several of them I have imitated from the best Authors, as well Foreigners, as those of our own Country. I have altered and improved others, and have added many new ones of my own Invention.[29]

Short, Martin, and Adams, then, were contemporaries from the mid-eighteenth century, all of whom were first and foremost

[26]Millburn, *op.cit.* (note 3).

[27]For example G.Adams, *Micrographia Illustrata,* (London, 1746); G.Adams, *A Treatise describing and explaining the Construction and Use of the New Celestial and Terrestrial Globes,* (London, 1766). His son also wrote textbooks, for example G.Adams the younger, *An Essay on Electricity,* (London, 1784); G.Adams the younger, *An Essay on Vision,* (London, 1789).

[28]It is worth noting that his treatise on the globes went through some 30 editions.

[29]Adams, *Micrographia Illustrata,* p.224.

instrument makers, yet whose trades were very different. Martin dealt in a wide range of cheap apparatus, Adams dealt in a wide range of expensive apparatus, and Short dealt in one type of instrument only. Their membership of the scientific community, however, was assured not only by their design and construction of instruments for scientific purposes, but also by their wider interests - textbook writing in the case of Martin and Adams, and research in practical astronomy in the case of Short.

4. The Instrument Maker and the Advancement of Science in the Eighteenth Century: John Dollond and Jesse Ramsden.

The above contention, that the instrument maker had a role to play in scientific endeavour, and had a comprehension of the scientific enterprise, is borne out by the situation in other countries such as the Netherlands. Hackmann has studied the role of the instrument maker there with reference to Jan van Musschenbroek and his younger brother Petrus, the latter being the maker, and points out the prevalence of questions put to Jan by Petrus on theoretical scientific matters - such as the effects of a vacuum on fire, the causes which prevent two thermometers from giving the same readings, and about problems in mechanics.[30] Hackmann also states that Petrus' scientific expertise was such that he was able to point out errors committed by Jan in his textbooks, and he extends his argument to include England, where, like Petrus van Musschenbroek in the Netherlands,

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[30] W.D.Hackmann, "The Growth of Science in the Netherlands in the Seventeenth and Early Eighteenth Centuries", in M.P.Crosland (ed.), *The Emergence of Science in Western Europe*, (London, 1975), pp. 89-110, on p.98.

instrument makers such as Short, Ramsden, and Nairne were seen as making "significant scientific contributions".[31] Unfortunately, Hackmann uses misleading vocabulary in referring to Petrus van Musschenbroek and these Englishmen as "not mere artificers but...scientists in their own right".[32] Calling an eighteenth-century instrument maker a "scientist" gives a false impression of the actors' own enterprise, though it is of some help in conveying the idea that these were not mere manual workers, but were men who had a full comprehension of the work for which scientific instruments might be required. That the English makers were appreciated as such by eminent foreign philosophers can be illustrated by the views of J.D.Cassini (great-grandson of the Italian J.D.Cassini, first director of the Paris Observatory), who, lamenting a decline in the quality of work of the French instrument makers whom he employed at Paris, wrote: "..les Ramsden, les Dollond sont geomètres et physiciens, nos meilleurs artistes ne sont qu'ouvriers", which was translated by King as "Ramsden and the Dollonds are geometers and scientists, our best artists are only workmen".[33] Although King's translation suffers from an anachronistic use of the word *scientist,* the sentiment is clear: Jesse Ramsden, and John and Peter Dollond were key members of the scientific community.

The name of John Dollond (1706-61) survives today in the firm of Dollond and Aitchison, opticians. Although he worked on

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[31] *Ibid.*
[32] *Ibid.*
[33] C.Wolf, *Histoire de l'Observatoire de Paris,* (Paris, 1902), p.292, translation in King, *op.cit.* (note 12), p.229.

optical instruments for much of his life, it was not until 1752 that he joined his son Peter in the optician's business which the latter had established in London in 1750.[34] In 1753 John devised the divided object-glass micrometer, which James Short applied to his reflecting telescopes. Dollond was most famous for his work on the correction of chromatic aberration; by a combination of lenses of different materials he created an achromatic objective which greatly improved the telescope as a research tool.[35] Although Dollond took out the patent for the achromatic lens, it was proved in an unsuccessful law suit against Peter Dollond after John's death that the discovery had first been made by Chester More Hall, of Essex.[36] Ramsden, a close friend of the Dollonds, and related to them by marriage, wrote to the Royal Society to emphasise that Dollond accepted that Hall was the inventor of the achromatic lens, around 1730, though he had come upon it independently; Ramsden felt that as Dollond had made the invention public, he deserved the patent.[37] Ramsden showed his high opinion of Dollond's skill in this same letter, by pointing out:

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[34] J.Kelly, *The Life of John Dollond,* (3rd edition, London, 1808). A recent work which deals with the history of these famous opticians is H.Barty-King, *Eyes Right: The Story of Dollond and Aitchison, Opticians, 1750-1985,* (London, 1986).

[35] His first work in this direction was J.Dollond, "On an improvement of refracting telescopes by increasing the number of eye-glasses", *Philosophical Transactions of the Royal Society of London,* 1753, pp.103-7. See also Dollond, *op.cit.* (note 19).

[36] King, *op.cit.* (note 12), pp.144-5.

[37] J.Ramsden, "Remarks on Invention of Achromatic Telescopes, 1789", Letters and Papers IX.138, Royal Society Archive.

> [John Dollond] was the only man at that time in London who either had knowledge or a wish to improve Optical and Mathematical Instruments.[38]

However, while Dollond's scientific expertise may not have been in doubt, family loyalty must have obscured Ramsden's vision, and caused him to ignore makers such as Short, Adams, and Bird, for whom the sentiments of the above tribute would have been just as appropriate.

Peter Dollond also had considerable scientific ability, as is shown by his concern during the law suit to demonstrate the novelty of John Dollond's work. He made considerable reference to Newton's *Opticks,* which his father's work seemed to contradict, and referred to correspondence his father carried on with Swiss mathematician Euler, for whom the chromatic aberration problem solved by Dollond proved elusive.[39]

From the foregoing it will be apparent that John Dollond, though he made his living by making instruments, was a fully contributing partner in scientific endeavour. He won the Copley Medal in 1758 for his work on the achromatic lens, and gained a Fellowship of the Royal Society in 1761, the year of his death.[40] What I wish to emphasise here is that an eighteenth-century maker such as John Dollond was not primarily motivated by an ambition to make his business the most efficient and lucrative possible, though this was an important aspect of his activity,

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[38] *Ibid.*

[39] P.Dollond, "Account of the late John Dollond's discovery, which led to the grand improvement of the refracting telescope", Letters and Papers IX.131, Royal Society Archive.

[40] Taylor, *op.cit.* (note 12), pp.155-6 gives a condensed biography of John Dollond.

but by a desire to do scientific work and to make real improvements in instruments, which would help to advance science.

This claim, that the makers of the period were keen to play their part in the advancement of science, more than in the advancement of their own businesses (even though motives of increasing their social profile may have been behind such activity), is made explicit in the correspondence of Jesse Ramsden. Ramsden was widely regarded as the most eminent and skilled of all the London makers of the eighteenth century, or indeed of any other era.[41] Born in 1731, he apprenticed himself to an instrument maker in London in 1755, and established his own business there in 1762.[42] While making the usual wide range of mathematical, optical, and philosophical instruments, his speciality was scale dividing, in which area he developed his celebrated dividing engine, an account of which was published in 1777.[43] It was largely due to the sophisticated nature of his work on the dividing engines that he was to achieve his world-wide reputation, and he began particularly to deal in large instruments for astronomy, and surveying expeditions. Brown points out that he also invented a new type of cistern barometer, and constructed early examples of plate electrical machines and precision balances.[44] Reward for his work came with a Fellowship of the Royal Society in 1786, and the Copley

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[41] *Ibid.*, p.32 is a secondary source endorsing this view.
[42] King, *op.cit.* (note 12), p.162.
[43] J.Ramsden, *Description of an Engine for Dividing Mathematical Instruments*, (London, 1777).

[44] O.Brown, "The Instrument Making Trade", in Porter, Schaffer, Bennett, and Brown, *op.cit.* (note 7), pp.19-37, on p.35.

Medal in 1795. These achievements followed several papers contributed to the *Philosophical Transactions.*[45]

Throughout his dominance of the instrument making scene, however, when these new developments in instrument design were being made, Ramsden was carrying on a business and employing a workforce. It has been suggested that in the Piccadilly workshop which he moved into in 1775, he employed in excess of 50 artisans.[46] Clearly this was a large workforce by any standards, but was particularly so in eighteenth-century London, where the craftsman very much retained his individual identity in the instrument making trade. Yet it is undeniable that Ramsden kept his identity, mainly due to the fact that he was not merely the manager of a business. Improvement in the designs of his instruments and the advancement of science were his primary motivations; this is borne out by his correspondence with the Royal Society.[47] Ramsden received a lot of criticism from clients for the time which he took to deliver seemingly straightforward orders, criticism to which he responded by stating that he had "been more attentive to the improvement of my Art, than to the accumulation of wealth".[48] Thus, when an order was placed with him, as in the case he was discussing here,

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[45] For example J.Ramsden, "A Description of Two New Micrometers", *Philosophical Transactions of the Royal Society of London,* 1779, pp.419-31; "A Description of a new Construction of Eye-glasses for such telescopes as may be applied to Mathematical Instruments", *Philosophical Transactions of the Royal Society of London,* 1783, pp.94-9.

[46] Brown, *op.cit.* (note 44).

[47] J.Ramsden to the Council of the Royal Society, 13 May 1790, Miscellaneous Manuscripts 3.30, Royal Society Archive.

[48] *Ibid.*

by General Roy, the surveyor, the delay was occasioned by his desire to construct the best possible instrument:

> ...the General applied to me to construct and make for him the best Instrument I could within a limited price, for measuring horizontal angles. In consequence thereof, to the neglect of a more lucrative business, and chiefly with a view to the advancement of Science and the honour of this Country, I constructed an Instrument on principles very different from what had ever been done before...[Ramsden goes on to explain how he kept contriving facets to improve accuracy - hence the delay in completion of the instrument]...surely a mechanic who employs his mind, neglects a constant business, and spends his money for the promotion of science, + after all receives abuse has...reason to be dissatisfied.[49]

This letter brings out exactly the characterisation I wish to make of the eighteenth-century instrument maker - although involved in a trade his outlook was not that of an entrepreneur. His motivations were more like those of a gentlemanly philosopher - a desire to render improvements in the principles of design of the tools used in science, improvements which would contribute to the advancement of science and the honour of the country. This is in contrast to the desire of other classes of artisans to carry out work conceived by others, in order to make their living.

Previous writers have conveyed this notion more vaguely by simply saying that in the eighteenth century the instrument makers *were* the "scientists" themselves,[50] a statement which my argument challenges, while allowing it a certain validity in expressing the motivations of the eighteenth-century artisan, which in many cases were indistinguishable from the man of science. Certainly the rewards in terms of scientific

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[49] *Ibid.*
[50] Hackmann, *op.cit.* (note 30), p.98.

recognition were the same. Some instrument makers were able to extend the expression of these motivations in practice by giving up instrument making in order to devote their time to other scientific pursuits. James Watt is the best known example of this transition - he started his working life as a precision instrument maker.[51] Other makers developed interests in burgeoning areas of scientific concern. For example Edward Nairne wrote a number of papers on research in electricity, which were published in the *Philosophical Transactions,* but remained within instrument making as a trade.[52]

5. The Instrument Maker in the Nineteenth Century: Summary of the Case Studies.

Having established the position of the eighteenth-century instrument maker in the scientific community, and having suggested his motivations as being the advancement of science and the honour of his country, rather than the accumulation of personal wealth, this thesis asks the question: "Did this position change in the new century, and if so, why?". I have already suggested that the answer to the first part is *yes,* and have postulated three possible contributory factors as answers to

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[51] Not surprisingly, many biographies of Watt exist. One of the longest established is J.P.Muirhead, *The Life of James Watt, with Selections from his Correspondence,* (London, 1858).

[52] For example E.Nairne, "Experiments on water obtained from melted ice of sea water", *Philosophical Transactions of the Royal Society of London,* 1776, pp.249-56; E.Nairne, "Experiments on electricity, being an attempt to show the advantage of elevated conductors", *Philosophical Transactions of the Royal Society of London,* 1778, pp.823-60; E.Nairne, "The effect of electricity in shortening wires", *Philosophical Transactions of the Royal Society of London,* 1780, pp.334-7.

the second part. The thesis attempts to expand on these answers by means of a number of case studies of key individuals and institutions in the scientific community for whom the instrument maker was of vital importance as the builder of the tools of their trade, and yet was excluded from the higher echelons of the elite community to which they belonged. As the centre of the instrument making trade in the nineteenth century, as in the eighteenth, was London, so the case studies have the unifying theme of all being London-based. These case studies provide a cross-section of the relationships which existed between the instrument maker and the man of science in the first half of the nineteenth century.

The first case study (Chapter 2) concentrates on the role of the maker within the longest established of all the English scientific institutions - the Royal Society of London. Whereas in the eighteenth century Fellowship was a regular award to the elite instrument maker, it became increasingly rare in the nineteenth century for men who made their living by constructing instruments to attain the title of Fellow of the Royal Society. There were only five such English makers in the nineteenth century, and all of these had some sort of family or business connection with the leading makers of the earlier period. More dramatic still was the decrease in the output of published material from the artisans - contributions to the *Philosophical Transactions* went from being a relative commonplace to being comparative rarities.

By considering the role this small number of Fellows played in the Society, and the nature of the contributions that they made to the *Philosophical Transactions,* the chapter attempts to show the way in which the Fellowship could be a career advantage to the maker. It also attempts to show that the content of the papers contributed had changed from those of the eighteenth-century maker, which were more pieces of pure research than notifications of the latest small development in instrument design. Overall, the chapter is concerned to show that the nineteenth-century maker, whether a Fellow or not, was not a full member of the scientific community represented by the Royal Society of London.

Chapter 3 discusses the relationship between the instrument maker and a man of science with interests in a wide range of subject areas, Charles Wheatstone.[53] The choice of Wheatstone is particularly significant because he came from a family of instrument makers, albeit *musical* instrument makers, and thus had a keen appreciation of the practical difficulties attending the work of the precision instrument maker. In addition, from a period of *practical* experience as a musical instrument maker, he had acquired the necessary skill to construct those scientific instruments which he required for his own research and for which he did not wish to employ an instrument maker. Thus, whilst not being unique amongst men of science in the period in being able to construct his own instruments, Wheatstone was rather unique in coming from a background which enabled him to understand the motivations of the instrument maker.

Wheatstone's own motivations, however, were to pursue the

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[53] Bowers, *op.cit.* (note 5) is the most complete survey of his interests.

scientific research that had interested him since a child, and with the financial security of the family business to fall back on, he was able to do this, quickly achieving a considerable reputation in the elite London scientific community, particularly in the area of acoustical research. He was to establish himself firmly as a member of this community by securing an institutional base in the form of a professorship at King's College, London.

The chapter examines Wheatstone's employment of instrument makers and his attitudes towards them, both in the context of his professorial position and his position as an individual researcher, pursuing work in a wide variety of areas, particularly electrical telegraphy; research for which he employed a sizeable number of instrument makers. I argue that from his position as a member of the elite scientific community he regarded their work as important, having a keen appreciation of the skill required, and yet subordinate to his own work. The instrument maker was not a member of the same community as Wheatstone.

The individual discussed in Chapter 4 is Charles Babbage. Like Wheatstone, Babbage required instruments for his own research in a number of areas, and thus had constant occasion to employ instrument makers. The chapter deals particularly with the relationship between Babbage and the artisan whom he employed to work on his famous difference engine, Joseph Clement.[54] Babbage was of course notable also for his criticism of the existing

[54] The source material here is particularly the collection of Babbage correspondence in the British Library, London.

scientific establishment, and the Royal Society most of all, in his 1830 work *Reflections on the Decline of Science in England.*[55] The chapter shows that although he advocated a distillation of the expertise of artisans as a desideratum in order to ensure scientific and technological progress, he regarded the work of the artisan as very much subordinate to that of the elite philosopher such as himself in ensuring this progress. That he saw himself as an individual in whom power should be vested in the scientific community, as a leading member of an elite group in society, is borne out by the analysis of *Reflections on the Decline of Science in England,* his other published work, and his unpublished correspondence, which is provided in this chapter. Babbage's attitude was unlikely to grant a key place in the scientific community, the community which he saw as the guarantor of national prosperity, to the constructor of scientific instruments.

This idea of the philosopher as a member of an elite group in society from which the instrument maker was excluded, no matter what his scientific status in previous times, is further established in Chapter 5. The subject of this chapter is the Astronomer Royal, George Airy, in the context of his position in charge of the Royal Observatory, Greenwich. In this capacity, Airy dealt more with instrument makers on a day to day basis than did any other man of science in the period, and the basic source material for this chapter has been the extensive correspondence of Airy with the instrument makers who were employed to construct

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[55] C.Babbage, *Reflections on the Decline of Science in England, and on Some of its Causes,* (London, 1830).

instruments for the Observatory, from large telescopes to magnetic instruments to thermometers.[56]

The key themes in this chapter concern the uses made *of* the makers by Airy and the Observatory, and the uses made *by* the makers of Airy and the Observatory. The makers could use commissions from the Observatory in order to demonstrate their technical skill and ability, gaining advantage in direct financial terms and in terms of reputation. Hence the chapter argues that work for the Royal Observatory tended to advance a maker's business rather than enhance his role in the scientific community, a tendency which would not have been the primary motivation for the eighteenth-century maker. Airy used the makers, however, first and foremost as artisans *expected* to carry out his instructions to the letter, and only to deviate from his designs on small matters of engineering practice.

The argument which the chapter emphasises is that Airy's perceived position as a member of an elite group in society, with a vital role to play in the running of the nation's chief observatory, and hence in the progress of science and of the nation as a whole, meant that a group such as the instrument makers were rendered subservient to his goals and the goals mapped out by this new elite group, and were expected to deliver orders on time and according to the philosophers' instructions. Any delay caused by a maker like Ramsden, pursuing improvements in the *principles* of design of the instruments he was

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[56] The Airy collection, numbering over 800 volumes, is now housed in Cambridge University Library, having moved there from the Royal Greenwich Observatory in 1988/89.

commissioned to build, would not have been tolerated, as the only group with the necessary expertise to effect such improvements was seen as being the group of philosophers of which Airy was a part, and which saw itself as the proper site for scientific power.

Chapter 6 is concerned with another member of this elite group in his institutional context - Michael Faraday. Faraday was one of the most famous members of the scientific community, whilst taking very little part in scientific organisation and politics. I attempt to show that he had very definite views on the importance of men of science to society, and on their relationship to other groups such as instrument makers, despite this aversion to participation in scientific politics. His views on this subject are also integrated with his religious beliefs. The chapter also discusses Faraday's relationship with the instrument makers he employed as an individual and as a worker at the Royal Institution, and contrasts his work and context with another group of practitioners interested in electricity, a group who included instrument makers among their number but who pursued their studies on the periphery of the elite scientific community and its institutional base. This group, which found one focus of activity in the short-lived London Electrical Society, has been studied in recent years by David Gooding[57] and

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[57] D.Gooding, ""Magnetic curves" and the magnetic field: experimentation and representation in the history of a theory", in D.Gooding, T.J.Pinch, and S.Schaffer (eds.), *The Uses of Experiment. Studies in the Natural Sciences,* (Cambridge, 1989), pp.183-223.

Iwan Rhys Morus.[58]

Having discussed four individual members of the scientific elite, the following chapter is concerned with a fuller development of a perception of the nature of this group and its place in society. The focus in Chapter 7 is on the British Association for the Advancement of Science. Founded in 1831, the Association contained the leading philosophers and instrument makers in its number, and claimed to be more of a forum for the real man of science than could be the Royal Society, for which calls for reform at the time were vehement. Thus the Association's declared interests were seen as a direct expression of the interests of the scientific community. This chapter considers the role that instrument makers were able to play in the new institution, and brings out the theme of *career advancement* as the primary motive behind makers' participation in the annual meetings. Makers could display instruments and contribute papers at the meetings which, given that they took place at different locations in the British Isles, spread the makers' names in circles of potential employers at a local and national level, and hence served as useful investments which, if successful, guaranteed at least some financial returns.

In contrast, the philosophers used the Association also as a means of career advancement, but with social status, not

[58] I.R.Morus, *The Politics of Power. Reform and Regulation in the work of William Robert Grove,* (Ph.D. Thesis, University of Cambridge, 1989), esp.pp.12-46.

financial return, as their goal.[59] Chapter 7 is concerned with the means by which this group of philosophers took part in the proceedings of the B.A.A.S. and were able to use it as a forum in which to state their claims to recognition as an elite group in society and as the chief agents of technological progress and the creation of national wealth. Governing power in the B.A.A.S. was to be vested in a group of philosophers who subordinated the activities of other groups to the progress of society and the furtherance of their own careers - which meant the enhancement of their personal status, not the accumulation of wealth. Instrument makers, though granted a role within the Association and within society, were one such subordinate group. Any sort of power within the scientific community as defined by the B.A.A.S. was denied to the instrument maker.

It should be noted that I include the British Association as a London-based institution in this thesis, despite the fact that its annual meetings never took place in London. The justification for this is that its Council, which carried on its business between meetings, was based in London. In addition, governing power in the Association was wielded by philosophers who were very much metropolitan-based. Thus the inclusion of the British Association, while it may initially seem anomalous amongst these other London-based case studies, can be justified.

The final case study (<u>Chapter 8</u>) discusses the other avenues by which the ambitious instrument maker could attempt to gain

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[59] This theme is discussed in J.B.Morrell and A.Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*, (Oxford, 1981).

status within the scientific community. Firstly, the makers role in the new scientific societies which burgeoned in the early nineteenth century (such as the Astronomical Society and the Microscopical Society of London) is addressed. This is followed by an analysis of the ways in which makers could employ publication media such as books and scientific journals. Having discussed the various ways in which the instrument maker could make use of institutions and publishing media within the scientific community, and argued that his activity in these areas was motivated by a desire to promote his trade, the chapter goes on to consider at length the nature of the business pressures which the maker faced in the nineteenth century, and which caused him to have less time to devote to original work than had been the case in the eighteenth century.

6. The Philosopher in the Nineteenth Century: The Role of Political Economy.

By means of these 7 case studies, then, this thesis attempts to show the loss of status in the scientific community for the scientific instrument maker in the first half of the nineteenth century, and to ascribe it to three broad processes: the increasing complexity of the designs demanded for new instruments, the effects of the Industrial Revolution in giving rise to an expanded market, and the emergence of a new elite in society from which the instrument makers were excluded. The emphasis throughout is on the latter process, and just as important in this thesis as the loss of status on the part of the maker is the means by which the philosopher was able to articulate his claim to membership of an elite group in society.

A key element in the philosopher's strategy, a linking theme in the thesis as a whole, was to emphasise the relevance of his activities to *political economy.*

The philosophers could show by using political economy that their activity and their discovery of scientific principles could, if properly applied to practical ends, produce wealth for the State, and thus in turn for the individual.[60] The most frequently cited example was of course the steam engine.[61] Political economy was in many ways the ideal vehicle for the legitimation of their practice, because it was so popular at the time. Whereas not everyone read the latest Jane Austen novel, or the latest work by famous men of science such as Humphry Davy or Michael Faraday, or a new work on astronomy, everyone from Royalty to factory workers read political economy.[62] The leading examples of the science were David Ricardo's *Principles of*

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[60] Besides this idea, of the philosophers using political economy as a justification of their enterprise, it has also been shown recently that political economy could be used as a resource to solve problems *within* natural philosophy: see M.N.Wise (with the collaboration of C.W.Smith), "Work and Waste: Political Economy and Natural Philosophy in Nineteenth Century Britain", *History of Science,* 1989 (27), pp.263-301, 391-449, and 1990 (28), pp.221-61.

[61] See for example "Address by Roderick Impey Murchison and Major Edward Sabine", *Report of the Tenth Meeting of the British Association for the Advancement of Science, held at Glasgow in 1840,* pp.xxxv-xlviii, on p.xxxv which eulogises, in his native city, the intellectual feat of Watt in guaranteeing progress. In the rest of this thesis these publications are abbreviated to, for example, *B.A.A.S. Report, Glasgow, 1840,* etc.

[62] For an assessment of the role of political economy, see M.Berg, *The Machinery Question and the Making of Political Economy 1815-1848,* (Cambridge, 1980).

Political Economy and Taxation,[63] James Mill's *Elements of Political Economy,*[64] his son John Stuart Mill's rather later *Principles of Political Economy,*[65] and above all Adam Smith's *Wealth of Nations.*[66] Smith's exposition of the virtues of the division of labour gave an explicit role to the philosopher in the creation of wealth. He wrote:

> All the improvements in machinery, however, have by no means been the inventions of those who had occasion to use the machines... Many improvements have been made by those who are called philosophers or men of speculation, whose trade it is, not to do any thing, but to observe every thing, and who, upon that account, are often capable of combining together the powers of the most distant and dissimilar objects. In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens.[67]

Smith thus gave a role to the man of science in the creation of wealth. In addition, Smith distinguished between productive and unproductive labour; productive labour which adds to the value of the subject upon which it is bestowed, and unproductive labour, which does not.[68] While the latter can have a certain importance, and among this class of labour he includes that of churchmen, lawyers, and physicians, as well as musicians and

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[63] D.Ricardo, *Principles of Political Economy and Taxation,* (London, 1817).

[64] James Mill, *Elements of Political Economy,* (London, 1821).

[65] John Stuart Mill, *Principles of Political Economy,* (London, 1848).

[66] A.Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations,* (London, 1776). New edition, edited by R.H.Campbell, A.S.Skinner, and W.B.Todd, (Oxford, 1976). Subsequent page numbers refer to new edition.

[67] *Ibid.,* p.21.

[68] *Ibid.,* p.330.

buffoons, productive labour is emphasised as more important, especially when elsewhere in the book he calls the appellation *unproductive* "humiliating".[69] Philosophers could thus pride themselves that their labour was productive and so in some sense superior to that of the three traditional professions of divinity, law, and medicine. It should be noted of course that the labour of the instrument maker also complied with the definition of *productive* labour; however, the man of science extended Smith's discussion to make his *the* most vital role in the creation of wealth, in so doing elevating himself above classes such as instrument makers, engineers, and indeed artisans in general, in a social hierarchy.

Later writers on political economy endorsed Smith's views on the value of the work of the philosopher. John Stuart Mill, writing in 1848, ascribed the same central role to the philosopher in assuring national wealth:

> In a national, or universal, point of view, the labour of the savant, or speculative thinker, is as much a part of production in the very narrowest sense, as that of the inventor of a practical art; many such inventions having been the direct consequences of theoretic discoveries, and every extension of knowledge of the powers of nature being fruitful of applications to the purposes of outward life...[Mill cites the electro-magnetic telegraph as an example of this]...No limit can be set to the importance, even in a purely productive and material point of view, of mere thought...But when...we...consider not individual acts, and the motives by which they are determined, but national and universal results, intellectual speculation must be looked upon as a most influential part of the productive labour of society, and the portion of its resources employed in carrying on and in remunerating such labour as a highly productive part of its expenditure.[70]

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[69] *Ibid.*, p.664.
[70] J.S.Mill, *op.cit.* (note 65), p.41.

Mill's work was rather unusual in political economy texts of the period in making such explicit reference to the vital role seen as being played by the man of science in the economic progress of the nation. While personal economic advancement was not a primary motivation for the group of philosophers discussed in this thesis, demonstration that their work caused an economic advance was important as a legitimation of their activity. Mill's work effectively expounded other aspects of their philosophy as well - for example their perception of their position in a social hierarchy above classes such as the artisan community would have been supported by passages such as the following:

> Of the features which characterise the progressive economical movement of civilised nations, that which first excites attention, through its intimate connection with the phenomena of Production, is the perpetual, and so far as human foresight can extend, the unlimited, growth of man's power over nature...This increasing physical knowledge is now, too, more rapidly than at any former period, converted, by practical ingenuity, into physical power...[Mill again uses the telegraph as an example]...the manual part of these great scientific operations is now never wanting to the intellectual; there is no difficulty in finding or forming, in a sufficient number of the working hands of the community, the skill requisite for executing the most delicate processes of the application of science to practical uses.[71]

In other words, the role played by the artisan, such as the instrument maker, involved manual work, not thought, and however skilled the work, it was not as important in terms of the generation of wealth as was the intellectual labour of the designer of such tools.

I do not wish to claim that philosophers used Mill directly

[71] *Ibid.,* p.696.

as a resource in order to claim their elite social status in nineteenth-century Britain. Rather, I wish to show by this reference to Mill's work that philosophers could point to the doctrines of political economy, when seeking to establish claims to intellectual authority and elite status. The *Principles of Political Economy* has also been quoted to demonstrate that themes manifest to the leading social thinker of his generation - themes such as the central position of the philosopher in national economic advance - are integral to this thesis.

During the course of the case studies it will emerge that some philosophers had a different view of the role the philosopher played in the progress of technology than others - William Whewell's vision was unlike that of Charles Babbage, for example. But political economy *per se* was employed across the range of commitments prevalent in the group of philosophers discussed, and for all who claimed membership of the elite scientific community, groups of actors without claims to the central role in economic progress, such as instrument makers, were outside that community. In the next chapter I propose to discuss the institution which, at the start of the nineteenth century, would have been regarded by many as identical with the *scientific community* to which I have been referring in this Introduction - the Royal Society. As such it forms the natural starting point for the arguments I wish to make about those engaged in the scientific enterprise in nineteenth-century Britain.

CHAPTER TWO

THE ROYAL SOCIETY

The elite precision instrument maker was a full member of the scientific community in the eighteenth century. This thesis aims to show that this was no longer the case by the first half of the nineteenth century. It is therefore proposed to examine the position of the leading makers in the context of their membership of the country's first scientific society - the Royal Society - in the nineteenth century, in comparison with their participation in its affairs and contributions to its learned journal in the eighteenth century, as described in the Introduction (Chapter 1). The analysis covers two basic issues:

(i) the value to the instrument makers of Fellowship and participation in the affairs of the Society; and

(ii) the use that those in positions of power within the Society made of groups such as instrument makers.

I will argue that the dominance of various interest groups amongst the philosophers caused a general loss of status to the instrument maker within the Society, though participation in the Society's activity was still an important career goal for the leading makers.

The important point to note, before proceeding to any analysis of the role of a particular group within the Society over the period in question, is that the composition of the dominant interest groups was not constant in time. For the first 20 years of the nineteenth century the Society was a virtual dictatorship under the Presidency of Sir Joseph Banks,[1] though on Banks' death a rather different state of affairs prevailed, with a "scientific" President, Sir Humphry Davy, at the helm.[2] The Presidencies of Davies Gilbert[3] and the Duke of Sussex were characterised by demands for reform of the Society by active minorities amongst the Fellowship. This period in the history of the Society, and in particular the events leading up to John Herschel's defeat at the hands of the Duke of Sussex in the Presidential Election of 1830, has been extensively discussed by historians of science,[4] and this chapter is not intended to be a contribution to that discussion. Rather, I mention the

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[1] Banks' reign and the "Banksian Learned Empire" are discussed in D.P.Miller, *The Royal Society of London 1800-1835: A Study in the Cultural Politics of Scientific Organization* (Ph.D. Thesis, University of Pennsylvania, 1981), esp.pp.9-64. See also H.C.Cameron, *Sir Joseph Banks. The Autocrat of the Philosophers,* (London, 1952), and E.Smith, *The Life of Sir Joseph Banks,* (London, 1911).

[2] On Davy's Presidency, see Miller, *op.cit.* (note 1), pp.243-88. Also of interest are M.B.Hall, *All Scientists Now. The Royal Society in the Nineteenth Century,* (Cambridge, 1984), pp.16-32, and L.F.Gilbert, "The Election to the Presidency of the Royal Society in 1820", *Notes and Records of the Royal Society of London,* 1955 (11), pp.256-79. A more recent extension of the work in Miller's Ph.D. Thesis is D.P.Miller, "Between Hostile Camps: Sir Humphry Davy's Presidency of the Royal Society of London 1820-1827", *British Journal for the History of Science,* 1983 (16), pp.1-47. On Davy's career, see J.A.Paris, *The Life of Sir Humphry Davy,* (London, 1831).

[3] A.C.Todd, *Beyond the Blaze. A Biography of Davies Gilbert,* (Truro, 1967).

[4] L.P.Williams, "The Royal Society and the founding of the British Association for the Advancement of Science", *Notes and Records of the Royal Society of London,* 1961 (16), pp.221-33; Miller, *op.cit.* (note 1), pp.350-72; Hall, *op.cit.* (note 2), pp.45-62; R.M.Macleod, "Whigs and Savants: Reflections on the Reform Movement in the Royal Society", in I.Inkster and J.B.Morrell (eds.), *Metropolis and Province. Science in British Culture 1780-1850.* (London, 1983), pp.55-90.

Presidential history of the Society in the period only to emphasise that the existence of different individuals in positions of authority would be expected to have different consequences for groups with interests in the affairs of the Society. This chapter examines the similarities between the attitudes to instrument makers across the different regimes, and I argue that, whatever the composition of the Council, whether it be predominantly aristocratic or mainly of men of science, the instrument makers were subordinate to higher interests.

1. The "Reform Movement" in the Royal Society.

Historiography of the Royal Society in the early nineteenth century is somewhat plagued by an abstract notion of a "reform movement" seeking to uphold the claims to power in the Society of those genuinely interested in science, over those seeking only the prestige of the letters F.R.S.[5] Unfortunately, it has been usual only to characterise this "reform movement" in very vague terms so that it has been difficult to ascertain how exactly it operated, or, for that matter, if it ever existed except as a historical construct to explain retrospectively the *effect* of reform. In recent years, however, work has appeared which has been a welcome addition to the literature on the Royal Society and which makes explicit the motives of various interest groups within the Society wishing to effect change.

Miller has argued that historians in the past have suffered from imagining a large group of reformers with a single unified

[5] Particularly weak in this respect is H.G.Lyons, *The Royal Society 1660-1940: A History of its Administration under its Charters*, (Cambridge, 1944), pp.228-71.

strategy where none existed.[6] His argument is that the reform group in the Royal Society in the 1820s drew upon four main groups, all of which existed to some extent in the early part of the century as well. Miller's groups are, firstly, a "Cambridge Network", based around the University, secondly, a group of London mathematical practitioners (the group this essay will be concerned with), thirdly, a group of geologists active in the Geological Society of London, and fourthly, a group of military and naval men devoted to scientific activity (Miller calls them "Scientific Servicemen").

The "Cambridge Network" gained cohesion with the formation of the Analytical Society by John Herschel, Charles Babbage, and George Peacock in 1812.[7] This Society had as its declared aim the reform of British mathematics, upon a French model, and provided a base for the friendships around which the "Network" developed, as well as giving a focus to young philosophers with grievances against the prevalent establishment. Although Herschel and Babbage gave up their work in pure mathematics fairly early in their careers, mathematical ability of the very highest order was maintained by them and by other members of the "Network" as a necessity for someone to be considered a real philosopher.[8] These men saw themselves as having such ability and used this vision as part of their rhetoric to claim intellectual superiority over

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[6] Miller, *op.cit.* (note 1), pp.75-6.

[7] For background on the Analytical Society, see J.M.Dubbey, *The Mathematical Work of Charles Babbage,* (Cambridge, 1978); S.S.Schweber, "Prefatory Essay", in S.S.Schweber (ed.), *Aspects of the Life and Thought of Sir John Frederick William Herschel,* (2 volumes, New York, 1981), Volume 1, pp.54-67.

[8] Miller, *op.cit.* (note 1), pp.77-96.

other groups in society and other groups of participants in the scientific enterprise. As a prevalent ideology among the members of the Cambridge Network, this notion of mathematical competence as a guarantor of intellectual and social superiority will be a recurrent theme in this thesis, particularly in the chapters on Cambridge-educated mathematicians Babbage and Airy.

However, in this chapter I wish to concentrate on the second of Miller's reform groups, the London mathematical practitioners.[9] Essentially, a *mathematical practitioner* is seen as someone who was capable of applying mathematics to the solutions of everyday problems in astronomy, navigation, surveying, etc.. As such application generally involved mathematical instruments we can see the significance of the category, because the instrument maker, certainly in the eighteenth century, was a central member of this group, and, as we have seen, also of the scientific community as a whole. In the nineteenth century, however, the maker became not only peripheral to the scientific community, but also in some sense peripheral to the interests of the sub-groups within the mathematical practitioners who joined the other groups seeking reform. I would maintain that it was those mathematical practitioners who showed an interest in the advancement of theory, such as Peter Barlow and Samuel Hunter Christie,[10] who were important to Miller's analysis, whereas the concept of scientific reform was a secondary interest to instrument makers, who became more concerned with their businesses than with

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[9] *Ibid.,* pp.96-120.
[10] Their work is discussed in *ibid.,* pp.102-3.

effecting change within the Royal Society. So in a sense the instrument makers' interests grew apart from the interests of the metropolitan mathematical practitioners, a state of affairs that was well established by the end of the Banks era.

It was also at this time that the foremost of the mathematical practitioners' claims to status within the scientific community were being heard, for during the period of Banks' presidency they had been rather external to the power groups of the Royal Society. This can partly be attributed to the generally aristocratic nature of most of the Fellowship, who tended to deny power to those of lesser social standing, and partly to the removal of Charles Hutton, a mathematical teacher, textbook writer, and researcher, from the post of Royal Society Foreign Secretary in 1784.[11] Miller shows that this resulted in the secession of a number of the mathematical practitioners from the Society, and that the bitterness of 1784 continued for many years to shape the attitudes of the mathematical practitioners, and the aristocratic Royal Society elite under Banks, towards each other.[12] Nevil Maskelyne, as Astronomer Royal, was one of Hutton's leading defenders in these disputes, and he was able to ensure that the mathematical practitioners had a role to play in the Royal Observatory and the Board of Longitude, but the attitude of Banks and his supporters meant that the practitioners involved lost their previously central role in the affairs of the Society.

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[11] *Ibid.*, pp.108-9; Cameron, *op.cit.* (note 1), pp.128-34; Lyons, *op.cit.* (note 5), pp.212-14.

[12] Miller, *op.cit.* (note 1), pp.108-15.

This loss of status necessarily extended to some degree to the whole community of London mathematical practitioners, and as a result the mathematical teachers and researchers of the community were forced to pursue their work outside the Royal Society, with instrument makers entering Society affairs mainly as necessary workmen. The small part that those such as Edward Troughton were able to play in research and Society business only served to highlight the general trend. By the time the practitioners came to form alliances of mutual interest with other dissatisfied groups such as the Cambridge mathematicians, the division of labour had in practice further emphasised the position of the maker as *artisan* so that any claims to intellectual status and power within the scientific community as a whole which the practitioners made, were made by an element which did not feature the instrument maker among its number.

Hence, when Miller refers to the discontent of the mathematical practitioners with the Royal Society establishment during the Presidencies of Davy and Gilbert in the 1820s, he is dealing with only a sub-group of the practitioners as he initially characterised them, a sub-group which included those seeking to establish claims to intellectual authority by their mathematical expertise, but a sub-group which did not include the instrument maker, whose status now was effectively that of a skilled manual workman.

In addition, it was members of this sub-group of the mathematical practitioners claiming intellectual authority, and members of the "Cambridge Network", who could point to their achievements in instrument design as one facet of their ability and one justification for the status they sought. The real innovations that were being made in instrument design in this period came from the side of these men, not from that of the makers. Mathematics became a method of invention in connection with the study of physical instruments, and this use of mathematical theory increased accuracy to a point which would have been impossible by mere rule of thumb workmanship.[13] Thus philosophers versed in mathematics became the inventors of new instruments, and their constructors were *only capable of making* practical refinements, owing to the complexity of the theory behind the innovations (this theme will be discussed further in the chapter on Airy). Such effects further diminished the instrument makers' role in scientific research. I would now like to examine in more detail the role that the maker managed to play in the Royal Society in the face of such changes.

<u>2. The Instrument Maker as a Fellow of the Royal Society.</u>

Although Fellowship of the Royal Society was a relative commonplace among the top London precision instrument makers of the eighteenth century, and contributions of research papers on instrument design and other scientific topics (for example, Short on astronomy and Nairne on electricity) abounded, these ceased to be the case in the early nineteenth century. *If* membership of the scientific community was to be judged on contributions made to the *Philosophical Transactions*, the bare statistic that only five such contributions were made by instrument makers in the nineteenth century, compared with over 80 in the last 60 years

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[13] *Ibid.*, p.174.

of the eighteenth century, seems to suggest that instrument makers could no longer claim to be members of that community.[14]

This analysis aims to go beyond that bare statistic in order to examine the part that those makers who did become Fellows played in the work of the Society, and also examines the work done for the Society by instrument makers who did not become Fellows. In this way, I hope to show that the statistic representing the decrease in research work submitted for publication in the *Philosophical Transactions* was a true reflection of the diminishing role these artisans played in the affairs of the country's first scientific society.

Five London instrument makers became Fellows of the Royal Society in the nineteenth century: Edward Troughton, in 1810, George Dollond (1819), Thomas Jones (1835), William Frodsham (1839), and William Simms (1852).[15] All of these men had some family or business connection with makers in the eighteenth century as well. Troughton's brother John was a top maker in the latter part of the century, and Simms was Edward Troughton's partner. Dollond was the nephew of Peter Dollond, Jones had been an apprentice of Jesse Ramsden, and Frodsham was part of a long

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[14] These statistics have as a basic assumption my definition of *instrument maker* given in chapter 1.

[15] See the List of Fellows in *The Record of the Royal Society of London*, (London, 1912). Thomas Grubb and his son Howard, both of Dublin, were also elected (in 1864 and 1883 respectively), though as a part of the Irish scientific community they fall outside the scope of this thesis.

line of famous chronometer makers of the same name.[16] Neither Frodsham nor Simms had research papers published in the *Philosophical Transactions,* Troughton and Jones wrote one such paper each,[17] and Dollond wrote two.[18] One maker who never became a Fellow also contributed a paper - Francis Watkins.[19] These were the only papers by men who made their living by constructing instruments which appeared in the foremost learned journal of the scientific community in the nineteenth century.

It is important to note, however, before setting any store by this dearth of contributions to the *Philosophical Transactions,* that the journal's composition in other subject areas had not been constant in the nineteenth century. For example, from 1800 to 1830 the journal did not contain a single paper on botany, and a decreasing number on geology and

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[16] For brief biographies of these men, see E.G.R.Taylor, *The Mathematical Practitioners of Hanoverian England, 1714-1840,* (Cambridge, 1966).

[17] E.Troughton, "An Account of a Method of dividing Astronomical and other Instruments by ocular Inspection; in which the usual Tools for grinding are not employed", *Philosophical Transactions of the Royal Society of London,* 1809, pp.105-45; T.Jones, "Description of an Improved Hygrometer", *Philosophical Transactions of the Royal Society of London,* 1826, pp.53-4.

[18] G.Dollond, "An Account of a Micrometer made of Rock Crystal", *Philosophical Transactions of the Royal Society of London,* 1821, pp.101-4; G.Dollond, "An Account of a Concave Achromatic Glass Lens, as adapted to the Wired Micrometer when applied to a Telescope, which has the property of increasing the magnifying power of the Telescope without increasing the diameter of the Micrometer Wires", *Philosophical Transactions of the Royal Society of London,* 1834, pp.199-203.

[19] F.Watkins, "On the magnetic powers of soft iron", *Philosophical Transactions of the Royal Society of London,* 1833, pp.333-42. This paper was communicated to the Society on behalf of Watkins by John George Children.

astronomy. These matters can be fairly easily accounted for, if it is realised, for example, that the Linnean Society, having been founded in 1788, provided an arena for the latest botanical research, outside of the Royal Society. Similarly, geologists and astronomers could communicate their researches just as easily to the Geological and Astronomical Societies as to the Royal. For the instrument makers no such specialist society existed as a forum for their research, and although some independent journals were around in the early nineteenth century,[20] the lack of research papers by instrument makers in the *Philosophical Transactions* can be taken as indicative of a general lack of written research by makers, where the dearth of botanical research in the journal cannot be taken as proof of a loss of interest in botany.

By considering the roles the three makers who were Fellows and who did make contributions to the *Philosophical Transactions* played in the Royal Society in the nineteenth century, general themes relevant to the changed role of the maker in the scientific community will become apparent. In short, the maker was able to use participation in the affairs of the Society as a convenient means of promotion of his own business, rather than as a disinterested contribution to the advancement of science (as had Ramsden, for example), but his participation was very

[20] By this I mean journals such as the *Philosophical Magazine*, and W.Nicholson's *Journal of Natural Philosophy, Chemistry, and the Arts.*

much subordinate to that of the different ruling elites in the Society, who employed makers to help in work, prosecution of which would be useful to the former in career terms.

3. Edward Troughton: The Instrument Maker as Artist.

The first maker to become a Fellow of the Royal Society in the nineteenth century was Edward Troughton. Born in 1753, Troughton became apprentice to, and then partner of, his elder brother John.[21] Edward considered that his brother:

> ...in the art of dividing, was justly considered the rival of Ramsden; but he was then almost unknown beyond the circle of the mathematical and optical instrument makers; for whom he was chiefly occupied in the division, by hand, of small astronomical quadrants, and Hadley's sextants of large radius.[22]

The younger Troughton, by 1800, had attained a pre-eminent position among the elite mathematical instrument makers of London by his practical work, in particular on large astronomical instruments. However, it was apparent that, unlike contemporaries such as Ramsden, written research was not forthcoming from Troughton, and also, unlike Ramsden and other makers, he did not have the full claim to membership of the scientific community that a Fellowship of the Royal Society would provide. That Troughton did make improvements in instruments like the other makers, however, is shown by a revealing letter which he wrote to *Nicholson's Journal of Natural Philosophy* in 1804:

> ...I have often thought of registering... the improvements which I frequently make in

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[21] A.W.Skempton and J.Brown, "John and Edward Troughton, Mathematical Instrument Makers", *Notes and Records of the Royal Society of London,* 1973 (27), pp.233-62.

[22] Troughton, *op.cit.* (note 17), p.106.

> astronomical and other instruments. No man perhaps had ever a greater antipathy to writing than myself, and drawn as I am into the vortex of business, and working with my own hands from nine to twelve hours in a day, leaves me with but little time for literary pursuits.[23]

The sentiments expressed here show that even by the early 1800s, an instrument maker's business was becoming very much a full-time occupation. Economic pressures forced the maker to devote almost all his time to the actual running of the business, where in the past a maker such as Short or Ramsden or the Dollonds could as easily devote their time to research of a scientific nature. In addition, though it is somewhat early to make general comparisons, the improvements which Troughton referred to his having made in instruments were very much *practical* improvements, whereas the eighteenth-century maker's discourse was concerned with the improvements he made in the *principles* of instrument design.

Troughton's main written contribution to research and to the furtherance of the instrument maker's art was a paper submitted to the Royal Society on his method of dividing the scales of astronomical instruments, published in the *Philosophical Transactions* for 1809,[24] and earning him the Society's highest accolade, the Copley Medal, for that year. The paper was communicated to the Society by the Astronomer Royal, Nevil Maskelyne, who was well acquainted with Troughton's work from commissions which had been executed by the maker for the Royal

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[23] E.Troughton, "Description of a Tubular Pendulum", *Journal of Natural Philosophy, Chemistry and the Arts,* 1804, pp.225-30, on p.225.

[24] Troughton, *op.cit.* (note 17).

Observatory at Greenwich. The letter which Troughton wrote to Maskelyne in initially presenting him with the paper illustrates the artisan's view of his status with respect to a top man of science, and also of the state of his own art. As such it is worth quoting in full:

> The science which you profess, and the Art which it has fallen to my lot to cultivate, are so nearly allied that, had I been personally unknown to you, and a stranger to the patronage which you have always given to the useful arts, I should still have wished the papers annexed to have passed through your hands to the public. You will readily thence infer, how much I feel myself flattered by having obtained from your condescension, the privilege of their being presented to the Royal Society, through a channel which must secure for them the most favourable reception.
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> My reputation for the dividing of Astronomical and other instruments is by no means unknown to the world, but the means by which I accomplish it, I have hitherto thought proper to conceal. And if that concealment had been essential to the advancing of that reputation, or to the immediate security of my own interests, it is probable that it might still longer have rested with myself. Relying, however, as I do, on the probability that I shall find sufficient employment while I am capable of active life, I know of no honourable motive that should prevent me from allowing it to be useful to others.
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> How a young Artist, who may just be beginning to make his way to fame or wealth, may receive it, I know not, but I wish him to understand, that I consider myself now in the act of making him a very valuable present.[25]

Troughton's outlook in this letter may again be usefully contrasted with that of Ramsden or the Dollonds. Ramsden saw his improvements in instrument design as a contribution to the advancement of science; Troughton's paper is explicitly seen as a contribution to the instrument maker's *art*, and although I have

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[25] E.Troughton to N.Maskelyne, 23 June 1808, Miscellaneous Correspondence 1.35, Royal Society Archive.

stressed the increasing economic pressures which the instrument maker experienced in the wake of the Industrial Revolution, it should not be assumed that the maker responded to these pressures in ways concerned only with the expansion of their business. As this letter illustrates, gentlemanly honour could be part of the code of conduct of the instrument maker just as much as it was for the philosopher - hence Troughton's willingness to make his method public rather than attempt to continue to make a profit from it. Of course it will have been the case that he wished to profit from his method in another way - by its making his name more well known in the scientific community. The paper in the *Philosophical Transactions* and the Copley Medal would have helped to achieve this, and these were followed in 1810 by his election to a Fellowship of the Royal Society, the first maker in the nineteenth century to be so honoured.

Troughton's paper was a particularly lengthy one for the *Philosophical Transactions,* running to nearly 40 pages of text, in addition to a number of technical drawings. His method of dividing was seen as providing both an economy of time for the workman, and an economy of skill in the sense that if the steps as Troughton laid them down were followed, any workman could divide a scale as accurately, and quicker, than the most skilled artisan could by any other method.[26] It is not my purpose to enter here into the technical details of the method of scale dividing expressed in the 1809 paper - the paper itself conveys these better than any paraphrase. However, it should be pointed

[26]Troughton, *op.cit.* (note 17), pp.136, 142.

out that Troughton's method was explicitly seen as applying to instruments of *large* radii - the dividing engine developed by Ramsden was acknowledged as still being the best method of division of smaller instruments.[27] But the main point about this paper is that it was a *practical* addition to the instrument maker's repertoire, a testimony to Troughton's practical expertise in his art, but an addition that was seen as giving opportunity to a wider range of artisans than before to produce accurate work, which would inevitably reduce the claim to special status of the elite instrument maker:

> The number of persons dividing originally have hitherto been very few; the practice of it being so limited that, in less than twice seven years, a man could hardly hope to become a workman in this most difficult art...if, by the method here revealed, I have not rendered original dividing equally easy with what copying was before, I have spent much labour, time, and thought in vain. I have no doubt indeed, that any careful workman who...has the ability to construct an astronomical instrument, will, by following the steps here marked out, be able to divide it, the first time he tries, better than the most experienced workman, by any former method.[28]

Whatever the implications of Troughton's method of scale dividing for future instrument making practice, in terms of the gradual removal of the individual skilled element in the process, it was an effective means of establishing his own name in scientific and Royal Society cirles. Besides constructing a number of instruments for all the major institutions in London, and for many observatories overseas, work which established his position as the leading instrument maker of his day, Troughton was able to serve on Royal Society committees of inquiry into

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[27] *Ibid.*, p.112.
[28] *Ibid.*, pp.133-4.

various subject areas.

It was often the case that the Royal Society's advice was sought by the Government on questions which had been raised in Parliament. One such instance was in 1816, and concerned the request for determination of the length of a pendulum vibrating seconds in the latitude of London.[29] Besides notable Fellows such as William Hyde Wollaston, Astronomer Royal John Pond, and President Banks, Troughton served on this committee in order to add his expertise on instrumental matters concerned with the determination.[30] This is not to say that on every occasion that instrumental considerations entered the work of a delegated committee, an instrument maker was included. For example the 1819 Commission of Inquiry on Weights and Measures (not strictly speaking a Royal Society committee, though all its members were in fact Fellows) included only philosophers, with no instrument makers present.[31] However, it was true that the instrument maker was able to aspire to some part within the affairs of the Royal Society by serving on certain of such committees. This work of committees will be considered at more length later.

Troughton was to maintain his position as the pre-eminent member of the instrument making community throughout his life, with his participation in the affairs of the Royal Society as a major contributory factor in this. It is worth noting that he was prevented from acquisition of the full range of instrument making

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[29] C.R.Weld, *A History of the Royal Society,* (2 volumes, London, 1848), Volume 2, pp.252-3, (all subsequent references are to this volume). See also Hall, *op.cit.* (note 2), p.14.

[30] Weld, *op.cit.* (note 29), p.253.
[31] *Ibid.,* pp.255-70.

skills by the defect of colour-blindness, which meant he could not do any lens work - he usually delegated this work to opticians such as George Dollond or Charles Tulley.[32]

Troughton took William Simms into partnership in 1826, by which time he was an old man, so that Simms immediately took over effective running of the business. He had for some time regarded Simms as the best maker in the London artisan community,[33] and by 1831, after a term as Vice-President of the Royal Astronomical Society, Troughton retired.[34] Taylor points out that a reviewer in the *Philosophical Magazine* was able to write:

> Mr.Troughton stands quite unrivalled in the construction of original astronomical instruments ...we assert that Troughton does, and always will hold that rank among makers of astronomical instruments that Sir Isaac Newton does among philosophers.[35]

Captain Kater, meanwhile, described Troughton as "second to none in the Kingdom in point of accuracy".[36] Clearly Edward Troughton's position as the foremost instrument maker of his era was not in doubt. However, the nature of the position of the foremost instrument maker of the day with respect to the Royal Society and the scientific community had changed. Troughton had

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[32] R.Sheepshanks, "Memoir of Edward Troughton", *Memoirs of the Royal Astronomical Society,* 1836 (9), pp.283-90.

[33] E.Troughton to the Secretary of the Royal Society, 14 June 1824, Greenwich Observatory Correspondence, Item 30, Royal Society Archive.

[34] Sheepshanks, *op.cit.* (note 32).
[35] Taylor, *op.cit.* (note 16), p.299.
[36] *Ibid.*

made little written contribution to learned journals,[37] whereas earlier makers such as Short and Nairne had been, to say the least, prolific in their output. In addition, the nature of such research had changed from being a contribution to instrument making design principles to being an addition to the instrument maker's art. On Troughton's death in 1835, the maker's position as very much an *artisan,* albeit a skilled one, was established, and only two were Fellows of the Royal Society, underlining this phenomenon.

4. George Dollond: The Instrument Maker as Practical Expert.

One of these Fellows, George Dollond, came from a secure family background in instrument making. The other, Thomas Jones, had been an apprentice to Jesse Ramsden. Dollond, born George Huggins in 1774, had changed his name to Dollond, and was Peter Dollond's nephew.[38] George Dollond had a keen appreciation of the benefits that his career had received from the family connection with two of the eighteenth century's leading instrument makers, so much so that he wrote to the Royal Society in 1842:

> Being desirous of paying every respect in my power to the memory of my late grandfather, Mr.John Dollond, the Inventor of the Achromatic Refracting Telescope, for which he was honoured by the Society with the Copley Medal, and having in my

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[37] There are only 6 entries under Troughton's name in the *Royal Society Catalogue of Scientific Papers,* and only one substantial paper after his election to the Fellowship of the Royal Society - E.Troughton, "An Account of the Repeating Circle, and of the Altitude and Azimuth Instrument", *Memoirs of the Astronomical Society,* 1822 (1), pp.33-54.

[38] Taylor, *op.cit.* (note 16), p.335.

> possession a small but highly finished portrait of that most excellent man, from whose talents I have received so many advantages...[39]

Dollond goes on to praise his grandfather further and to propose to make a present of the portrait, to be hung in the Meeting Room of the Society. The following year we find him proposing to make another present, this time of a bust of John Dollond.[40]

Work which Dollond did for his uncle, and for Edward Troughton, (whose colour-blindness prevented him from undertaking optical work), established his reputation in scientific circles, which was further endorsed by his election to a Fellowship of the Royal Society in 1819.[41] However, unlike his famous grandfather, he could not point to a collection of original optical research as a basis for his election - rather it was a reward for *practical* skill in instrument making.

The two research papers which Dollond contributed to the *Philosophical Transactions* subsequent to his election made him the most prolific nineteenth-century instrument maker in this respect. Both papers were very short, certainly compared to the 1809 paper by Troughton which had won the Copley Medal. In 1821 Dollond's first paper appeared, 3 pages of text on a type of micrometer made of rock crystal he had developed.[42] Even the title makes it apparent that this was a minor practical variation in an existing instrument, rather than an instrument on entirely

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[39] G.Dollond to the President and Council of the Royal Society, 6 June 1842, Miscellaneous Correspondence 3.222, Royal Society Archive.

[40] G.Dollond to J.W.Lubbock, 3 November 1843, Miscellaneous Correspondence 3.308, Royal Society Archive.

[41] *The Record of the Royal Society of London*, p.383.

[42] Dollond, *op.cit.* (note 18).

new principles. Also, whilst it would be untrue to claim that Dollond or any other instrument maker was ignorant of mathematics, the paper did not contain any mathematical equations, making it very much an address to a *practical,* rather than a theoretical, question.

Dollond's second paper was concerned with a concave achromatic glass lens, adapted to the micrometer of a telescope.[43] Published in 1834, this *application,* as Dollond acknowledged, arose from an *idea* on the part of a philosopher, Peter Barlow. As Dollond wrote:

> The achromatic lens which I have applied to the wired micrometer, and which has been found to produce such very considerable advantages to that instrument, arose out of a trial that was made at the suggestion of Professor Barlow, for the purpose of improving the chromatic aberrations which affected the field of the eye-glasses applied to the telescope invented by that gentleman with a fluid correcting lens, and made by myself for the Royal Society.[44]

In other words Dollond acknowledged that the initial intellectual labour was provided by a man of science, whilst maintaining that his practical and manual labour were an important part of the process by which the idea was brought to fruition:

> ...it [is] not...my wish to take credit to myself for anything like an invention, but merely for the application of the lens to the micrometer, as I am fully convinced that a concave lens, either simple or achromatic, was never so applied before.[45]

This is essentially the point I wish to emphasise about the nature of the contributions made to the journal by instrument makers in the nineteenth century - not only were they rare, but

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[43] Dollond, *op.cit.* (note 18).
[44] *Ibid.,* p.199.
[45] *Ibid.,* p.203.

they were very much examples of ingenious practical application but no more. The deeper principles upon which instrument design depended were not touched upon. Barlow's own contemporaneous paper illustrated the breadth of his interest in the subject, as an extension of Dollond's thinking on *one* situation in which the concave achromatic lens could be applied in practice:

> The great advantage which has attended Mr.Dollond's ingenious application of the negative achromatic lens to the micrometer eyepiece, seems to make it desirable that the principles on which that lens is constructed, and its general application, should be more fully illustrated than is done... by him in his recent paper in the *Philosophical Transactions...* [The lens] would most likely have been altogether lost sight of, had it not again occurred to Mr.Dollond to try its effect on the micrometer eyepiece for the Rev. Mr. Dawes. It is therefore to Mr.Dollond we are indebted for snatching the lens from the oblivion into which I had allowed it to fall.[46]

Thus Dollond's ingenuity was acknowledged, but Barlow's superior expertise in scientific terms was implied. Practical ingenuity in given situations, however, could be of considerable value to the Royal Society. It was owing to a desire for such practical expertise that Dollond was appointed to a committee whose brief was to consider the "philosophical deficiencies attendant on the manufacture of glass for scientific optical purposes".[47] As leading optician of his day, and a Fellow, Dollond was a natural choice for the committee, appointed on 6 May 1824, and which included Humphry Davy, William Hyde

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[46] P.Barlow, "On the Principle of Construction and general Application of the Negative Achromatic Lens to Telescopes and Eyepieces of Every Description", *Philosophical Transactions of the Royal Society of London,* 1834, pp.205-7, on p.205.

[47] Weld, *op.cit.* (note 29), p.396.

Wollaston, Davies Gilbert, James South, Thomas Young, Dollond, and four lesser-known philosophers.[48] Michael Faraday gave an account of the work which had been accomplished by the committee by 1830, of which the following is an extract:

> The Government not only removed the restrictions to experiments on glass occasioned by the Excise laws and regulations, but undertook to bear all the expenses of furnaces, materials, and labour, as long as the investigations offered a reasonable hope of success. In consequence of these facilities, a small glass-furnace was erected, and many experiments, both on a large and a small scale were made with flint and other glass... The researches... soon showed themselves to be a work of labour... and on May 5, 1825, a Sub-committee was appointed, to whom the direct superintendence and performance of experiments was entrusted... It was my business to investigate particularly the chemical part of the inquiry, Mr.Dollond was to work and try the glass, and ascertain practically its good or bad qualities, whilst Mr.Herschel was to examine its physical properties...[49]

However, Dollond, as a practical optician, did not find the samples that he was given to be of the desired quality for lenses, and the work of the committee and sub-committee in the seven years of their existence up to 1831 was acknowledged to be a failure in practical terms.[50] The work of the committee is of interest nonetheless, as an illustration of the role which the skilled instrument maker was required to play in scientific projects on behalf of the Royal Society.

Dollond was able to play a role in the Royal Society *Club* as well - part of the social side of Fellowship of the Royal

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[48] *Ibid.*

[49] M.Faraday, "On the Manufacture of Glass for Optical Purposes", *Philosophical Transactions of the Royal Society of London,* 1830, pp.1-57.

[50] See Hall, *op.cit.* (note 2), p.32, for a short account of the achievements of the Committee.

Society.[51] The Club, which dined before the meetings of the Society, was limited in membership to 40 Fellows, and Dollond first attended as the guest of one of these, the Astronomer Royal John Pond, in 1813.[52] This suggests that even by this stage in his career Dollond, though without any letters to his name, was a social acquaintance of the philosophers who employed him - he had certainly carried out optical work for Pond at the Royal Observatory. Dollond himself was admitted into the elite club in 1833,[53] and there is a record of his having attended the dinners on a regular basis, and having had Richard Owen, the anatomist, dine as his guest in January 1835.[54] This suggests that Dollond had a wider range of scientific acquaintances than merely astronomers needing telescopes from him, but the role which he was able to take in these scientific circles remained very much one of an expert in *practical* matters, with deeper theoretical matters not being of concern to him. The Council of the Royal Society continued to use him to advise on instrumental matters - with his increased visibility as a member of the Royal Society Club he was an obvious choice:

> Agreeable to the request of the Council Oct 28 1836 I have examined the telescope made upon Prof.Barlow's principle, and I find that the fluid has escaped from the correcting lens, which renders the telescope entirely useless, until it is refilled, or the spare lens put in its place. There is also a scratch upon the outer object-

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[51] For details of the history of the Royal Society Club, see A.Geikie, *Annals of the Royal Society Club,* (London, 1917).

[52] *Ibid.,* p.244.

[53] *Ibid.*

[54] *Ibid.,* p.316.

> glass, but not of sufficient importance to affect the performance of the telescope, when otherwise correct...[55]

Besides the technical advisory work for which the Council drew upon Dollond's expertise, an extensive catalogue of the instruments in the Society's possession was drawn up by the optician in 1834,[56] and he served on the Meteorological Committee of the Society in the 1830s[57] showing that his scientific interests extended beyond optics and optical instrumentation. However, the best illustration of his acknowledged expertise in practical optical matters is that he was called upon by the Society as a referee for papers submitted for publication in the *Philosophical Transactions.* It had been decided by the President and Council that the standard of the papers read and published needed to be raised if the Royal Society was to maintain its position as the foremost forum for research in the sciences. In accordance with this sentiment, the Secretaries were advised, in January 1833, that when there was any doubt as to the suitability of a paper for reading or publication, they should refer to such two Members of the Council as they considered most conversant with the subject of the Paper in question, to determine whether it should be read or not".[58] This system

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[55] G.Dollond to J.G.Children, 31 November 1836, Miscellaneous Correspondence 2.230, Royal Society Archive.

[56] *Instruments and Apparatus belonging to the Royal Society,* (London, 1834).

[57] Meteorological Committee Minutes etc., Domestic Manuscripts 3.107, Royal Society Archive.

[58] The system of refereeing was first announced in a vague way on 30 November 1832 - see *Proceedings of the Royal Society of London,* 1832, p.141., and Hall, *op.cit.* (note 2), p.68.

operated until 1838, when permanent committees in each department of science were appointed to deal with the refereeing of papers. Refereeing as such, however, dates from 1833.

A referee's report by Dollond exists in the Society's archives on a paper by J.B.Reade on single achromatic eye-pieces,[59] an area in which Dollond would have been expected to demonstrate some practical expertise. This is borne out by the report itself:

> In my opinion Dr.Reade's paper is very carelessly written, and contains more hearsay upon the principal point in his argument respecting Telescopes, than knowledge of the subject, and his recommendation for throwing aside the Huygenian and other systems of eye glasses likely to lead the truly practical observer into many difficulties.
> The proposed Achromatic lens can only act as a single lens of the same form and focus and cannot, as he allows, be made of so short a focal length as is in general required for such an adaptation. And the difficulties which must necessarily occur in finding out the proper "eye hole" to be applied to the brilliancy or magnitude of the Planet or Star, if it were very superior, would I think tire the patience of any true Observer. And also it does appear to me... that limiting the emergent pencil, is equal to limiting the aperture of the Object-Glass of the Telescope, either of these methods having the effect of reducing the light that should otherwise enter the eye, and therefore, reducing in appearance those circles or rings of aberration which surround a Star when seen through a good Telescope...
> I therefore conclude that Dr.Reade must in some way have deceived himself.[60]

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[59] J.B.Reade, "On the Construction and Use of Single Achromatic Eye-pieces, and their superiority to the Double Eye-piece of Huyghens", *Proceedings of the Royal Society of London,* 1840, p.195.

[60] G.Dollond, "Report on Paper by J.B.Reade", Referee's Reports 1.192, Royal Society Archive.

Dollond's report is worth quoting at some length not because of any intrinsic interest for this discussion of the contents of Reade's paper, but because of the revealing *nature* of the discussion. He is concerned with the practical inadequacies of Reade's argument. As such the validity of the theory behind the paper is not addressed. This contrasts with the report of the other referee on the paper, Astronomer Royal George Airy, whose discussion was centred not only on practical problems but on theoretical ones as well.[61] Airy's opinion of the paper was that it contained too much vague discussion, and not enough hard theory. Thus it was a proper paper for a philosophical journal, but not for the *Philosophical Transactions.* Dollond, on the other hand, was concerned that the paper was not well written, and with the practical faults in Reade's argument. The gulf between the nature of the two reports is indicative of the gulf between the ideologies of the two men - Dollond, wishing to underline his position as an expert on practical optical problems by criticism of the work of another supposed optical expert, and Airy, wishing to establish what *real* scientific research was, and the sort of paper one was expected to produce if one was to be considered worthy of membership of the elite group of philosophers of which he was one representative.

This notion of what exactly *scientific* work was, as defined by actors in the period, will be a recurring theme in this thesis. While Dollond may have viewed his expertise in optical matters as a guarantee of full participation in Royal Society

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[61] G.B.Airy, "Report on Paper by J.B.Reade", Referee's Reports 1.191, Royal Society Archive.

matters, his discourse would not have been regarded by men of science as that of a philosopher, and as such he was *excluded* from full membership of the scientific elite, being regarded as pre-eminent in instrument making practice but not as a philosopher. Nonetheless, Dollond's status is not unproblematic for this argument, as his admission to the Royal Society Club shows he was valued as a social contributor to Royal Society business, an unusual accolade for an instrument maker. This may partly be explained by reference to the prevalence of practical committees on which he served, thus making him a visible Fellow, and partly by his social status as Optician to the King. The later makers who became Fellows took no such part in the Society's affairs, and therefore fit in more fully with the characterisation of instrument maker as *artisan* versus man of science as *philosopher* expressed in this thesis.

5. Thomas Jones: The Instrument Maker as Artisan.

Thomas Jones, born in 1775, had, as already mentioned, learnt his trade under Jesse Ramsden, starting work as an instrument maker aged 14.[62] In 1811 he obtained a patent for a dividing instrument, the sectograph;[63] he published a description of it in 1814,[64] and a work on the mountain barometer in 1817.[65]

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[62] "Thomas Jones", in *Dictionary of National Biography.* See also Taylor, *op.cit.* (note 16), pp.342-3.

[63] *Ibid.,* p.342.

[64] T.Jones, *Description and Use of "The Sectograph",* (London, 1814).

[65] T.Jones, *A Companion to the Mountain Barometer,* (London, 1817).

While specialising in the whole range of optical, mathematical, and philosophical instruments, Jones was perhaps best known in scientific circles for his work as a rival to Troughton in the construction of large observatory instruments. Although rivals in business, the two makers were personal friends; for example one contemporary, in discussing the mural circles, one by each maker, at Greenwich, observed that the circles "seem to regard each other as antagonists, yet there is the same cordiality between them, as there has subsisted between their respective makers for many years".[66]

Jones was already a firmly established maker by the time he made his only contribution to the *Philosophical Transactions,* in 1826.[67] This short paper was communicated to the Society by Captain Henry Kater, for whom Jones had been working for some years, particularly as maker of "Kater's pendulums" under instructions from their inventor.[68] The subject of Jones' contribution was an improved hygrometer which he had developed. It is significant, however, that at the end of the paper, after a fairly full description of the practical working of the instrument, and the principles upon which it was based, Jones saw fit to add:

> I ought also perhaps to mention that an instrument somewhat similar in principle has been used in Vienna, and was mentioned by Professor Baumgarten of that capital to a friend, who communicated the fact to myself.[69]

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[66] W.Pearson, *An Introduction to Practical Astronomy,* (London, 1829), pp.554-8.

[67] Jones, *op.cit.* (note 17).
[68] Taylor, *op.cit.* (note 16), p.342.
[69] Jones, *op.cit.* (note 17), p.54.

In other words, the body of the paper gave the impression that Jones had contrived a totally new instrument, whereas in fact he was forced to acknowledge that he had merely made a practical development of an existing instrument.

It is not clear whether the publication of this paper made any difference to Jones' position within the scientific community, or to his business. Certainly, no Copley Medal or Fellowship followed it as had been the case with Troughton. However, Jones remained active as an instrument maker, and was eventually elected a Fellow of the Royal Society in 1835, at the age of 60, the reward for a lifetime's devotion to instrument making and service to the scientific community. Such an accolade would have been more significant *socially* than as an aid to his business at Jones' age, though I would maintain that for those makers such as Dollond and Troughton who were elected rather earlier in their careers, a Fellowship of the Royal Society was an invaluable title in attracting potential clients, in particular from abroad.

It seems from extant correspondence that Jones was not merely a Fellow of the Royal Society by name, but that he took some part in its affairs by lending instruments and by attending meetings. For example, he wrote to Roget, the Secretary, in 1844, about an instrument in the Society's possession: "I have sent a great number of times for an upright barometer belonging to me, it has a thermometer in front and a floating gauge - have the goodness to let the bearer have it".[70] Clearly the

[70]T.Jones to P.M.Roget, 7 March 1844, Miscellaneous Correspondence 4.11, Royal Society Archive.

gentlemanly courtesy of returning to him what was his carried some weight with Jones, and his own gentlemanliness can be illustrated by his withdrawal from the Royal Society when he felt he was no longer able to make any useful contribution to its affairs: "In consequence of my advanced age + inability to attend the meetings, I beg leave to state that I should like to withdraw my name from the Society".[71]

Jones died the following year, 1852, the same year in which William Simms was elected, having just completed the construction of George Airy's transit circle at the Royal Observatory, Greenwich. I shall discuss Simms at some length in the chapter on Airy and the Observatory later in this thesis, and at this point will only note that after Jones' and Dollond's deaths he was the only instrument maker alive with the title F.R.S.

The fifth nineteenth-century maker to be a Fellow, other than Simms, Jones, Dollond, and Troughton, is slightly peripheral to the discussion here, firstly because he was a chronometer maker, not a maker of optical, mathematical, and philosophical instruments, and secondly because he seems to have taken very little part in the affairs of the Society. Born in 1778, William James Frodsham was the third generation of a family of eminent clockmakers, and, along with Edward Dent, was one of the best known horologists of the nineteenth century.[72] His only

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[71] T.Jones to the Secretary of the Royal Society, 29 November 1851, Miscellaneous Correspondence 5.27, Royal Society Archive.

[72] Taylor, *op.cit.* (note 16), p.338. Considerable detail is provided on W.J.Frodsham, and his predecessors and successors, in V.Mercer, *The Frodshams,* (London, 1981).

contribution to Royal Society meetings was a paper read in 1838, the year before his election, on a development in the pendulum - a paper very much in a practical vein, and which did not gain publication in the *Philosophical Transactions,* though an abstract of it did appear in the *Proceedings of the Royal Society.*[73] On Frodsham's death in 1850, as has been indicated, Jones and Dollond were the only living makers to be Fellows of the Royal Society, and they too soon died, leaving only William Simms.

I have discussed the roles which these five Fellows, Troughton, Dollond, Jones, Frodsham, and Simms, played in the affairs of the Royal Society, as an illustration of the different way the instrument maker entered the life of the Society in the nineteenth century compared to the eighteenth century. Where an eighteenth-century maker such as Short, Nairne, or Ramsden featured in the Society and the scientific community it represented not only as a constructor of scientific instruments, but also as a researcher in science and a designer of entirely new instruments, the nineteenth-century maker who had the good fortune to be elected a Fellow brought to the Society only his practical skill and expertise, not his skill in designing grand new scientific tools, nor an ability to contribute to burgeoning areas of scientific research. As such, the role the maker was able to achieve in the Society was bound to be a lesser one than before, and the chief benefit which Fellowship could confer on a

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[73]W.J.Frodsham, "Experiments on the Vibration of the Pendulum", *Proceedings of the Royal Society of London,* 1838, p.78. See also Mercer, *op.cit.* (note 72), pp.37ff.

maker was in terms of an advertisement for his business - the three magical letters could be of great value in attracting potential customers.

However, a Fellowship of the Royal Society did not guarantee for a maker full membership of the elite scientific community. The nature of the accomplishments necessary to attain such status was effectively defined by a group of actors for whom the skills of the instrument makers were very much secondary, practical, manual skills, and not primary, theoretical, intellectual skills. This group was able to use the Royal Society, and others such as instrument makers, in order to further their own careers and enhance their social status, monetary concerns being secondary or non-existent to them, by effective prosecution of research projects, and by being seen to be in positions of power within a recognised intellectual environment. Research topics such as those investigated by the Pendulum Committee demonstrate the means by which the group of philosophers who sought power and status in society were able to establish their claims to intellectual authority through the medium of the Royal Society.

6. The Work of Committees: The Role of the Philosopher and the Role of the Artisan.

As we have seen, the Pendulum Committee was formed in response to a request the Prince Regent received in Parliament for an exact determination of the length of a seconds pendulum at the Royal Observatory, and at other stations of the trigonometrical survey. Such a determined length, it was hoped, would form a basis for a standard measure. The Council appointed a committee to study the problem which included Troughton, Pond, Davies Gilbert, Kater, and others.[74] Kater in particular showed great interest in pendulum work, and was rewarded with the Copley Medal in 1817.[75] Work on the pendulum was to continue well into the 1820s with the Royal Society as a focus.

Miller lays great emphasis on the work in areas such as pendulum experiments as a focus of ideas, aspirations, and expertise, in metropolitan science in the period.[76] Three of the components of Miller's reforming alliance had interests in this area - the highly trained Cambridge mathematicians, the scientific servicemen, and the mathematical practitioners, and he sees each as having provided skills in different aspects of the project. The mathematical practitioner was expected to be skilled in the design and use of pendulum apparatus, and could thus improve its accuracy and usefulness, whereas the sophisticated mathematician could test the law-like behaviour of the empirical findings of the scientific serviceman against the mathematical theory of the figure of the earth. In this way Miller sees the pendulum programme as having been an important area of co-operation of these three sub-groups within metropolitan science,[77] and as a means of emphasising their expertise and their claims to power within the scientific community and in society as a whole.

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[74] Weld, *op.cit.* (note 29), p.253.

[75] *Ibid.,* pp.261-3. See also Hall, *op.cit.* (note 2), p.14. Kater's work is summarised in H.Kater, "An Account of Experiments for determining the length of the pendulum vibrating seconds in the latitude of London", *Philosophical Transactions of the Royal Society of London,* 1818, pp.33-102.

[76] Miller, *op.cit.* (note 1), pp.176-97.

[77] *Ibid.*

While of value as an analysis of the motives of those actors who were firmly within one of his historical sub-groups, Miller's argument suffers from a rather loose characterisation of his category of *mathematical practitioner.* Whereas earlier in his dissertation he used the term to include the instrument maker, his later use of the term clearly refers to individuals such as Kater, whose focus of activity was the design and use of instruments, not their construction, and the role of the instrument maker in the pendulum programme is ignored. I wish to argue that the instrument maker's role which Miller ignores was a real one, and that the programme represented a real source of revenue for makers, and an opportunity to display their practical skills. Several makers' bills survive from the period 1816-18, when the pendulum committee was active, which give an indication of the income to be derived from pendulum work. Thomas Jones and George Dollond seem to have been the most popular makers,[78] though William Cary, like Jones a former pupil of Ramsden, was also employed to some extent.[79] Jones supplied a brass experimental pendulum to order at £68.18.6.,[80] which was a considerable sum in 1818, so pendulum work would have been welcomed as lucrative by the instrument makers. However, the role which the instrument maker was able to play in the programme of pendulum investigations was very much that of a manual workman, subordinate to the work of the mathematical practitioners and the

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[78] Pendulum Committee, Instrument Makers' Bills, Domestic Manuscripts 3.125ff., Royal Society Archive.

[79] *Ibid.*, 3.127, Bill from W.Cary.
[80] *Ibid.*, 3.135, Bill from T.Jones.

sophisticated mathematicians involved in the enterprise.

Both practitioners and mathematicians would have been in accord on the skilled role to be played by the maker, whilst also maintaining that it was a less vital role than their own. In addition, the mathematicians would have regarded their contribution as the central one, with that of the practitioners a subordinate one as well. It was this group of mathematical philosophers who were particularly keen to express their claims to intellectual authority and status, men such as William Whewell and George Airy, whose ideology will be further discussed later in this thesis. At this stage, it is only necessary to note that the pronouncements of these men in their quest for intellectual authority and power implicitly excluded from authority other groups without expertise in the fields of study they pursued, that is, in non-mathematical, non-theoretical areas, and this meant groups such as instrument makers whose expertise was *practical*.

Philosophers, however, could acknowledge the work of the maker not only as important in practical terms, but as of enduring value. John Herschel's *Discourse* included his views on the importance to science of standard scales, and of course, implicitly, of those who constructed them:

> It is not enough to possess a standard of an abstract kind; a real material measure must be constructed, and exact copies of it taken. This, however, is not very difficult; the great difficulty is to preserve it unaltered from age to age; for unless we transmit to posterity the units

> of our measurements, such as we have ourselves used them, we, in fact, only half bequeath to them our observations.[81]

So whatever their own motives in emphasising intellectual authority, the philosophers had to admit an important role for the instrument maker in scientific activity. This was not the same thing as admitting that the maker was a full member of the scientific community, or that his work was of the same value as theirs. That the philosopher's labour was in some sense superior to that of all other groups was a central part of their own rhetoric in seeking to establish their status. Therefore, even if in some situations the instrument maker seemed to be an equal partner in the scientific enterprise, as with Dollond working alongside Faraday and Herschel in the Optical Glass Committee, the rhetoric of the philosophers generally emphasised *their* leading role in any such enterprise.

The Meteorology Committee of the Royal Society brought philosophers into contact with instrument makers in a similar way to that which had been the case with the Pendulum Committee some years before. Meteorology and its instruments - thermometers, barometers, hygrometers, and so on, had certainly been a major concern of the Society since its earliest days. In 1773 it had been resolved that meteorological observations with thermometer, barometer, hygrometer, and rain and wind gauges should be made twice daily at the Society's apartments. The results of these observations were published regularly in the *Philosophical*

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[81] J.F.W.Herschel, *A Preliminary Discourse on the Study of Natural Philosophy,* (London, 1830), p.128.

Transactions up to 1843.[82] In 1822 a committee was appointed to examine the instruments used. It was at the recommendation of the Meteorological Committee that the famous water barometer was erected at the Society's premises. J.F.Daniell seems to have been the motive force behind this recommendation; he had believed that an accurately constructed water barometer could be expected to shed light upon several important points of physical theory. He realised that the only way to achieve his aim was to construct the whole tube of a single piece of glass, *and to boil the water* in it, as with the mercury in the usual design of barometer.[83]

Daniell contrived a plan to overcome the great practical difficulty of being able to form a uniform tube of sufficient length. Weld's *History of the Royal Society* gives an account of the method of constructing the barometer, but is notable, and indeed typical of contemporary literature, for giving little attention to the maker compared to the attention given to the philosopher who developed the process. John Newman, perhaps the leading meteorological instrument maker of the day, is only mentioned in a footnote as "Mr.Newman, who overcame the difficulties of the various processes with the greatest skill".[84] Thus his role in the production of the water barometer is treated as very much subordinate to that of Daniell, presumably because any skilled instrument maker could have played his part, whereas Daniell's philosophical expertise was seen by Weld not only as more central to the realisation of the design, but also as a

[82] On Meteorology, see Hall, *op.cit.* (note 2), pp.155-7.
[83] On the Water Barometer, see Weld, *op.cit.* (note 29), pp.452-6.

[84] *Ibid.*, p.454.

rather more rare commodity.

Newman's profit from his role in this project was in terms of increased prestige in the scientific community, and, consequently, increased prestige and revenue for his business. Thus, being able to say that he was "Maker of the Water-Barometer in the Royal Society's Apartments"[85] served much the same purpose from a business point of view as being able to say that one was a Fellow of the Royal Society - it enhanced one's reputation. Newman, however, in common with a number of elite makers in the period, merely constructed instruments for the Society, and did not provide it with any research papers (his name will be a prominent one in later chapters of this thesis, particularly in the chapter on Faraday and the Royal Institution, for whom he was the official maker). The only makers not to become Fellows who did have a paper read at a Royal Society meeting were Francis Watkins, as mentioned earlier, and Thomas Robinson, who contributed a short paper on the mountain barometer in 1831.[86]

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[85] Newman usually described himself as "Philosophical Instrument Maker to the Royal Institution of Great Britain" on his bill headings - for example, see J.Newman to G.B.Airy, 1841, Royal Greenwich Observatory, Airy Papers, Vol.718 f.24.

[86] T.C.Robinson, "Description of a Mountain Barometer", Archived Papers 15.10, Royal Society Archive. This paper appeared in condensed form as T.C.Robinson, "Description of a Mountain Barometer", *Proceedings of the Royal Society of London,* 1831, p.40.

7. The Instrument Maker as a Subordinate Participant in the Scientific Enterprise.

What I wish to emphasise about makers such as Newman and Robinson who did work for the Royal Society was that they were very much *employees* of the ruling groups in the scientific community. It was expected of them to deliver their work to the highest standards, but it was also important for them to be seen to be under the control of the scientific elite. Thus Newman, in constructing the water barometer, was controlled by Daniell, and Jones, for example, was under the control of Kater and the other members of the Pendulum Committee, in constructing the pendulum apparatus. This aspect of *control* could be discerned in the attitude of the scientific elite towards makers even in areas such as microscopy, where it was *expected* of any top maker that he would respond to an advertisement from the Royal Society offering a reward for the best microscope. From the maker's point of view, the Royal Society's demands seemed to ignore the fact that he had a business to maintain; as Andrew Ross wrote following one such advertisement in 1843:

> As I am unwilling it should be supposed that I disregarded the advertisement published by the Royal Society offering a premium for the best Microscope I beg to inform the Gentlemen to whom this subject has been committed that during the interval between the advertisement and the present time I have been fully engaged upon a Microscope for the London Microscopical Society of which I may state a highly satisfactory report has been made and as an instrument for the Royal Society must occupy the maker's whole attention I have been prevented from complying with the conditions of the advertisement - under other circumstances had time and opportunity been afforded me, I should have been happy to employ my best exertions

> to furnish an instrument which should have been an acquisition to the Royal Society and a credit to myself.[87]

Despite the slight resentment implied by Ross of the Royal Society's attempt at control, the letter indicates the important feature of work for the Society - it became a concrete testimony to the ability of a maker.

The list of instruments in the Society's possession compiled by George Dollond in 1834 shows the wide range of makers who had been able to profit in direct financial terms and reputation by their work for the Society.[88] Of a total of 82 items in the list of Society property at this time, John Newman had made 7, George Dollond himself 4, the firm of Troughton and Simms 4, and Thomas Jones 4. In addition, William Cary, who constructed some apparatus for the Pendulum Committee, appeared 3 times as maker in the list. Of the eighteenth-century makers represented, John Bird was the most frequent, with 6 items. Ramsden, Short, and Nairne were also present. Of course the list would have been much more extensive if Dollond had been able to take into account instruments made initially for the Royal Society, but which were in private hands in 1834. For example he had no knowledge of the whereabouts even of instruments he himself had constructed for the Society, such as a repeating circle which was at one time held by Captain Kater.[89]

The extent of the market for instruments provided by the

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[87] A.Ross to the Secretary of the Royal Society, 20 May 1843, Miscellaneous Correspondence 3.283, Royal Society Archive.

[88] *Instruments and Apparatus, op.cit.* (note 55).

[89] G.Dollond to J.W.Lubbock, 3 July 1835, Lubbock Papers D202, Royal Society Archive.

Royal Society, then, was a considerable one in the nineteenth century, rather larger than might have been implied by the list of instruments stored in the Society's apartments. As I have attempted to show, the role of the maker in the Society was predominantly as a constructor of instruments subject to the control of the elite philosophers. Except in a few isolated cases, the maker was unable to participate any further in the Society's affairs.

It would be misleading, however, to think that the constitution of the Royal Society precluded instrument makers from attaining a Fellowship, and that this might account for the dearth of Fellows in the century and particularly for their complete absence (unless we include the Irishmen, Thomas and Howard Grubb) from 1852. In fact, the opposite was the case, for a clause included in the directions for members proposing a candidate for election was that this could be done if he was "one who has invented or materially improved any astronomical, mathematical, or philosophical instrument, or chemical process, which should be specified".[90] William Simms was the only English maker ever to fulfill this clause. No other man who made his living by construction of instruments (again with the exception of the Grubbs) subsequently became a Fellow, though a large number of examples might be expected of philosophers with interests in instrument design who gained election because

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[90] For details of the directions given to Fellows proposing candidates, see M.P.Crosland, "Explicit Qualifications as a Criterion for Membership of the Royal Society: A Historical Review", *Notes and Records of the Royal Society of London,* 1983 (37), pp.167-87, on p.180.

of their success in that area.

One maker did believe that he had the necessary credentials for election, and that he fulfilled the conditions of the above clause, and proceeded to offer himself for a Fellowship. Louis Casella was the maker, one of the leading thermometer makers in London, and particularly acclaimed for his work for the British Association.[91] Casella wrote to the President and Council in 1878:

> Having with the Co-operation of a Committee of the Royal Society produced an arrangement by means of which the Temperature of the Sea has now been measured; Having also, after many years of continuous effort succeeded in perfecting the clinical thermometer - now of such general application in medical practice, as well as in the treatment of Disease of Cattle, I venture, respectfully to submit to your [illeg.] body whether the services thus rendered may be considered of such public utility as to justify me in applying as I now do for the distinguished honour of being elected a Fellow of your Society. It would of course ill become me to attempt to assess the value of my own discoveries, such as they are, they are doubtless well known to many members of your Society... had I adopted the course of patenting the Clinical Thermometer, its more complete identification with my name would have rendered it unnecessary ever to add that my object in designing it was [to aid] the Medical Profession... Should you think fit to confer upon me the honour I so much desire, it would be valued as the highest acknowledgement of, and most esteemed reward for, the many years of toil and anxiety spent in perfecting the Scientific Instruments referred to.[92]

[91] *B.A.A.S. Report, Glasgow, 1855,* pp.xxxviii-xxxix.
[92] L.P.Casella to the President and Council of the Royal Society, 10 January 1878, Miscellaneous Correspondence 11.148, Royal Society Archive.

Casella's solicitation is reminiscent of that of Benjamin Martin more than a century before.[93] Neither man was ever to achieve the accolade they so much desired, perhaps because their letters implied some commercial motive behind their quest for Fellowship, even if Casella's rhetoric seemed very much to disguise any such motive. Certainly Casella used the letters F.R.A.S. and F.R.G.S. as an aid to his business, but he was never to add F.R.S. to these. Subject to the control of a scientific elite, the instrument maker could no longer aspire to full membership of their community.

The role of the instrument maker in the scientific community has been treated in this chapter without explicit reference to chronology. My justification for ignoring exact references to the nature of the regime in power, when discussing specific examples of the treatment by philosophers of instrument makers, is that the removal of the instrument maker from a stage upon which he could demonstrate his skill in instrument design and scientific research, was as apparent a phenomenon in the Banksian regime as it was later in the century.

During the reign of Banks the contributory factors in this loss of status involved a general dissatisfaction of those in power in the Banksian Learned Empire with the mathematical practitioners of London, of whom the instrument makers were a component. As Banks' reign drew to a close, the mathematical practitioners were in a position to articulate their claims to

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[93] J.R.Millburn, "Benjamin Martin and the Royal Society", *Notes and Records of the Royal Society of London,* 1973 (28), pp.15-23.

intellectual status on the basis of their scientific work, but the makers, having performed predominantly manual, practical work, were unable to pursue similar claims.

The period of Humphry Davy's Presidency, and of that of Davies Gilbert, were characterised by an increasing dissatisfaction with the perceived state of the Royal Society, and the conditions one seemed to need to fulfill in order to achieve a Fellowship, dissatisfaction expressed in particularly devastating form by Charles Babbage in his *Reflections on the Decline of Science in England.*[94] However, while Davy may not be seen as a member of the "reform alliance", his own views on the position in society which should be accorded to men of science can be seen to bear a remarkable similarity to the rhetoric employed by the leading spokesmen of the alliance seeking to displace one whom they perceived as an extension of the Banksian regime. Some of Davy's lectures, for example, included such components of the "reformist" discourse as the emphasis on the usefulness to the State of philosophical knowledge and its cultivators:

> The progression of physical science is much more connected with your prosperity than is usually imagined. You owe to experimental philosophy some of the most important and peculiar of your advantages. It is not by foreign conquests that you are become great, but by a conquest of nature in your own country. It is not so much by colonization that you have attained your pre-eminence or wealth, but by the cultivation of the riches of your own soil... There is no country which ought so much to glory in the progress of science, which is so much interested in its success, as this happy island. Science has been a

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[94] C.Babbage, *Reflections on the Decline of Science in England, and on Some of its Causes,* (London, 1830), pp.50-166.

> prime cause of creating for us the inexhaustible wealth of manufactures, and it is by science that it must be preserved and extended...[95]

In the early years of his Presidency, in fact, Davy promoted a number of measures which were important to those seeking reform, notably steps to reduce elections and so improve the quality of new Fellows.[96] In addition, by the appointments made to the Council, it was apparent that he wished to bring the potentially troublesome reformers within the power base of the Society, as a form of appeasement - Babbage, Herschel, and South were all first appointed at this time.[97] Committee work also increased under Davy, and instead of the committees being composed of members of social standing of assumed general omniscience, consideration was given to the experts in the subject particularly under investigation, as we have seen for example with the membership of the Optical Glass Committee. Miller shows at some length that such changes were not isolated during the early years of Davy's Presidency, and that the reformers saw many of their aspirations being fulfilled. However, he goes on to show that Davy's attitudes changed in the later years of his tenure,[98] in an attempt to satisfy the members of the Banksian Learned Empire who had been dissatisfied with the extent of the reforms Davy introduced - after all it was aristocratic patronage which had made Davy's own career possible.

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[95] Extracts from Davy's Lectures, in J.Davy (ed.), *The Collected Works of Sir Humphry Davy,* (9 volumes, London, 1840), Volume 8, pp.358-9.

[96] Miller, *op.cit.* (note 1), pp.243-88 discusses the Royal Society under Humphry Davy.

[97] *Ibid.,* p.258.
[98] *Ibid.,* pp.266-74.

Essentially Davy's problem was that he could not pursue measures sufficient to satisfy either the reform alliance or the survivors of the Banksian Learned Empire, and ended up unpopular with both for doing either far too little or far too much. The point I wish to stress is that Davy's Presidency shows us that in his initial policies he exhibited his belief in the exalted position that men of science ought to hold in society. Thus, while in his later policies he may be regarded as little more than a continuation of the Banksian era, in his early rhetoric it can be seen that the construction of a scientific elite was not a motive unique to the reform alliance who were eventually to put John Herschel up for the Presidency in 1830. The notion of a collective identity for real men of science, real workers in science, and not merely those with a vague interest in it, cannot simply be ascribed to the members of the reform alliance, for it was certainly becoming apparent in the attitudes of philosophers outside that reform alliance in the 1820s. Admittedly the full articulation of such claims to elite status had to wait until some time into the reign of the Duke of Sussex as President, with real men of science dominating the Council, and until some time after that a member of the peerage was favoured as President of the Royal Society.[99] However, the claims of those aspiring to an elite position in society were heard long before the 1830 election, and from voices such as Davy, not explicitly reformist

[99] The Duke of Sussex was succeeded in 1838 by the Marquis of Northampton, who was in turn succeeded by the Earl of Rosse, in 1848, and by Lord Wrottesley in 1854. It was 1858 before the Royal Society again had a President who was not a member of the peerage, Sir Benjamin Collins Brodie.

in their stance.

The assertion of a national importance for the work of men of science, and a consequent assumption of their collective identity as an elite group in society, can be seen as having consequences for groups which were not identified with the men of science. Those who were key members of the Banksian Learned Empire, but who did not do scientific work necessarily lost status in the new community, particularly after 1830. So, I wish to argue, did the instrument makers. In the Banksian era they had lost status in the scientific community as a result of a general antipathy to the London community of mathematical practitioners, and had lost their status in *that* community due to a gradual loss of interest in research, or ability to develop new instruments, and an increasing devotion to the manual, business side of their trade. In subsequent eras, defined by the Presidencies of Davy, Gilbert, and Sussex, the claims to elite status advanced by men of science, whether explicitly reformist or not, implicitly relegated the instrument maker to the position of a manual worker necessary to the furtherment of the scientific enterprise, though without any key role in that enterprise. In short, the maker was peripheral to the interests of the elites who were in control, or who effectively expressed claims to control, throughout the first half of the nineteenth century, although the natures of these elites themselves were very different.

As instrument makers were not seen as making any contribution to the accumulation of scientific truths and the advancement of science, they were effectively precluded from election to a body which honoured success in such pursuits. However, it is important to note that not all those who became Fellows could claim to be full members of the scientific community. A number of practical engineers gained admission - such as both the Brunels, and so did a number of industrial chemists.[100] The important point about these men, though they did not all participate in the accumulation of scientific truths, was that they were seen as performing an important social role - while not members of the scientific elite they did *apply* the work of that group in order to produce wealth for the nation. As such, this was an important justification for the work of the scientific elite itself, and these two groups, the men of science and the engineers, established claims to intellectual authority by means of their participation in the processes leading to the production of national wealth, with Fellowship of the Royal Society coming, later in the century, to be seen as the institutional reward for this participation.

During this period, the instrument maker fell behind in his competence to design new instruments, owing to their mathematical complexity, and he was forced, in post-Industrial Revolution Britain, to devote his time not to design, and scientific pursuits, but to increasing the efficiency of his business.[101] In addition, he was unable to claim membership of the scientific elite who uncovered new scientific principles whose application produced wealth for the State, nor of that group of practical engineers more directly responsible for the application itself.

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[100]Hall, *op.cit.* (note 2), p.144.
[101]This idea will be covered in more detail in Chapter 8.

As such, the instrument maker became strictly a manual worker, with the intellectual labour of instrument design being predominantly left to others. Full membership of the scientific community for the maker in the nineteenth century was unattainable, whereas in the eighteenth century it was achievable. Some reasons for the change have been developed in this chapter. In the next few chapters I propose to expand these themes by explicit reference to particular members of the new scientific elite which has been referred to in this discussion of the Royal Society.

CHAPTER THREE

CHARLES WHEATSTONE

The first man of science whose relationship with instrument makers I would like to consider in detail is Charles Wheatstone. My aim in this case study is to portray Wheatstone as an individual who attained membership of the philosophical elite from humbler origins, namely a background in a manual trade. By studying Wheatstone's place in the scientific community, I hope to show how membership of this community was valued for its social status by those belonging to it. In turn I hope to show that the idea of such status which Wheatstone believed his scientific work conferred upon him meant that he regarded himself as performing a more crucial role in society than did groups such as scientific instrument makers. This is a theme which will be developed more fully in the succeeding chapters. In this chapter I simply wish to show how an individual could be motivated by a desire to achieve status in nineteenth-century society, and how this manifested itself in his treatment of other individuals.

Existing scholarship on Wheatstone is rich in factual information, but fails to place him convincingly in a contextual historical framework.[1] By characterising the scientific elite and his motivations and place within it, I hope to redress this balance. Wheatstone is not the most famous man of science of the early nineteenth century, but the nature of his rise to scientific eminence, and the subsequent consolidation of his status act as justifications for his inclusion as an individual case study in this thesis. Also, unlike Babbage and Airy,

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[1]The only full-length biography of Wheatstone is B.Bowers, *Sir Charles Wheatstone FRS, 1802-1875*, (London, 1975). Prior to this the most complete factual acoount was in W.T.Jeans, *Lives of the Electricians*, (London, 1887), pp.105-230.

Wheatstone's is a famous name to the layman, from physics textbooks. There is a tendency in such textbooks to associate men of science with one particular discovery or achievement. This is unfortunate for two reasons. Firstly, it implies that the "achievement" in question represented some sort of climax or culmination of the subject's work. Secondly, it ignores other work which the subject may have done. Such a tendency exists with Charles Wheatstone and the Wheatstone Bridge. Few remember that Wheatstone invented the first practical electric telegraph. Even fewer remember that he invented the concertina. Many remember that Charles Wheatstone gave his name to the Wheatstone Bridge.

The bridge circuit for comparison of electrical resistances appeared in Wheatstone's 1843 Bakerian Lecture "An Account of Several New Instruments and Processes for Determining the Constants of a Voltaic Circuit", which was published in the same year.[2] Wheatstone called it his "differential resistance measurer". Ironically, the arrangement which was to become known as the Wheatstone Bridge was not new, and Wheatstone acknowledged as much, giving the credit to Samuel Hunter Christie.[3] However, Wheatstone was responsible for the development of the bridge circuit in that he showed how it could be used as a method of comparing resistances (rather than for only comparing electromotive forces, which was what Christie had shown), and also in

[2] C.Wheatstone, "An Account of Several New Instruments and Processes for Determining the Constants of a Voltaic Circuit", *Philosophical Transactions of the Royal Society of London*, 1843, pp.303-27.

[3] *Ibid.*

that he put the arrangement on to a circuit board.[4]

The differential resistance measurer was thus an *instrument* which could be used for probing nature, and knowledge acquired by its use could be of value to mankind. In this sense only is the Wheatstone Bridge typical of Charles Wheatstone. The prime purpose for which he laboured was not a desire to obtain financial reward, in the form of patents, nor was it mere curiosity. Wheatstone wanted to make use of the sources of power in nature for the benefit of mankind. Instruments and apparatus to achieve this were therefore of fundamental concern to him. In his detachment from financial motivations, his desire to better the condition of mankind, and in his concern with practical instruments and apparatus, he shared many of the elements of the ideology of the scientific community in the early nineteenth century.

1. The Early Life of the Philosopher.

Charles Wheatstone was born in 1802, the son of a Gloucester musical instrument manufacturer.[5] This had been the family business for over 50 years, and his father William must have regarded a move to set up the business in London as commercially advisable, as the family moved there in 1806, when Charles, the second child, was only 4 years old.[6] It is reasonable to assume that his father's work would have influenced Charles at an early age, and that he would have picked up some basic practical skills. In 1816 his father arranged for him to go and work for

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[4] Bowers, *op.cit.* (note 1), p.98.
[5] *Ibid.*, p.4.
[6] *Ibid.*

his uncle at 436 Strand.[7] His uncle (also called Charles Wheatstone) had been working as a musical instrument maker since about 1805 in London.[8] Though the young Charles would already have acquired sufficient practical skills to construct musical instruments, at no stage was he keen to follow his father and uncle into the musical business. The practical skills which he acquired during these formative years were to remain with him throughout his life and were to be valuable when he turned to a career in science, where development of new and useful instruments was to be an important concern. At this stage, however, Wheatstone fitted very much into the category of *artisan*, or manual workman. We should not assume that his passage to membership of the scientific community was apparent at this point in his career.

Wheatstone's interest in the manufacture of musical instruments was recognised by his uncle as being rather less than his desire to pore over books, and his father took him away from the uncle's business after a few months. By this time it was apparent that Wheatstone, who had been well educated[9] and was described as "excellent"[10] in mathematics and physics at school, was paying more attention to the *causes* of musical sounds and the principles underlying the mode of their propagation than to the manufacture of the instruments which produced them. He was still able to be of use to the family

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[7] *Ibid.*, p.5.

[8] *Ibid.*, p.4.

[9] He was educated at several schools, notably in Vere Street, London, from 1813. *Ibid.*, p.5.

[10] *Ibid.*

business, however, as his enquiries into the principles of the science of sound bore as fruit several new instruments, the first being the keyed *flute harmonique* in 1818.[11] This provided one of the first indications that Wheatstone's motivations were to give *philosophical* explanations of phenomena, and thus that he may have aspired to go beyond his artisan status and be seen as a *philosopher.* Isolated investigations could not confer such status, however; this had to be earned by more sustained philosophical work.

The first experiments which Wheatstone performed, as a teenager, on the transmission of sound arose from a consideration of the distance between the strings and sound boards of instruments. His aim was to find how great the distance could be without having an adverse effect on the sound produced. He stretched a string on a steel bow and connected the bow to the sound board of a piano through a glass rod (nearly 2 metres long). The sound produced was just as good as when the string was touching the board.[12] This type of investigation, one which Wheatstone repeated much later in his career, at the Royal Institution, represented considerable practical sophistication for a teenager.

These experiments were the basis for a number of public demonstrations, the most notable being the demonstration, in his father's music shop in Pall Mall, of Wheatstone's Acoucryptophone, or Enchanted Lyre. This consisted of a hollow box shaped like an antique lyre. The box was hung from a wire

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[11] *Ibid.*, p.7.
[12] *Ibid.*

passing through the ceiling of the room and hanging upon the sounding board of, usually, a piano, though in principle any other musical instrument, in an upper storey. When the musician struck this instrument, the vibrations passed down the wire and were heard by the audience as if they had emanated from the lyre. It was easy to perform this experiment with stringed instruments, but Wheatstone could not accomplish the demonstration with wind instruments where the vibrating body was a column of air. However, the demonstration with other than wind instruments was a success, much of the reason for this being the sense of mystery which it caused in the audience.[13]

Demonstrations such as this were to bring Charles Wheatstone to the notice of the scientific establishment. However, Wheatstone did not wish to prolong the sense of mystery which the demonstration caused in the audience. He believed that his work was more beneficial if the principles upon which it was based could be made widely known. Not desiring to prolong the mystery of the Acoucryptophone merely to reap financial rewards, he published an account of the experiments by which he was led to invent the apparatus.[14] Such behaviour was fully in keeping with the gentlemanly ethos of the scientific world, which in general repudiated the idea of monetary reward for its work, so we can see that Wheatstone had now some claim to membership of the scientific community, at least in that his behaviour was

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[13] *Ibid.*, pp.7-8.

[14] C.Wheatstone, "New Experiments on Sound", *Annals of Philosophy*, 1823 (6), pp.81-90.

consistent with the ideology of that community.[15]

It was in the same year as his first publication, 1823, that his uncle Charles died.[16] Wheatstone returned to the Strand, where he had been so briefly employed seven years previously, and took up in business as a musical instrument manufacturer with his brother, William Dolman Wheatstone. In 1826 they joined forces with their father's business.[17] Wheatstone's connection with the business did not cease around this time, though it may be thought that with his more philosophical studies of sound, he had less time to devote to musical instrument manufacture. His connection with the business certainly lasted until 1846, because there is correspondence of Wheatstone's up to that date which bears the address of the shop, 20 Conduit St.(where the business moved in 1829).[18]

It seems certain that Wheatstone himself did not devote much time to the manufacture of musical instruments. Wheatstone and Co. had some staff on the premises, but some of their work was also done by established makers with their own businesses, for example George Jones.[19] Wheatstone travelled for the business during the 1820s though there is evidence that he found this travelling distasteful.[20] His inventions and suggestions returned

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[15] For a detailed analysis of the ideology of those "gentlemen" who pursued science (particularly geology) in the early nineteenth century, see M.J.S.Rudwick, *The Great Devonian Controversy. The Shaping of Scientific Knowledge among Gentlemanly Specialists,* (Chicago, 1985), esp.pp.9-18.

[16] Bowers, *op.cit.* (note 1), p.8.
[17] *Ibid.,* p.9.
[18] *Ibid.*
[19] *Ibid.,* p.37.
[20] "Sir Charles Wheatstone", *Minutes of Proceedings of the Institution of Civil Engineers,* 1876-7 (47), pp.283-90.

a profit to the concern, however, so he may have been excused the time which he devoted to the study of the principles of sound. The greatest contribution which Wheatstone made to the business was the concertina, for which he took out a patent in 1829.[21] The concertina is one of very few musical instruments whose invention can be explicitly attributed to one man. He was patenting improvements to it as late as 1844,[22] and among Wheatstone's papers at King's College, London, has been inserted an advertisement for C.Wheatstone and Co. of 3 Ives St., Draycott Ave., London, describing themselves as "Manufacturers of Concertinas", dated 1958.[23]

Consideration of Wheatstone's work on musical instruments is essential if we are to understand his wider concerns with scientific instruments. Apprenticeship as a musical instrument maker meant that he acquired practical, manual skills, and he was able to combine these skills with the philosophical skills which he learnt through books to devise and build new instruments in many different fields. It was not unique to find a man of science who could build his own instruments, but it was rather more extraordinary to find a man of science who had a background in manufacture of instruments.

Wheatstone is a prime example of the futility of attempting to construct boundaries between practitioners of pure science and practitioners of applied science in the early nineteenth century.

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[21]Patent No.5803 (Wind Musical Instruments), 19 June 1829.
[22]Patent No.10041 (Concertina and other musical instruments in which the sounds are produced by the action of wind on vibratory springs), 8 February 1844.

[23]Wheatstone Papers, "Foolscap File", King's College, London.

He studied sound in what might be described as the manner of a "pure" scientist, aiming to elucidate the principles governing sounds, but he was always alert to the practical applications that might be made of his discoveries, and at this "applied" end he was able not only to devise instruments and apparatus, but was able to build them as well. When he began studying areas other than sound, this same pattern applied. The real distinction which existed in this period was that between the philosopher and artisan. The *philosopher* studied nature with a view to accumulating truths and laws, but also always with an eye to the application of these laws. This application was an important part of the philosopher's *raison d'etre.* The *artisan,* on the other hand, performed practical work, and this was to an increasing extent devised by the philosopher, as in the case of a new instrument or a new example of engineering. It will be argued in this thesis that the philosopher/artisan distinction was brought into sharper relief in the nineteenth century, in the wake of the industrial revolution. In particular, where an instrument maker might previously have performed work appropriate to a *philosopher,* by the mid-nineteenth century his status was very much that of an *artisan,* according to the above definition. Charles Wheatstone had started his career as an artisan, but his activities pressed his claim to be seen as a philosopher.

2. Studies on Sound and Entry to the Philosophical Community.

In 1825, Wheatstone wrote to John Herschel to report on a new philosophical instrument, called the Kaleidophone. "...it is the first that I have constructed"[24] wrote Wheatstone. An account of the Kaleidophone was published in 1827.[25] The purpose of this instrument was not to be *useful* as such; its purpose was to provide a demonstration of the complex motion of a sounding body. However he did regard the Kaleidophone as of use in as much as it involved the application of scientific principles to "ornamental and amusing purposes",[26] thus increasing their popularity and enabling one to remember their effects more easily.

The Kaleidophone consisted of a steel wire in a firm base, with a bead of silvered glass on top of the wire. When the wire was set vibrating the reflection of a point of light in the glass bead was observed to follow symmetric and ornate paths, by persistence of vision. Wheatstone is known to have lent Babbage his Kaleidophone.[27]

By this time Wheatstone was employing instrument makers to construct his new designs for him, rather than performing all such work himself. Among the many other instruments bearing on the science of sound which he devised was a demonstration apparatus concerned with resonance. An example of this is to be

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[24] Wheatstone to J.F.W.Herschel, 1825, cited in Bowers, *op.cit.* (note 1), p.15.

[25] C.Wheatstone, "Description of the Kaleidophone, or Phonic Kaleidoscope", *Quarterly Journal of Science, Literature, and the Arts* , 1827, pp.344-51.

[26] *Ibid.,* p.344.

[27] Wheatstone to C.Babbage, British Library Add.Ms. 37201 f.586.

found in the Natural Philosophy Collection of the Department of Physics at Aberdeen University. The apparatus is a hollow metal toroid, with a small gap at one side. The upper half can be swung out to make an S shape instead of the toroid shape. The instrument was used to demonstrate Wheatstone's theoretical prediction that when an organ pipe open at both ends was sounding its lowest resonant note (i.e. its fundamental mode), the air in the centre of the pipe would be stationary and the air at the ends would be vibrating most. When the apparatus has the S shape this resonance can be excited by sounding a bowed plate at the correct frequency over one of the openings. The vibrating air comes out at both ends of the pipe at the same time, and goes in both ends of the pipe at the same time. However, when the apparatus has the toroid shape, the resonance cannot be excited, because the plate is trying to push the air in one end at the same time as it is pulling air out the other end.[28]

Here we have a piece of demonstration apparatus of Wheatstone's which, unlike the Kaleidophone, had very little dramatic impact. Still, this instrument evidently did find a market, even though it illustrated a minor point of theory. The instrument was manufactured by the instrument makers Watkins and Hill, of 5 Charing Cross, London, and their involvement in this and other areas illustrates the benefit the work of a philosopher could be in creating a market for the artisan.

Francis Watkins was curator of philosophical instruments in

[28] J.S.Reid, "A Forgotten Demonstration by Charles Wheatstone", *Bulletin of the Scientific Instrument Society,* 1987 (12), p.11.

the University of London,[29] and the firm of Watkins and Hill, who ceased to occupy the Charing Cross premises in 1856,[30] were one of the most notable names on instruments (optical, mathematical, and philosophical) of this period. For example they made Wheatstone's spark spectrum apparatus, around 1834.[31] This machine could observe the spectra produced from electrodes of mercury, tin, cadmium, lead, and other metals. The appropriate metal was melted in a cup in the machine and an electric discharge was passed through it to the electrode above. The operator could then produce sparks in rapid succession by turning a handle, and the sparks could be observed through a prism by means of a telescope with a micrometer eyepiece.[32]

Wheatstone's relationship with the firm evidently continued for some time, as they were making pseudoscopes for him in 1850 and 1855,[33] and he was also using a Watkins and Hill induction coil in some experiments of around 1867.[34]

Watkins was clearly a technically competent maker to have marketed such a variety of instruments. The instrument making trade, by the nineteenth century, was such that the maker's name on an instrument was no guarantee that he had constructed it himself. With Watkins, however, there exists correspondence which

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[29]E.G.R.Taylor, *The Mathematical Practitioners of Hanoverian England 1714-1840*, (Cambridge, 1966), p.438.

[30]Reid, *op.cit.* (note 28).

[31]Notes to exhibits in Science Museum, London.

[32]*Ibid.*

[33]*Ibid.* A pseudoscope is an instrument with which an observer sees the converse of an object (the instrument has prisms deployed so that the left eye sees the right eye's view, and vice versa).

[34]Wheatstone Papers, File 26, King's College, London.

demonstrates that he had an interest in scientific principles. For example, he wrote to Babbage in 1844 returning Babbage's "mechanical notations of the condensing steam engine",[35] and earlier, in 1834, was asking Babbage to refer him to a journal article concerning "M.Plateau's experiment", on induction.[36] The name of Francis Watkins will recur in this thesis as an example of a prominent instrument maker in London in this period. For the moment, I simply wish to draw attention to the fact that Wheatstone admired the work of Watkins and Hill enough to employ them more than once, and that Watkins himself had an interest in the philosophical principles behind the instruments he made. However, it should also be noted that Watkins, unlike Wheatstone, did not contribute anything new to scientific knowledge, nor did he devise *new* instruments as did his eighteenth-century counterparts (though he could refine existing designs). The main benefit to his business derived from building instruments whose designs, as with the acoustic demonstration apparatus, had been developed by philosophers such as Wheatstone.

Wheatstone had made known his theoretical studies on sound in the late 1820s and early 1830s, and gained recognition in the scientific community, not so much by published articles as by lectures. Wheatstone did not in general give these lectures himself, however. Many were given by Michael Faraday. The first recorded session on which Faraday lectured on material supplied

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[35] F.Watkins to C.Babbage, 24 May 1844, British Library Add.Ms. 37193 f.69.

[36] F.Watkins to C.Babbage, 7 March 1834, British Library Add.Ms. 37188 f.246.

by Wheatstone was on 15 February 1828, and the subject of the lecture was "Resonance".[37] In another such joint discourse, on 11 June 1830, proposals of Wheatstone were announced by Faraday to the Royal Institution about a method for finding the velocity of an electric spark.[38]

By this time Wheatstone's attentions were turning from sound to the more fashionable, and potentially more useful, study of electricity, though also from the standpoint of its *transmission*. He published his last paper on sound in 1833: it was an analysis of Chladni's figures.[39] The main reason for the delivery by Faraday of lectures for him was that Wheatstone was a failure as a lecturer. He had been timid as a boy, and though eloquent in private conversation, was unable to address an audience without a major attack of nervousness. His style was always regarded as poor: for example he turned his back to audiences and mumbled into his lecture notes.[40] In 1813 he had won a gold medal for proficiency in French at his school in Vere Street, but it was not awarded to him because he refused to recite a speech at the Prizegiving.[41] Bearing this in mind, it is not surprising that he chose Michael Faraday, a personal friend, and noted for his

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[37] C.Wheatstone, "On the Resonances, or Reciprocated Vibrations of Columns of Air", *Quarterly Journal of Science, Literature, and the Arts,* 1828, pp.175-83.

[38] M.Faraday, "On the laws of co-existing vibrations in strings and rods", *Quarterly Journal of Science, Literature and the Arts,* 1830, pp.405-6.

[39] C.Wheatstone, "On the Figures obtained by strewing Sand on Vibrating Surfaces, commonly called Acoustic Figures" *Philosophical Transactions of the Royal Society of London,* 1833, pp.593-634.

[40] "Sir Charles Wheatstone", *op.cit.* (note 20).

[41] *Ibid.*

lecturing ability, to deliver lectures for him, firstly on sound, and later on electricity. What may be surprising is that a notoriously bad lecturer should have been appointed to a professorship at a University, though it was undoubtedly this appointment which consolidated Wheatstone's position among the philosophical elite. In the next section I would like to consider the way in which he was able to take advantage of this institutional base to emphasise his claims to knowledge and authority, both by his teaching and by his research.

3. Wheatstone as Professor at King's College, London.

Wheatstone was appointed Professor of Experimental Philosophy at King's College, London, in 1834. Henry Moseley had been Professor of Natural and Experimental Philosophy, though on Wheatstone's appointment Moseley became Professor of Natural Philosophy and Astronomy. It seems that Wheatstone had not applied for the post but that his reputation was already such that his services had been sought.[42] Wheatstone retained the professorship for the rest of his life, though it was by no means a full time job for him. For example he was still able to devote some time to the musical instrument business, and would in future years devote a great part of his time to areas such as electric telegraphy and the development of meteorological instruments. Some professors, such as John Phillips, the Professor of Geology, did not even live in London (Phillips lived in York),[43] so it is obvious that University professorships were not regarded as exclusive occupations.

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[42] Bowers, *op.cit.* (note 1), pp.54-5.
[43] *Ibid.,* p.54.

What the performance of Wheatstone's work at King's, which will be discussed in this section, illustrates is that one's institutional context could be of considerable advantage in one's ability to perform scientific work. The privileged status which he possessed as a result of his professorship would have been appreciated by Wheatstone, and work which his institutional base enabled him to do reinforced this elite status.

As Professor of Experimental Philosophy in a University, Wheatstone had a fundamental concern with instruments and apparatus to be used to illustrate his lectures. Among Wheatstone's letters and papers at King's College is a file labelled "Lectures on Sound".[44] In this file are several sheets in Wheatstone's hand which would seem to be lists of apparatus which he employed in his lectures or laboratory demonstrations as Professor of Experimental Philosophy. These instruments include those used for the study and demonstration of principles of meteorology, optics, heat, electricity, magnetism and mechanics, as well as those to be used in lectures on sound.

The King's College historian, F.J.C.Hearnshaw, stated in his centenary history[45] that Wheatstone had wholly ceased lecturing by 1840, and the only lectures he was known to have given were those on sound in 1835. These lectures are known to have commenced on February 17, 1835, and concluded on April 7, 1835,

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[44] Wheatstone Papers, File 22, King's College, London.
[45] F.J.C.Hearnshaw, *A Centenary History of King's College, London,* (London, 1929), p.149.

and the fee for the course was £1.1s.[46] However, a printed syllabus exists among Wheatstone's papers at the college for another course of lectures, beginning on Tuesday 9 May, 1837, and finishing on Tuesday 27 June, 1837. These lectures were entitled "The Measures of Sound, Light, Heat, Magnetism, and Electricity".[47] If these lectures took place, and the printed syllabus suggests they should have, then it seems that these extensive lists of apparatus among the papers represent instruments used in connection with those lectures. Wheatstone was certainly in the habit of using experiments and diagrams in his lectures: for example in the 1835 course of lectures on sound, he typically displayed 20 experiments and diagrams in a single lecture.[48]

The breadth of subjects covered by the 1837 syllabus bears testimony to how far Wheatstone had come from his studies on sound a decade earlier. For example on May 23, he lectured on "Photometers of Bouguer, Leslie, Humboldt, Quetelet, le Maistre, Ritchie, Potter, etc.- A new photometer", and also covered "Measures of the duration of light - Experiments with revolving disks, mirrors, and prisms - A new chronoscope".[49] On June 6, he moved on to measures of temperature and lectured on Daniell's Pyrometer, Nobili and Melloni's thermoscope etc., and the following week lectured on measures of the radiation and absorption of heat and the experiments of Leslie, Delaroche,

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[46] Wheatstone Papers, File 22, King's College, London.

[47] *Ibid.*
[48] *Ibid.*

[49] *Ibid.*

Melloni, and others. The lecture on measures of dynamic electricity included the galvanometer, Harris' thermo-electrometer, and Faraday's voltameter.[50]

By reference to such varied areas in his teaching Wheatstone was able to emphasise the breadth of his expertise to a receptive audience, but he did not only use King's as a place to teach. He also carried on personal research there, most notably experiments on the velocity of electricity.

As a boy of 15, Wheatstone had been present at some of Francis Ronalds' experiments with an electric telegraph in his garden at Hammersmith.[51] One of Ronalds' experiments tried to find the speed of electricity in a wire by measuring the time delay between connection of an electrical machine at one end of a wire and the firing of a cannon at the other end. No time lapse was ever observed, so it was presumed that the velocity of electricity was infinite, or at any rate too high ever to be measured.[52] In the early 1830s, Wheatstone did attempt to measure the speed of a spark through the air, but was more successful in his attempts to measure the speed with which electricity travelled in a wire.[53]

Wheatstone pointed out that previous experiments which attempted to detect time intervals between discharges across

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[50] *Ibid.*

[51] G.Hubbard, *Cooke and Wheatstone and the Invention of the Electric Telegraph,* (London, 1965), p.8.

[52] *Ibid.,* p.7.

[53] C.Wheatstone, "An Account of some Experiments to measure the Velocity of Electricity and the Duration of Electric Light" *Philosophical Transactions of the Royal Society of London,* 1834, pp.583-91.

spark gaps at opposite ends of a wire (the ends being close together to facilitate observation) had failed to detect any time lag because they involved the theoretical assumption that electricity was a single fluid, passing from one end of the wire to the other.[54] Wheatstone wanted to make his experiment independent of this theoretical assumption, and did so by placing a third spark gap at the centre of the wire. This arrangement would still be capable of detecting a velocity if electricity was a single fluid, but unlike previous trials, it would also work if electricity was composed of two fluids, as in that event the spark ought to occur at the middle gap later than at the two outer gaps (where sparks should occur simultaneously).

Wheatstone first carried out these experiments at the Gallery of Practical Science in Adelaide Street (off the Strand). He used half a mile of copper wire and a rotating mirror, employed because the apparent motion of the reflected image in a small moving mirror would be equal to an extensive motion of the object itself.[55] It was vital to determine the angular velocity of the axle carrying the mirror, and to do this Wheatstone employed an arm of the apparatus itself to produce a sound whose frequency was an indication of the angular velocity of the mirror. The maximum angular velocity he could achieve was 800 revolutions per second. Subsequent addition of a registering apparatus reduced this to 600 revolutions per second. The figure for the velocity of electricity which was quoted when the paper concerning these experiments was published, was 288,000 miles per

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[54] *Ibid.*, p.587.
[55] *Ibid.*

second. However, Wheatstone was careful to state that this represented not a *measurement* of the velocity of electricity, but a *detection* of the fact that it had a finite value.[56]

In February 1836, Wheatstone obtained permission from King's College to perform some similar experiments in the vaults of the College. He also got a grant from the Royal Society of £50 towards the expenses of these experiments, which he regarded as somewhat inadequate.[57] As if to point this out, on his death in 1875 he left £500 to the same Royal Society fund.[58]

In the report of his proposals for these experiments in the summer of 1835, Wheatstone suggested using a longer circuit than in the earlier trials: one of 4 miles in length. He also suggested the use of two different types of metal, each 4 miles long, for the purposes of comparison, and the two wires might even be connected to form an 8-mile circuit. A 4-mile circuit of copper wire was used in the King's College experiments, though it seems the proposal to build a circuit of iron wire was not implemented. As a result of these experiments, the original figure of 288,000 miles per second as the detected velocity of electricity was considerably reduced.[59]

By research which he carried out in the 1830s on the velocity of electricity, and by the teaching which he performed during the early years of his tenure, then, Wheatstone was able to use the institutional base of King's College to establish fully his position as part of the elite of the scientific

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[56] *Ibid.*, p.591.
[57] Bowers, *op.cit.* (note 1), p.48.
[58] *Ibid.*, p.58.
[59] *Ibid.*, p.49.

community. In later chapters I will consider the similar way in which Faraday and Airy were able to use their institutional bases to consolidate their positions among the scientific elite and press the claims of men of science to status in society at large. I will also discuss the work of Charles Babbage, who did not possess an institutional base for much of his career.

The research which Wheatstone performed at King's, moreover, was work which would have been difficult to fund outside an institutional base, and he was able to get instrument makers to do work for him for which his employer paid, in whatever area he was working. Among his other electrical concerns at this time, for example, was the generation of electricity, and particularly thermo-electricity. Early in 1837 he pointed out the capability of the thermo-electric pile as a source of electricity.[60] It was Seebeck who had discovered, in 1822, that when different metals are soldered together, and their junction heated, a current is generated. Wheatstone's thermo-electric pile had 33 elements of Bismuth and Antimony formed into a cylindrical bundle. Two thick wires connected the poles of the pile with a spiral of copper ribbon, with the coils of this spiral being well insulated. On heating the pile and breaking the contact, a small spark was seen. The experiment suggested to Wheatstone that electricity from different sources was similar.[61]

The pile was made by the instrument maker John Newman, who made optical, mathematical, and philosophical instruments. Newman was most notable for his barometers and thermometers. For example

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[60] Jeans, *op.cit.* (note 1), p.129.
[61] *Ibid.*, pp.129-30.

he made the standard barometer placed in the rooms of the Royal Society in 1822, and adjusted the water-barometer erected there in 1831 by Daniell (the same year that Daniell was appointed Professor of Chemistry at King's College).[62] In 1836, at his premises of 122 Regent St., he made a small copy of Saxton's electro-magnetic machine.[63]

Newman evidently did some work for Wheatstone after constructing this pile, for there is a small manuscript note in Wheatstone's hand in the King's College archive, dated July 5, 1843, which reads "Mr.Newman. Thermo-electric battery to be put in order. Earthquake indicators to be finished. Directions for apparatus for the velocity of electricity for Melloni to be given".[64] This type of note is not uncommon in Wheatstone's papers. This particular one is of interest as it reveals that Wheatstone had not completely left the study of thermo-electricity after 1837, and though it may only be a reminder for Wheatstone's own benefit, it suggests that Wheatstone and Newman worked together in a number of different areas. Wheatstone performed some experiments at King's College on 27 June 1843 in which he attempted to find the electro-motive force of a thermo-electric element compared to that of a standard voltaic element.[65] He found that the electro-motive forces were in the ratio 1:94.6, i.e. that of the thermo-electric element was much smaller.[66] The thermo-electric element, of Bismuth and Copper, was likely to have been made by Newman, whose name will recur

[62] Taylor, *op.cit.* (note 29), p.400.
[63] *Ibid.*
[64] Wheatstone Papers, File 4, King's College, London.
[65] Wheatstone Papers, File 23, King's College, London.
[66] *Ibid.*

throughout this thesis, particularly in the chapter on Faraday and the Royal Institution, for whom he was the official instrument maker.

The performance of Wheatstone's research at King's, then, shows that the instrument maker could have a role to play in the investigations of the philosopher. Although Wheatstone was a skilled manual worker himself, the delegation of such tasks to artisans gave him *time* to devote to his philosophical work, and thus they performed an important supporting role. The role of the Professor himself, however, was seen as the most important one, both in terms of his work in the College, and in terms of the benefits which accrued to society as a result of his work. The example of such service to society which Wheatstone regarded as his greatest was the electric telegraph, and herein also lay the major scientific import of these experiments he was able to perform at King's. The fact that electricity had a finite, high velocity, showed that communication by electricity over long distances was at least a possibility. Moreover, as he was seen as having measured one of the fundamental constants of nature, Wheatstone's reputation in the scientific world increased as a result of this work performed at King's.

4. The Electric Telegraph.

The most extensive and fruitful electrical researches which Wheatstone undertook during his career were those concerning the electric telegraph. He was first led to think in terms of the possibility of communication over long distances using electricity by his studies of the transmission of sound through solid bodies. The velocity of electricity experiments were also a necessary prelude to his telegraphic researches, as the velocity of electricity which he detected was large enough to suggest that communication by means of electricity was feasible. The practicability of such communication was another matter, however, and though there had been several attempts made to construct a working telegraph using electricity, none was useful over anything but short distances.[67]

The earliest account of an electric telegraph was in the *Scot's Magazine*[68] of 1753. This telegraph used 26 wires between sender and receiver, and had an electrostatic machine at one end and spark gaps at the other. However, as it used static electricity, it could not be a practical proposition over long distances as it was too difficult to maintain the insulation. Also, with a wire for each letter of the alphabet, it was complicated. Francis Ronalds' telegraph of 1816 also used static electricity but it only needed a single wire, and depended on a clockwork mechanism turning a lettered dial at each end. The system could work over distances of 150 metres or more, though it was slow.[69]

When he came to consider the practicability of telegraphic communication, however, Wheatstone never entertained the possibility of using static electricity. The problems of insulation were too great. Instead, he made use of the Oersted

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[67] Hubbard, *op.cit.* (note 51), pp.6-14.
[68] Letter signed "C.M.", *Scots' Magazine,* 7 February 1753.
[69] Hubbard, *op.cit.* (note 51), p.8. For these and other early forms of telegraph, see J.J.Fahie, *A History of the Electric Telegraph to the Year 1837,* (London, 1884).

effect, and also, unlike Ronalds' telegraph, he did not use the idea of clockwork mechanisms turning lettered dials at each end.[70] Other researchers had utilised the Oersted effect prior to Wheatstone's commencement of telegraphic studies in 1836, although none of their attempts were practical over anything but the shortest distances. A design due to Schilling was demonstrated by Professor Muncke, of Heidelberg, in 1836. This demonstration was seen by William Fothergill Cooke (1806-1879), who constructed his own electric telegraph three weeks later.[71]

Cooke was not a scientific man. He had no scientific education. One of the factors which his six wire, three circuit, three needle instrument incorporated, and which he felt was essential to any practical telegraph was the idea of reciprocal communication: signals could be sent or received at either end.[72] For any scientific man it was obvious that electrical signals could be sent both ways along a wire, but Cooke, not being well versed in electrical theory, saw it as appropriate to emphasise the fact that his machine incorporated this feature. The instrument also included a galvanometer, to detect injuries to the wire, and an alarum at each end, meaning that the instrument need not be constantly attended, as a bell would ring before a message was to be received. The idea of the alarum did not originate with Cooke, though it seems initially he claimed it as his own.[73]

The next instrument which Cooke conceived was similar to

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[70] Bowers, *op.cit.* (note 1), p.102.
[71] Hubbard, *op.cit.* (note 51), p.28.
[72] Bowers, *op.cit.* (note 1), p.107.
[73] *Ibid.*

Ronalds' telegraph in that it employed a single circuit only, though of course it utilised the Oersted effect, rather than static electricity. By this time he had begun to run into difficulties and after consulting Faraday with little success, was referred to Wheatstone by Roget.[74] Wheatstone's advantage, essentially, was that he knew Ohm's Law and Cooke did not, and this was the reason that Cooke had been experiencing difficulties in getting his telegraphs to work.

Wheatstone's main achievements in telegraphy by this time included apparatus for making and breaking the circuit, and his commutating principle, "by which a few wires were converted into a number of circuits".[75] In consulting Wheatstone in 1837, Cooke was not interested in the advance of scientific theory. His desire was merely to obtain money by taking out a patent for a telegraph which worked. In other words he was an entrepreneur. On the other hand, Wheatstone's main concern with the telegraph was as a piece of scientific research. His primary concern was not with making money. However, he saw in Cooke the business expertise necessary to make the electric telegraph a commercial success, which would at least emphasise Wheatstone's own expertise as its inventor, and entitle him to some recognition by society as a scientific man with the ability to apply his knowledge for the betterment of the condition of mankind. They took out a patent in June 1837,[76] though this patent actually excluded all of Cooke's instruments (this was not the intention) and was a direct consequence of

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[74] *Ibid.*, p.109.
[75] Hubbard, *op.cit.* (note 51), p.38.
[76] Patent No.7390 (Electric Telegraphs), 12 June 1837.

Wheatstone's permutating keyboard. It had five wires for five needles, and the keyboard selected wires for the outward and return currents. The new features which the patent included were the permutation of circuits using a keyboard, the use of two needles on the dial, whose convergence indicated letters, and the use of vertical weighted astatic pairs of needles.[77]

By this time Cooke was explicitly concerned with business affairs, and not with design of instruments, and he received 55% of the profits compared to Wheatstone's 45%. It has been argued that Wheatstone deserved a higher percentage of the profits.[78] After all, it was he who had applied Ohm's theory to telegraphic circuits, which enabled him "to ascertain the best proportions between the length, thickness etc. of the multiplying coils and the other resistances in the circuit, and to determine the number and size of the elements of the battery to produce the maximum effect".[79] This, in short, was why the 1837 Cooke and Wheatstone telegraph worked, and all previous telegraphs failed. However, one could also argue that without Cooke the telegraph would not have been a commercial success, as he secured its adoption by the railways. Such retrospective judgements as to who desrved financial reward are misleading and unhelpful in an historical discussion. It is more important to consider Wheatstone's own

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[77] Hubbard, *op.cit.* (note 51), pp.58-67 gives details of the patent.

[78] Bowers, *op.cit.* (note 1), p.109.

[79] C.Wheatstone, *The Electric Telegraph: Was it invented by Professor Wheatstone? Mr.Wheatstone's Answer* (a reply to a pamphlet by Cooke), (London, 1856).

feelings, which were that he was the scientific man responsible for *inventing* the telegraph, and so he ought to be recognised as the most important individual in its realisation. This seeking of an acknowledgement of the vital role he played was more of a motivation for him than the money. The member of the scientific elite, as will be argued throughout this thesis, was motivated by a belief in his value to society more than by financial ambition. Those who aspired to such membership therefore ought to be free from such motives. In this context a controversy involving the ambitious instrument maker E.M.Clarke is worth relating, owing to its relevance to the telegraph.

In 1837, Cooke and Wheatstone were certainly not able to operate their telegraph by means of a magneto-electric device (or "magneto" for short). In 1840, however, they took out a new patent for a telegraph employing such a device in place of the usual battery.[80] For some years afterwards the battery-driven version was still dominant, though by the mid-1840s the magneto was to become the predominant means of driving the telegraph.[81]

The first such generator had been developed in 1831 by Faraday, only a month after he had discovered the principle of electromagnetic induction upon which it was based. The Faraday generator used a rotating copper disc and a permanent magnet, but could only generate a low electro-motive force. Subsequent generators had magnets and coils of wire rotating relative to each other. Pixii's machine of 1832 had the coils fixed and the permanent magnet rotated.[82] This machine seems to have been the

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[80] Bowers, *op.cit.* (note 1), p.67.
[81] *Ibid.*
[82] *Ibid.*, p.68.

inspiration for Saxton's machine in 1833, although in this machine the magnet was fixed and the coils rotated.[83] Whilst mentioning Pixii, Saxton did claim originality for his design.[84] The arrangement which Wheatstone used in his 1840 ABC telegraph was one dating from 1835, due to Clarke.

Joseph Saxton, of 24 Sussex Street, London, was the contriver and maker of a "great magnet" exhibited at the Gallery of Practical Science in Adelaide Street, off the Strand. This was apparently a magneto-electric machine, according to his contemporary Francis Watkins.[85] Saxton's 1833 magneto-electric machine was exhibited at the meeting of the British Association for the Advancement of Science of that year, held in Cambridge.[86] Saxton's machines were certainly used by Wheatstone, as several of them occur in the lists of apparatus which he had at King's College.[87] It may therefore be surprising that it was the arrangement of Edward Montague Clarke that Wheatstone used in the magneto of his 1840 ABC telegraph, as Clarke and Saxton were great rivals. Clarke, an optical and philosophical instrument maker, was for some time employed by Watkins and Hill before moving to his own premises in Lowther Arcade.[88] Clarke designed and built a thermo-electrometer (which measured the power of a battery to heat metal wires) and he also modified and built a

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[83] *Ibid.*
[84] J.Saxton, "Mr.J.Saxton on his Magneto-electrical Machine, with Remarks on Mr.E.M.Clarke", *Philosophical Magazine,* 1836, pp.360-5.

[85] Taylor, *op.cit.* (note 29), p.459.
[86] *Ibid.*
[87] Wheatstone Papers, File 22, King's College, London.
[88] Taylor, *op.cit.* (note 29), p.359.

design of Faraday's for an instrument called the volta-electrometer which measured the volume of gases given off by the decomposition of acidulated water.[89] However Clarke was most famous for his Magnetic-Electrical Machine, of which an account was published in 1836.[90] Here, he described himself as "Edward M.Clarke, Magnetician". Saxton had at that stage not published an account of his machine, and in doing so the following month, he charged Clarke with stealing his invention:

> A reader...might be misled, from the paper I have alluded to, to believe that the electro-magnetic machine there represented was the invention of the writer, and that the experiments there mentioned were made for the first time by its means. No conclusion, however, would be more erroneous. The machine which Mr.Clarke calls his invention, differs from mine only in a slight variation in the situation of its parts, and is in no respect superior to it.[91]

Saxton then pointed out that his machine was well known from mentions it received in articles by Faraday and Daniell, as well as Wheatstone. Both Saxton's and Clarke's accounts include diagrams, and both only address how the machines are constructed and what effects they can produce. They do not deal with any theoretical questions concerning their mode of operation.

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[89] I.R.Morus, "The Sociology of Sparks: An Episode in the History and Meaning of Electricity", *Social Studies of Science,* 1988 (18), pp.387-417, on p.399.

[90] E.M.Clarke, "Description of E.M.Clarke's Magnetic Electrical Machine" *Philosophical Magazine,* 1836, pp.262-6. A slightly extended version of this paper is E.M.Clarke, "A Description of a Magnetic Electrical Machine, invented by E.M.Clarke, Magnetician", *Annals of Electricity,* 1836-7 (1), pp.145-55.

[91] Saxton, *op.cit.* (note 84), p.361. For further detail on this dispute, see I.R.Morus, *The Politics of Power. Reform and Regulation in the work of William Robert Grove,* (Ph.D. Thesis, University of Cambridge, 1989), pp.18-21.

Clarke's reply to Saxton[92] pointed out that the Clarke machine greatly reduced the vibration of the magnets, which was unavoidable in the Saxton arrangement, but the two men essentially talked through each other, Clarke claiming that all he had done was to describe "E.M.Clarke's Magnetic-Electrical Machine", Saxton claiming that Clarke, by omitting to mention him, considered himself the inventor. Wheatstone, then, used Clarke's arrangement because it was an improvement on Saxton's in terms of vibration. He sided with Saxton, however, in as much as he recognised that Clarke was claiming inventions as his own with which he had very little to do. Wheatstone wrote to Cooke on 20 March, 1838:

> I hope you have thought nothing more of Clark's [sic] offer to you. I think it would be a most ill-judged step to enter into any arrangement with him, and I will give you my reasons.
> 1st. I am perfectly satisfied with the energy, convenience, and portability of the battery I employ, and do not imagine it can be much exceeded in any of these respects.
> 2nd. Clark has on several occasions asserted that he has made important discoveries which were nothing but vain boasts.
> 3rd. Nothing would give me more annoyance than to put it in the power of a man like Clark to say that the Telegraph was not completed without his assistance, and this he would be able to do by the arrangement you proposed, even if his "invention" should turn out, as I am sure it will, mere moonshine.
> 4th. Neither you nor I can insert in any patent we may hereafter take out, this thing you propose to purchase, without inserting Clark's name in the patent.[93]

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[92] E.M.Clarke, "Reply of Mr.E.M.Clarke to Mr.J.Saxton" *Philosophical Magazine,* 1837, pp.455-9, on p.456.

[93] Wheatstone to W.F.Cooke, 20 March 1838, Cooke Papers, Volume 1, Institution of Electrical Engineers.

Admittedly Clarke had achieved a reputation for pirating other people's inventions by this time, but Wheatstone's letter indicates a wider concern with infringements on intellectual property which belonged to men of science. In Wheatstone's eyes, Clarke was an instrument maker, not a man of science. As such he ought to construct instruments, and not claim to have invented grand new machines when it was plain that the ideas for these had come from men of science. When he wrote the above letter, Wheatstone was established in the chair of Experimental Philosophy at King's College and regarded himself as a member of a scientific elite. This elite did not include instrument-makers such as Clarke. Although Wheatstone's motives for being part of this scientific elite were not monetary ones, he was always keen to ensure that scientific expertise brought to its possessor the pecuniary rewards that it should. Thus, although he was initially offered one-sixth of the profits of the original telegraph patent, he was eventually to settle for a 45% share. In the case of Clarke, Wheatstone did not wish the instrument-maker to derive income from an "invention" which was probably only a modification of a previous design by a man of science. So, while the letter to Cooke was directly concerned with Clarke, Wheatstone's sentiments were more widely applicable.

This was to become of more than passing interest in 1840, when Cooke became dissatisfied with what he saw as the increasing tendency of Wheatstone to ignore him when referring to the invention of the electric telegraph. Although Wheatstone denied ever ignoring Cooke, it is true that on occasions he omitted explicitly to mention him, which was considered by Cooke to amount to the same thing. Cooke often omitted to mention Wheatstone too - for example in 1845, it is stated in *Chambers' Magazine:*

> Mr.Cooke, the inventor, who, with the assistance of Professor Wheatstone, has brought the instrument to its high condition of usefulness, was in the room, and readily explained to the writer not only the nature, but the origin and progress of the invention.[94]

There were undoubtedly causes for grievance on both sides, and the case was put to arbitration. The task of the arbitrators was to determine "in what shares, and with what priorities and relative degrees of merit, the said parties hereto are co-inventors of the Electric Telegraph".[95]

Wheatstone was adamant that only men who had made scientific enquiry their subject of study should be admitted as arbitrators. The reason for this was obvious: the arbitration concerned the invention of a scientific instrument, and only a man of science could truly appreciate quite what "invention" meant in this context. Daniell, acting on the part of Wheatstone, had certainly made scientific enquiry his subject of study, though the same could not be said of M.I.Brunel, who represented Cooke's interests. P.M.Roget was appointed as third arbitrator. Although the case proceeded, the outcome of 27 April, 1841 was not a resolution in favour of either man:

> Whilst Mr.Cooke is entitled to stand alone, as the gentleman to whom this country is indebted for having practically introduced and carried out the

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[94] *Chambers' Magazine*, June 7, 1845.

[95] W.F.Cooke, *The Electric Telegraph: Was it invented by Professor Wheatstone?*, (London, 1856), contained most of the material relating to the arbitration, and was printed along with Wheatstone's answer to this pamphlet of Cooke.

> Electric Telegraph as a useful undertaking, promising to be a work of national importance, and Professor Wheatstone is acknowledged as the scientific man, whose profound and successful researches have already prepared the public to receive it as a project capable of practical application, it is to the united labours of two gentlemen so well qualified for mutual assistance, that we must attribute the rapid progress which this important invention has made during the five years since they have been associated.[96]

Both men said they were satisfied with the outcome, though Cooke did resurrect the dispute some thirteen years later and a series of pamphlets ensued with each man attempting to refute the claims of the other.[97] From Wheatstone's point of view, the main concern was that a man ignorant of scientific theory should not receive credit for a scientific invention. He did not wish to deny Cooke his part in the commercial realisation of the telegraph, but by the same token did not wish to give Cooke a scientific status he did not deserve. Cooke's arguments during the arbitration proceedings often tried to establish the extent of his progress towards a practical telegraph before he met Wheatstone, and sometimes he made use of the relations he had had with instrument makers to back up his claims. For example, some work was done for Cooke by the instrument maker Moore, of Clerkenwell.

In February, 1841, in the middle of the arbitration, Moore wrote to Cooke about the particulars of the first telegraph made "by us" which Cooke had asked for:

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[96] The award of the arbitrators was eventually published in 1856, as part of *ibid.*

[97] *Ibid.*

> I cannot give you any particulars of the instrument with keys as I find that it has never appeared against you in our books. I mean the instrument with ivory knobs with several plates of Brass at bottom with springs etc. fitted to a mahogany case and stand. This instrument was finished while I was at the Isle of Wight and no account was given of it by the man that made it. The man that made the large instrument has left our employ....
> I believe that the instrument that was made first was delivered to you some time in 1836 but it then not being considered complete was not booked. Consequently it remained until the date in hand...
> I am having search made for what can be found of the different parts of this instrument that were made and not sold and I will take the earliest opportunity of sending them.[98]

Further correspondence concerning the instruments which Moore made for Cooke was inconclusive; Moore found some clue as to the "instrument in question", but found also that it had never been booked against Cooke.

The arbitration between Cooke and Wheatstone was over by 1841 and they even took out another telegraph patent together in May 1845.[99] However by this time their relationship was effectively over, Wheatstone having been bought out of his share of the patents in return for a royalty. Though the dispute with Cooke was his most visible concerning the telegraph, Wheatstone also had conflicts with other men over telegraphic instruments. Again the theme was that instrument makers should not be allowed to take the credit for designing instruments with which they had little to do. The credit for designing scientific instruments belonged to men of science, such as Wheatstone himself.

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[98] J.Moore to W.F.Cooke, 2 February 1841, Cooke Papers, Volume 2, Institution of Electrical Engineers.

[99] Patent No.10655 (Electric telegraphs and other apparatus), 6 May 1845.

One such dispute of Wheatstone's was with Alexander Bain (1810-1877), who served for a short time as apprentice to a clockmaker in Wick. He came to London in 1827 and attended lectures at the Adelaide Gallery, seeing the electromagnetic experiments demonstrated there.[100] In October 1840, Wheatstone published a paper on the electro-magnetic clock, which is essentially an ABC telegraph receiver fitted with clock hands.[101] At the same time Wheatstone had devised a printing telegraph. In January 1841, Wheatstone was contacted by a Mr.Barwise, who claimed to be the inventor of the clock. Later it was stated that Barwise and Bain were joint inventors. Wheatstone did not respond until 1842, when he was directly charged by Bain in the press of pirating his invention. Wheatstone's answer was that there was no essential difference between his electro-magnetic clock and the type of electro-magnetic telegraph that was patented in January 1840 (the ABC telegraph). In sending messages the wheel for making and breaking the circuit was moved manually, whereas in showing time it was moved by clockwork. Bain's allegation was that he had communicated the invention of the clock to Wheatstone in August 1840, during a period from August to December that he was employed as a mechanic by the professor. Wheatstone claimed to have suggested the idea of an electro-magnetic clock (a single clock that could show its time in various locations) long before

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[100] C.K.Aked, "Alexander Bain. The Father of Electrical Horology", *Antiquarian Horology,* 1974 (9), pp.51-63, on p.53.

[101] C.Wheatstone, "Description of the Electro-magnetic Clock", *Proceedings of the Royal Society of London,* 1840, pp.249-50.

that date. This claim was verified by W.A.Miller of King's College, and John Martin the artist, who dated Wheatstone's suggestion of the clock to him in May 1840.[102]

Bain also disputed the invention of the printing telegraph. To this, Wheatstone simply pointed out that the printing apparatus was merely an addition to the electro-magnetinc telegraph of which he was obviously the inventor.[103] Edward Cowper said that he had sent Wheatstone information he had asked for about preparing manifold writing paper, and the best form of type for printing on it, on June 10, 1840, long before Bain had any idea of a printing telegraph.[104]

Wheatstone's position ought to have been vindicated, yet Bain was able to take out patents in 1841, 1843, and 1845, with the last two referring to improvements in printing telegraphs and electric clocks, respectively. Bain opposed the bill for incorporation of the Electric Telegraph Company (which bought Wheatstone out for £30,000) which was brought into Parliament in 1846. The bill passed through the Commons but the Lords felt an arrangement ought to be made with Bain. So he too was bought off, subsequently being elected a director, though he soon resigned. In 1851 he was paid £20,486 by the Company as a patentee, even though his patents were only modifications of Wheatstone's ideas. Despite the rewards from these patents, Bain died in near poverty in 1877.[105]

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[102] Jeans, *op.cit* (note 1), p.161.

[103] *Ibid.,* pp.161-2.

[104] *Ibid.,* p.162.

[105] Aked, *op.cit.* (note 100) is a very one-sided account of the dispute in Bain's favour.

As the electric telegraph served as a focus of financial ambition for individuals such as Cooke, Clarke, and Bain, this section has been mainly concerned with portraying the contrast between the motives of these men and those of the philosopher, Wheatstone. While it would not be correct to assume that Wheatstone had *no* interest in money, I have tried to show that his main interest in these disputes was in defending his status as a philosopher, of greater expertise and value to mankind than other groups such as artisans and entrepreneurs. In trying to resolve the tensions between his motives and those of the artisans whom he employed, it was inevitable that disputes should arise over intellectual property. In the next section I would like to develop further, by reference to his later research, telegraphic and otherwise, the notion that Wheatstone considered himself the vital element in the fulfillment of any practical project based on scientific principles, with the artisan performing only a supporting role.

5. Wheatstone's Later Researches and his Relationship with J.M.A.Stroh.

In the mid-1860s Wheatstone was working on the concept of a self-excited generator, or dynamo, a continuation of his concern with the production of electricity manifested in his earlier work on thermo-electricity. This machine was based on the concept that the magnetic field in it could be produced by an electromagnet which had been energised by the generator itself. Wheatstone, Werner von Siemens, and Samuel Alfred Varley were all working on dynamos in 1866.[106] Siemens published the first account of a working dynamo on 17 January 1867, and could therefore lay claim to being the inventor of the dynamo.[107] When a voltaic battery was connected with the wires of Siemens' electro-magnet and then disconnected, a feeble current was generated in the wire of the revolving armature by the permanent magnetism of the electro-magnets. This current, rather than the current from the battery, was then transmitted through the coils of its own exciting electro-magnet, and in this way an output several times the ordinary one was obtained.

Wheatstone displayed his instrument shortly afterwards. It had a soft iron core 15 inches long and bent into a horseshoe, which was wound breadthwise with silk-covered copper wire 640 feet long. The armature (i.e. the piece of iron extending across the ends of the horseshoe magnet) was hollow at two sides to accommodate another 80 feet of insulated wire wound lengthwise. The two wires were connected so as to form a single circuit, and the armature was made to rotate clockwise, thus producing powerful electrical effects.[108] Wheatstone, Siemens, and also Varley came upon their working dynamos independently, so all can lay claim to being the inventor of the dynamo. The credit is usually given to Siemens, as his was the first published account, though Varley had put such a machine on record in a provisional specification before this. Wheatstone, however, had possessed his

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[106] Jeans, *op.cit.* (note 1), pp.208-9.

[107] C.W.Siemens, "On the Conversion of Dynamical into Electrical Force without the Aid of Permanent Magnetism" *Proceedings of the Royal Society of London,* 1867, pp.367-9.

[108] Jeans, *op.cit.* (note 1), p.206.

machines (which he eventually exhibited in a working form in February 1867) before Varley's or Siemens' machines were either finished or ready for trial.[109] The reason this can be asserted is that Wheatstone employed an instrument maker, John Mathias Augustus Stroh, to construct self-excited generators for him. They were made in July and August 1866, and having been finished, tried, and approved of by Wheatstone, they were in the usual course of business charged for by Stroh on 12 September 1866.[110] Thus, chronologically at least, Charles Wheatstone had the first working dynamo, and was aided in its development by an instrument maker.

Stroh (1828-1914) worked for Wheatstone from the mid-1850s to Wheatstone's death in 1875. He was born in Germany, and apprenticed to a clock and watchmaker, but had moved to England on a permanent basis after a holiday during which he visited the Exhibition of 1851. He was introduced to Wheatstone shortly after this, and opened his own premises in London in 1860.[111] Manuscript papers of Wheatstone show that their collaboration was extensive and close. Stroh worked on telegraphs, notably ABC and Automatic telegraphs, in collaboration with Wheatstone, though he had many other scientific interests, including horology and photography. He was also interested in musical instruments.[112] He took out a patent jointly with Wheatstone in 1872 for improvements to reed instruments (harmoniums and concertinas),[113]

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[109] *Ibid.*, pp.208-9.
[110] *Ibid.*, p.208.
[111] "John Mathias Augustus Stroh", *Journal of the Institution of Electrical Engineers,* 1915 (53), pp.871-2.

[112] *Ibid.*
[113] Patent No.39/1872 (Musical Instruments), 4 January 1872.

and though the two never formed a company, they did take out another 3 patents, all related to the telegraph.[114] Wheatstone regarded himself as the elite scientist of the partnership but saw Stroh as more than a mere workman, and appreciated the technical knowledge as well as practical skills which Stroh could give to the partnership.

One of the patents obtained by Stroh referred to the synchronising mechanism for a master clock, patented in 1869. The pendulum of the clock carried a coil which swung over a pair of horseshoe magnets, and the current generated was transmitted to other dial movements in circuit with the coil, which thus became synchronous motors driven by the current. The system was a good one in that it eliminated problems due to faulty contacts, but was a poor time-keeper, as there was too much interference with the free motion of the pendulum.[115]

The nature of their relationship is shown by Wheatstone's manuscript papers, which contain several lists of apparatus to be made by Stroh. For example there is a list of 17 items, including a torsion electrometer, torsion galvanometer, rotary discharger, key discharger, apparatus for charging a column of water, a case of insulating discs, etc.[116] Occasionally we find a list, in Wheatstone's hand, of experiments which Stroh was to carry out. The nature of these instructions suggests that Stroh was more than a manual worker, for example we find in a note of

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[114] Patent Nos.2897/1870 (3 November 1870), 2172/1871 (18 August 1871), and 473/1872 (15 February 1872).

[115] Notes to exhibits in Science Museum, London.

[116] Wheatstone Papers, File 1, King's College, London.

experiments to be made, dated June 1861:

> 1.Experiments with the standard magnet of the present communicator, to determine the simultaneousness of the breaking and making currents
> 2.Magnet with fixed coil and rotating armature put in motion by weights. Break pieces to be arranged so as to stop either the breaking or making currents. For this experiment a narrow armature must be employed...[117]

There also exists a list of experiments "at Mr.Stroh's", dated June 1, 1865,[118] suggesting that he and Wheatstone also used his shop for research purposes.

The relationship between Wheatstone and Stroh, then, was a rather more extensive collaboration than he had with any maker in his earlier years. Importantly, it seems it was a relationship in which, more than before, Wheatstone regarded the maker as having a technical expertise, if not equal to his own, then at least sufficient to aid his own *scientific* contributions. Indeed, Stroh underlined his claims to expertise by becoming a member of the Society of Telegraph Engineers (which became the Institution of Electrical Engineers), and the Physical Society, as well as by publishing scientific papers and serving on various exhibition committees.[119] However, although Stroh initially used his instrument making skills in order to make his living when he arrived in Britain, he can more accurately be seen, from these later achievements, as a foreign man of science seeking to gain standing in the English scientific community. Thus it was not incongruous that Wheatstone should be generous in according credit to him for innovations, in that he saw him as having

[117] Wheatstone Papers, File 29, King's College, London.
[118] *Ibid.*
[119] "John Mathias Augustus Stroh", *op.cit.* (note 111).

sufficient expertise to warrant status, if not among the elite, then at least within the scientific community rather than the community of artisans. This opinion of Stroh contrasted sharply with his views of earlier makers such as Clarke, towards whose claims to have furthered *scientific* knowledge he was hostile, as we have seen in the case of the letter he wrote to Cooke concerning Clarke's battery.

This idea, of an instrument maker such as Clarke being only an *artisan,* in the eyes of Wheatstone, the elite *philosopher,* is exemplified further by a controversy which broke out in 1869 between W.A.Miller, the Professor of Chemistry at King's College, London, and the instrument makers Negretti and Zambra, in which Wheatstone was involved indirectly. The presence of Negretti and Zambras' letter to Miller among Wheatstone's papers suggests that Miller asked Wheatstone's advice on the matter.[120]

Negretti and Zambra wrote to Miller in November 1869 referring to an article of his in the Proceedings of the Royal Society of that year in which Miller described a new deep-sea thermometer, made at Miller's suggestion by Casella.[121] Louis P.Casella of 23 Hatton Garden, who died in 1897, was a maker of optical, mathematical, and philosophical instruments. Negretti and Zambras' contention was that they had exactly the same thermometer as early as 1864, and it was mentioned in a treatise

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[120] Negretti and Zambra to W.A.Miller, 23 November 1869, Wheatstone Papers, File 36, King's College, London.

[121] W.A.Miller, "Note upon a Self-Registering Thermometer adapted to Deep-sea Soundings", *Proceedings of the Royal Society of London,* 1869, pp.482-6.

of theirs on meteorological instruments of that year,[122] and given that Casella was a well-known maker of thermometers, he should have been aware of this treatise and been able to inform Miller of its contents.

Miller's self-registering thermometer adapted to deep-sea soundings was constructed upon Six's plan. In his paper, Miller stated that several of these thermometers had been prepared "with unusual care" by Mr.Casella.[123] The problem faced in the design of this instrument was to adjust the strength of the index spring and the size of the pin so as to permit it to move sufficiently freely when pressed by the mercury, though without running any risk of displacement while the instrument was being raised or lowered into the water. Casella's role was apparently to determine the conditions in the spring for best accuracy, and the tube diameter for best accuracy. "He has also himself had an hydraulic press constructed expressly with the view of testing these instruments".[124] With this press, the experiments described by Miller were made.

What Miller did to protect the thermometers from the effects of pressure was to enclose the bulb of such a Six's thermometer in a second or outer glass tube which was fused upon the stem of the instrument. This outer tube was almost filled with alcohol (some space was left to allow expansion) and the outer tube and its contents were hermetically sealed. Pressure variations then did not reach the bulb, though temperature variations reached it

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[122] Negretti and Zambra, *A Treatise on Meteorological Instruments*, (London, 1864).

[123] Miller, *op.cit.* (note 121).
[124] *Ibid.*

quickly. The design worked, though at a high pressure one such thermometer fractured. Apparently this was an early specimen, and subsequent specimens did not break.[125]

Negretti and Zambra were obviously keen to ensure their precedent in designing such a thermometer. There was a difference, however, between the Miller-Casella pressure-protected Six's thermometer of 1869 and that made by Negretti and Zambra in 1864, and this was that in the former, the evacuated space between sheath and bulb was partially filled with alcohol, and not with mercury as in the latter. The whole dispute, though not directly involving Wheatstone, is typical of the sort of conflict he became involved in where inventions of a financially promising nature were concerned. Practical men such as Bain, Clarke, and in this case the noted London instrument makers Negretti and Zambra, were always keen to have a share of the scientific acclaim which those like Wheatstone received, and this, together with the financial rewards a successful patent might bring, was enough to explain their perseverence in these kinds of dispute. Negretti and Zambra in particular were always ready to boast of their own achievements; for example, in a letter to E.J.Lowe (of Beeston Observatory, near Nottingham) in 1856 they speak of having made a "perfect" maximum thermometer and a "perfect" minimum thermometer, and that bearing such achievements in mind they considered it no surprise that the Jury at the Great Exhibition of 1851 awarded them a Prize Medal.[126]

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[125] *Ibid.*

[126] Negretti and Zambra to E.J.Lowe, 19 May 1856, British Library Add.Ms. 43829 f.28.

Wheatstone's concern was not with promoting a trade, such as was the case with Negretti and Zambra. If we consider his invention of the Polar Clock, we can see that it is typical of him in that it is an invention which again we can say is designed for the benefit and use of mankind. It enables the user to tell the time by the sun when the sun itself cannot be seen, by considering the polarisation of the sun's light.[127]

Wheatstone had two of these instruments made by W.H.Darker. One was accurate to 12 minutes and the other to 30 minutes.[128] Wheatstone needed the services of a maker here because he was not capable of performing the precision work necessary for the manufacture of a successful optical instrument. Optical instrument making required skills which a background in musical instrument manufacture did not provide. Thus both Wheatstone and Darker could be seen to supply different components of the labour required to produce a specific instrument: Wheatstone supplied the intellectual labour involved in its design, and Darker performed the manual work. As will be argued throughout this thesis, however, the philosopher emphasised his as the most important role, and his skill as the most valuable and rare

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[127] Bowers, *op.cit.* (note 1), pp.203-4. This instrument had a Nicol prism as an eyepiece and a plate of selenite as an object. The plane of polarisation of the sky is always 90^0 from the sun, and if the instrument is directed to the North Pole, the position of the prism will need to be adjusted to reproduce any neutralised or other given effect of the plate of selenite. By a consideration of this adjustment or rotation on its axis, the hour of the day can be obtained. See also C.Wheatstone, "On a means of determining the apparent Solar Time by the Diurnal Changes of the Plane of Polarisation at the North Pole of the Sky", *B.A.A.S. Report, Swansea, 1848,* pp.10-12.

[128] Bowers, *op.cit.* (note 1), p.204.

commodity among the processes required for the realisation of practical projects. Besides this division of labour which operated in the development of new instruments, there was also an increasing sense in which the name of a maker on an instrument constructed on his premises was no longer a guarantee that he had participated in its construction. From the point of view of the customer, this division of labour in the manufacture of instruments was not always a good thing, as it could allow for mistakes to be made by employees who were not specially trained. An interesting example of this is pointed to in a letter from John Browning to Wheatstone. John Browning was the leading spectroscope maker of the 1870s and 1880s. In 1882 he published a work on how to use the spectroscope.[129] Browning described himself as Optical and Physical Instrument Maker to Her Majesty's Government, the Royal Observatory, the Royal Society, and the Observatories of Cambridge, Utrecht etc.[130] He wrote to Wheatstone on 7 March, 1873: "I beg to enclose you the key of the spectroscope and very much regret my Packer omitted to fasten it to the case".[131] This sort of mistake, it could be argued, would have been less likely to occur if the instrument maker had been responsible for all the stages of the process. This may have been a reason why Wheatstone's partnership with Stroh was so important to him, as he could deal with Stroh on an

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129 J.Browning, *How to Work with the Spectroscope*, (London, 1882).

130 J.Browning to Wheatstone, 7 March 1873, Wheatstone Papers, File 18, King's College, London.

131 *Ibid.*

individual basis. In an age of increasing market pressures, however, there was little option for a maker such as Browning but to expand and depersonalise his business, if he wanted it to survive.

Wheatstone's attitude towards instruments and their makers must be viewed in the context of his ability as a manufacturer of musical instruments in his early years, and his consequent practical skills which enabled him to make the transition to designing and constructing scientific instruments with ease. The instruments he made himself covered the whole spectrum of those he designed. Most of his instruments were aimed at the resolution of a practical problem (e.g. the polar clock), or were practically useful contrivances to mankind (e.g. the electric telegraph), though he did make instruments to demonstrate points of theory as well. In this latter class comes the Kaleidophone, and also a wave machine which he constructed around 1842. This machine, of which examples still survive, consisted of a wooden box with trains of white beads on stalks, and was used as a model to demonstrate light waves in the ether, being able to show various types of polarised light. Such a machine is an excellent testimony to the abilities of Wheatstone the instrument maker.[132]

In Wheatstone's 1843 Bakerian Lecture he had no hesitation in being generous to S.H.H.Christie, and giving him the credit as inventor of the differential resistance measurer, which would later become known as the Wheatstone Bridge. However, one must remember all the occasions on which Cooke claimed that Wheatstone

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[132] One of these instruments can be seen in the Science Museum, London.

failed to give him credit for his part in the invention of the electric telegraph. Similarly, the names of instrument makers were usually left out of his published accounts unless, as with Stroh and the later telegraph patents, they had some technical input to make. Even so, Wheatstone generally viewed makers with respect and appreciated that they required a considerable level of knowledge to do the work which he asked of them. But Charles Wheatstone viewed himself as a member of a scientific elite, and membership of this group was something which he felt that an instrument maker, as an *artisan,* did not possess.

CHAPTER FOUR

CHARLES BABBAGE

The next individual I would like to consider at some length in this thesis is Charles Babbage (1791-1871). Unlike Wheatstone, Babbage had a University education, at Cambridge for three years, though he did not take the Mathematical Tripos at the end of this period. He also differed from Wheatstone in that he did not have an institutional base from which to carry out his scientific work, which he had therefore to finance mainly from his own pocket. While these differences between the two men must be considered major ones, I would like to show in the course of this chapter that their common pursuit of scientific work transcended any such differences and drew them together as members of what they recognised as a significant social grouping. Babbage also represents a particularly important case study for this thesis in that, as with Wheatstone and the electric telegraph, he was involved for the greater part of his career with a practical project - his calculating engines. This means that his relationship with artisans who assisted him on this project may be analysed in depth, as was done with Wheatstone and his assistants on the telegraph in Chapter 3. Such analysis forms a large proportion of this chapter, and in this way Babbage's writings on the position of men of science in society and their relationship to other groups are brought into sharper relief.

Babbage published his *Reflections on the Decline of*

Science in England[1] in 1830, and two years later in 1832 there appeared his mammoth *On the Economy of Machinery and Manufactures*[2], which is often considered to be his only truly complete work.[3] At a first glance it might seem that the two books have little in common, and are the writings of someone we would best describe as a polymath, but if we do this we are distorting Babbage's own motives. In fact, although *Reflections* is a polemic against the British Government and more particularly against the Royal Society, and *On the Economy* is a work of political economy, the two books have the unifying theme of Babbage's attitude to science and its applications, consideration of which eliminates the apparent paradox of his having interests in widely different fields.

Over half of *Reflections* is devoted to an attack on the Royal Society, or rather its ruling party. Babbage was critical of the method of electing Fellows, which, as he saw it, was a matter of knowing the right people. He even claimed that a complete ignorance of scientific matters would not have hindered one's desire to become a Fellow.[4] Babbage also criticised the election of Davies Gilbert as President; someone whom he admired as a gentleman, but whom he did not see as a

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[1] C.Babbage, *Reflections on the Decline of Science in England, and on some of its Causes,* (London, 1830), - hereafter *Reflections.*

[2] Charles Babbage, *On the Economy of Machinery and Manufactures,* (London, 1832) - hereafter *On the Economy.*

[3] P. and E. Morrison, *Charles Babbage and his Calculating Engines,* (New York, 1961), p.xxvi.

[4] *Reflections,* pp.50-2.

distinguished philosopher,[5] and he exposed some inconsistent practices connected with the awarding of the Royal Medals. In this latter case, Babbage himself had been the victim of the Society's transgression of its own rules for the award of the very first Royal Medal.[6]

The other major attack in the book was on the Government, and the lack of support which it offered to men of science. This was in sharp contrast to France, where the cultivators of scientific knowledge could not only derive an income from the State for their work, but could aspire to State honours as well. Babbage proposed the establishment of an Order of Merit as a reward for scientific endeavour in this country,[7] a proposal that was not adopted. Ironically, when Babbage was offered a knighthood in the 1840s, he declined it, presumably because he did not wish to be conflated with those obtaining honours for political reasons.

However, there is another theme in *Reflections* which we can discern in Babbage's other works, and particularly in *On the Economy,* and this is a concentration on the need to make the principles of abstract science available for application. *On the Economy* assumed that the principles were there, waiting for application, though of course it was also the role of the philosopher to find the principles in the first place. In the introductory remarks he stated:

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[5] *Ibid.,* pp.53-6.
[6] *Ibid.,* pp.115-24.
[7] *Ibid.,* pp.198-200.

> ...in England, particularly with respect to the more difficult and abstract sciences, we are much below other nations, not merely of equal rank, but below several even of inferior power.[8]

Babbage regretted that a country so "distinguished for its mechanical and manufacturing ingenuity" should be in such a state with regard to mathematical science.[9] Bearing in mind his Cambridge background, it was not surprising that he made the value judgement that mathematical science was a superior form of knowledge to other forms of science, and also that it was more important because it could be applied to useful ends (such a view of mathematics would have been welcomed by Airy, though not by Faraday and Wheatstone). This tied in with the central theme of *On the Economy.* The object of this book was to ensure that British industry was organised on a scientific basis; its argument being that the "scientific" approach was the only one which could solve the problems of industry. As such, this gave the philosopher a special place in society, and provided him with a legitimation of his activity. This notion of the economic benefit to be gained from the work of the philosopher was a vital one to Babbage, as we shall see throughout the rest of this chapter, both in his work on the calculating engines and in his scientific writings.

At almost 400 pages long, the work was a considerable one. The research was carried out in factories in Britain and on the continent, and Babbage acquired a reputation for listening carefully to what the workmen he met had to say about

[8] *Ibid.,* p.1.
[9] *Ibid.*

the methods they used. His respect for workmen's skills may have gone back a long way, according to Hyman, who argues that Babbage's own mechanical skills might have been inherited from his ancestors, who were goldsmiths (in fact his own father may have been a goldsmith, before he became a London banker).[10] Apparently Babbage had always had an interest in mechanical things - when shown a toy as a child he would not be content until he had ascertained how it worked, and this meant, if necessary, taking it apart.[11] Without wishing to agree with Hyman's style of historiography, I would maintain that Babbage *respected* the skills of the workmen whom he encountered during his researches, and, as we shall see later, he was always willing to give high praise to the skilled artisan.

On the Economy, then, was a work which fully showed the need for the application of scientific principles to industry, and such application would produce wealth for individual entrepreneurs, though more importantly for the nation. It was suggested at the York meeting of the British Association in 1831 that:

> a Report or Essay on the best practical method for making science available for the improvement of the mechanical arts would form an interesting paper for the Oxford Meeting; it was observed that in France the highest scientific acquirements are applied to the most trifling operations while in England the most important works were

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[10]A.Hyman, *Charles Babbage, Pioneer of the Computer,* (Oxford, 1982), p.9.

[11]C.Babbage, *Passages from the Life of a Philosopher,* (London, 1864), p.8. Hereafter *Passages.*

> successfully executed without any apparent reliance on the results of scientific calculations.[12]

Presumably with the publication of a full length book on the subject such an essay became unnecessary. It is interesting to note the favourable reference to the French in the above letter, something which Babbage, having just published *Reflections*, would have appreciated, considering the admiration he showed for the attitude of the French Government towards men of science.

Some emphasis should be placed upon Babbage's concern with political economy as a tying together of his work - that scientific principles should be applied to manufacturing industry to produce wealth. Clearly this was the principal message of *On the Economy*, but it was also implicit in the rhetoric of *Reflections*, which stressed the importance of the man of science and his work to society. Although I argue throughout this thesis for an increasing sense of collective identity among men of science, and emphasise the way in which the group was characterised by the insistence of its members on their value to the State, it must be conceded that Babbage's rhetoric on this point was considerably more strident, indeed vitriolic, than that of the subjects of the other case studies. Babbage also saw the calculating engines as capable of playing a role in economic advance, because of the labour they saved. His views therefore matched those of Adam Smith, whose perception of the importance of the philosopher has been discussed in the introduction to this thesis. Babbage differed from Smith, however, in that he

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[12] British Library Add. Ms. 37186 f.132.

was actually engaged in the pursuit of scientific truth and had a conscious idea of his own value to the State, which he wished to emphasise to those in authority.

This notion of the value of the philosopher to the State as an encapsulation of Babbage's ideology , was to some extent further shown in his attitude to the use of philosophical instruments in scientific investigations. He was part of a successful British Association delegation (Murchison was also involved) which met Thomas Spring-Rice (Chancellor of the Exchequer) and Charles Edward Poulett Thomson (President of the Board of Trade) about the remission of duty on the importation of foreign philosophical instruments. Babbage's motives were fairly clear - if there were foreign philosophical instruments which were not similar to any made in England, or which were of superior quality to those made in this country, then it was desirable that there should be as little restriction as possible placed on the acquisition of such instruments by an English philosopher. A philosopher might then use these instruments to advance his knowledge which ideally it would then be possible to apply for the benefit and wealth of the nation. However it should also be noted that a balance had to be struck, as an unlimited importation of foreign instruments would be detrimental to the instrument making trade in this country, even if principles elucidated using foreign instruments were beneficial to the economy. The representative of the Customs wrote to Spring-Rice citing some of the difficulties that had to be faced, a letter which he passed on to Babbage.[13] The only instruments found in the Table of Duties as it stood were telescopes (at 30% duty), and chronometers (at 25%, the same as watches). All other instruments were charged simply according to the duty on the material of which they were made. Thus sextants made of brass were charged with a duty of 30%, the same as any article made of brass. The representative of the Customs wrote:

> With respect however to the expediency of remitting the duty on Philosophical Instruments I would beg to observe that the words... embrace a variety of manufactures and I should entertain some doubt as to whether the unlimited repeal of duty on such articles would be advisable... It would therefore be desirable to have some specification of the articles more particularly alluded to by Mr.Babbage.[14]

Thus, if Babbage were to name a type of philosophical instrument which was likely to prove useful to the British Association (and thus to science in Britain), then provided its duty-free importation was not detrimental to the instrument making trade in Britain, the remission of duty on it would follow as a matter of course. As far as the makers of the instruments were concerned, it was desirable that they should be British, but if a superior product was to be found in France then there should be no hesitation in using it, as its employment could lead to advances in scientific knowledge, manufacturing technology, and so wealth.

Babbage's concerns with instruments and their makers, and

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[13] R.B.Dean to T.Spring-Rice, 17 April 1837, British Library Add. Ms. 37190 f.108.

[14] *Ibid.*

larger scale projects which we might better describe as engineering, were many and varied, from the difference engine as a major engineering project to the zenith micrometer as a precision instrument. He was capable of building some contrivances himself but in general would employ a maker instead[15] (another manifestation of his belief in economy and the division of labour), and would always have the greatest regard for the skill which the instrument maker or the engineer exhibited in his work.

However, despite this high regard for the level of workmanship displayed by his engineers and instrument makers, he did not include them in the same social grouping as men of science, as will be shown in the remainder of this chapter. The philosopher could become acquainted with the higher branches of abstract, mathematical science on which the artisan's work depended, and so had a higher status than the artisan. Babbage thus saw himself as part of a scientific elite, being versed in mathematical science; this was an elite which he felt ought to be honoured by government, in order that young philosophers might gain an added stimulus to be part of it. However skilled they were, it was not a group to which artisans could belong. Having discussed in the previous chapter how Charles Wheatstone rose to membership of this elite, and how his responses to artisans were conditioned by his perceived status, I would like in this chapter to consider Babbage's relationship with those

[15] See for example *Passages*, p.113 for his attitude towards the employment of makers on the analytical engine project.

outside what he saw as the philosophical community, bearing in mind that, unlike Wheatstone in his early career, he would have considered himself a member of an elite group from his Cambridge days onwards.

1. The Origins of the Difference Engine.

The greater part of Babbage's life was spent in a more or less active attempt to construct a working calculating engine - firstly the difference engine and secondly the analytical engine. He started work on the first difference engine in 1820, in as much as that was the year in which he commenced his construction of the first small model of such an engine,[16] and up to his death in 1871 he was still having new ideas concerning the construction of an analytical engine.

There is a disagreement as to when Babbage first thought of the idea of a calculating machine which could calculate and print tables. In his autobiography he gives the date as 1812 or 1813, when he was sitting in the rooms of the Analytical Society in Cambridge, and another member caught him half asleep over a table of logarithms on which he had been working. Upon the friend asking Babbage what he was dreaming about, Babbage replied "I am thinking that all these Tables might be calculated by machinery".[17] It is worth pointing out that Babbage did not remember this comment (if indeed it occurred), but that the friend, Thomas Romney Robinson, reminded him of it. In the

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[16] *Ibid.*, p.47; see also Hyman, *op.cit.* (note 10), pp.47-61.
[17] *Passages*, p.42.

autobiography he goes on to say that in 1819 he was occupied with devising means for the accurate division of astronomical instruments, and also was speculating about making machinery to compute mathematical tables. On telling Dr.Wollaston of the former plan, he was informed that he was using an old method described by the Duc de Chaulnes. However the idea of using machinery to calculate tables seemed a more promising one.[18]

Passages from the Life of a Philosopher was published some 50 years after the supposed incident in the rooms of the Analytical Society, and a more plausible account of Babbage's first thoughts on the idea of a difference engine may be an earlier account in which he stated that, along with John Herschel, he had been preparing certain tables for the Astronomical Society with the aid of (human) computers. On meeting to compare the results, Babbage and Herschel found many disagreements in the tables, and Babbage expressed to Herschel the wish "that we could calculate by steam".[19] Herschel said that this was quite possible. The date suggested is 1820 or 1821; the earlier of these is supported by the statement elsewhere that Babbage had started constructing the first model of the difference engine in the middle of 1820.[20]

There are many technical accounts of the difference engine, both contemporary and modern, and it is not my purpose to

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[18] *Ibid.*

[19] C.Babbage, *History of the Invention of the Calculating Engines,* 10, Buxton Papers, History of Science Museum, Oxford.

[20] *Passages,* p.47.

describe the mathematical principles on which it is based.[21] The essential point is that it is a machine whose object is to calculate and print tables and which, once started in operation, requires no further human intervention. Thus possibilities of human error do not arise. The idea of using a machine to calculate tables was fully in accord with Babbage's views on economy - it produced both economy of human effort (humans did not have to do the work), and of time (humans did not have to check the results, and the results were produced more quickly than humans could produce them, except in the simplest cases). We can also see that the difference engine was an application of the principles of abstract, mathematical science (the method of finite differences) to an object which would be of benefit to the nation, and this was consistent with the very views he expressed in *On the Economy.* The difference engine can therefore be seen not merely as an encapsulation of Babbage's views on economy, but also of his views on the importance of science to society, a theme which will be implicit throughout my discussion.

Work started on a small model of the difference engine in 1820.[22] Having made various drawings, Babbage himself began to make models of some parts of the machine, but when he ran into difficulties he was quick to employ workmen to help him in

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[21] See for example D.Lardner, "Babbage's Calculating Engine", *Edinburgh Review,* 1834 (59), pp.263-327; J.M.Dubbey, *The Mathematical Work of Charles Babbage,* (Cambridge, 1978), pp.181ff.; also *Passages,* pp.41-67.

[22] *Passages,* p.47.

constructing the parts he needed, as otherwise his invention might have been anticipated by somebody else.[23] Manual skill was something which he shared with the subjects of all my individual case studies (Wheatstone, Airy, and Faraday), but he appreciated the need to devote his time to the theoretical side of the work, so did not make use of his skill extensively. He was not without his difficulties concerning these workmen. One problem which he faced was to cut teeth of a particular form on the wheels on which numbers were to be expressed. Babbage's own lathe was not fit for the job, so he took the wheels to a wheel-cutter in Lambeth, who was left with Babbage's drawing as his guide. On receiving the wheels again, Babbage was disappointed to find that although the wheel-cutter had cut the shape of the teeth "perfectly", they did not work, and he began to entertain doubts as to his own reasoning. The problem was soon solved - the artisan had misunderstood the philosopher's instructions and had cut wheels with the number of teeth which his tools permitted, a number which was not the same as that which the machine required. He was able to arrange for his tools to cut the precise number of teeth required, and according to Babbage, the machine then worked perfectly.[24]

Having received the various parts of the model back from workmen, Babbage tried to put them together with his own hands, and although he failed in his construction of the first framework

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[23] M.Moseley, *Irascible Genius: Charles Babbage, Inventor,* (London, 1964), p.66.

[24] *Passages,* pp.43-4.

which was made of iron and not stiff enough, eventually a model of six figures worked and could print a few simple tables. This was as much as Babbage, with his own means as the source of payment for the artisan, could accomplish.[25] However he recognised that a larger difference engine was a desirable acquisition for the nation and so set out to gain the Government's financial support for its construction. His dealings with the Government over a twenty year period from 1822, when the Royal Society approved the project, Babbage met the Chancellor of the Exchequer, and the Government granted £1500 for a scheme that was expected to take about three years, to its eventual abandonment at the behest of Prime Minister Peel in 1842 after many years of pestering by Babbage for a decision on funding one way or the other, are well known.[26] I would like instead to concentrate on Babbage's relationships with the artisans who attempted to carry out the project for him, as this will shed light on his attitudes to artisans which, as we have seen in *On the Economy,* were generally favourable. Although the discussion in the next two sections will deal only with those artisans who performed the work on the calculating engines, the intention is to use this to build up a picture of Babbage's perception of his position in society, as a philosopher, and of the consequences this had for the status of the instrument maker.

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[25] Moseley, *op.cit.* (note 23), pp.65-6.

[26] See Hyman, *op.cit.* (note 10), or Moseley, *op.cit.* (note 23), for an extended account of these dealings. Also see *Passages,* pp.68-96, which is a statement drawn up by Sir Harris Nicolas from papers and documents in Babbage's possession.

2. The Philosopher and the Artisan: Joseph Clement and the Difference Engine.

Babbage's house in Devonshire Street, London, contained one room which had served previously as a workshop, and continued to do so. Another room was converted into a second workshop, and a third into a forge.[27] The engineer whom Babbage employed to construct the machine, on Marc Isambard Brunel's recommendation, was Joseph Clement.

Born in 1779, Clement had received very little education, learning only basic reading and writing at the village school of Great Ashby, Westmoreland. The rest of his education was a result of his own efforts as he grew older.[28] His father decided that he should become a thatcher, and later a slater, and Clement did work as a slater for five years, between the ages of 18 and 23. During this time it seems he became acquainted with the village blacksmith, and learned to work at the forge, being able to handle tools and, within a short time, even to shoe horses.[29] Clement also had a cousin, Farer, who was a clock and watchmaker, and he lent Clement some mechanics books which encouraged him to pursue the career of mechanic instead of that of slater. However, he continued slating until he had sufficient skill in mechanics.[30] Along with the blacksmith, he made a turning lathe, with which he proceeded to make musical instruments such as

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[27] Hyman, *op.cit.* (note 10), p.53.
[28] S.Smiles, *Industrial Biography. Iron Workers and Tool Makers,* (London, 1863), p.237.

[29] *Ibid.*
[30] *Ibid.*

flutes and clarinets, instruments which apparently he could also play.[31]

These instruments represent an increased level of precision in his work, over the more tolerant occupation of slating. He progressed further, with the aid of the descriptions in the books which he had borrowed from the watchmaker, to make a microscope, and proceeded to make a reflecting telescope as well.[32] By 1804 he had also begun to be interested in screw making, a trade in which he later acquired some fame for his precision.[33] Clement moved from Great Ashby to Kirkby Stephen, and thence to Carlisle. At Carlisle he was employed by Forster and Sons for two years, and then moved to Glasgow to work as a turner, expanding his technical knowledge by taking drawing lessons there from Peter Nicholson.[34] After about a year in Glasgow, he moved to Aberdeen, where he worked at designing, making, and fitting power looms. Importantly, however, he continued to devote himself to the study of practical mechanics, and was able to make some improvements in the tools with which he worked. It is also interesting to note that Clement attended a Natural Philosophy course given by Professor Copland at Marischal College, Aberdeen,

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[31] *Ibid.*, p.238.
[32] *Ibid.*
[33] C.R.Weld, *A History of the Royal Society,* (2 volumes, London, 1848), Volume 2, p.374n.

[34] Smiles, *op.cit.* (note 28), p.239.

in the 1812-13 session.[35] From Aberdeen he moved to London where he was employed by Alexander Galloway, mainly because he could draw. As he was poorly paid by Galloway, he went to work for Bramah of Pimlico, and then became chief draftsman at Maudslay and Field's, where he worked on the early marine engines.[36] After a short period in this sort of employment, Clement began to entertain hopes of setting up on his own account as a mechanical engineer. This he did in 1817. Many of the drawings in the *Transactions of the Society of Arts* from this date are from the hand of Clement. He was also active in the invention of mechanical contrivances, such as an instrument which could draw ellipses of all proportions on paper or copper, for which he was awarded the gold medal of the Society of Arts in 1818.[37]

Clearly then Clement had an impressive pedigree by the time he came to work for Charles Babbage on his difference engine in 1823; as an artisan he was skilled not only in what we would refer to as engineering, and drawing, but also in precision instrument making, as shown by his microscope and reflecting telescope. However, although in the writings of Victorian biographers such as Smiles there was always an emphasis on the *achievement* of the individual who had risen from humble beginnings (and therefore such works should not be taken as

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[35] *Ibid.*, p.240. Copland's extensive use of scientific instruments to illustrate these lectures is discussed in J.S.Reid, "Eighteenth-Century Scottish University Instruments. The Remarkable Professor Copland", *Bulletin of the Scientific Instrument Society*, 1990 (24), pp.2-8.

[36] Smiles, *op.cit.* (note 28), p.243.
[37] *Ibid.*, p.244.

fact), Clement was still seen as no more than an artisan; as Smiles stated:

> He was not educated in a literary sense, for he read but little, and could write with difficulty. He was eminently a mechanic, and had achieved his exquisite skill by observation, experience, and reflection. His head was a complete repertory of inventions, on which he was constantly drawing for the improvement of mechanical practice.[38]

In 1823 Babbage had very little knowledge of engineering, tool-making, or metal-working, and had only the experience which he had gained in his earlier efforts in constructing the first small models of the difference engine. On examining the contemporary workshops, he concluded that many of the tools which would be required to make the precision parts necessary for the difference engine simply did not exist, and he would have to invent them.[39] Clement's workshop, at 21 Prospect Place, Southwark, had only one good lathe when he was first employed by Babbage, but it soon became the leading centre for the development of precision machine tools. Workmen of the highest skill were employed in making these tools and in putting them to use in constructing the various parts of the difference engine.[40]

Initially Babbage's idea had been to use the £1500 from the Government, which he believed they would add to as the work progressed, to complete the engine in two or three years.[41]

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[38] *Ibid.*, p.257.

[39] C.Babbage, *The Exposition of 1851, or Views of the Industry, the Science, and the Government of England,* (London, 1851), p.175. Hereafter *The Exposition of 1851.*

[40] Hyman, *op.cit.* (note 10), p.53.

[41] *Passages,* p.70.

However, as the work continued he found that he was constantly having new ideas, and having to scrap all the work which had been done, in order to incorporate these modifications. This used up both money and time, and no further Government grants were forthcoming. Babbage paid the bills out of his own pocket, and though he pressed the Government, no minute of his conversation with the Chancellor of 1823 had been taken,[42] and so the Government had no obligation to advance any more funds. Work proceeded actively on the difference engine for more than the expected two or three years, and by the time Babbage went on his continental tour of 1827 and 1828 he was already suspecting Clement of using the income which he derived from the difference engine project for construction of lathes for other purposes.[43] It is important to note, therefore, that the difference engine project did not represent the exclusive activity of Clement's workshop, although some correspondence may give the impression that it did. It has been suggested that between one-fifth and one-third of the effort of the workshop was concentrated on Babbage's difference engine.[44]

On returning from the continent in 1828, Babbage applied to the Government for funds again, and with Royal Society approval being gained, a further grant of £1500 was made to enable the machine to be completed.[45] £17000 was eventually to be spent on

[42] *Ibid.*, p.71.
[43] Hyman, *op.cit.* (note 10), p.123.
[44] Morrison, *op.cit.* (note 3), p.xxv.
[45] Treasury Minute of 28 April 1829.

the machine.[46] Babbage was satisfied with the new arrangements, though he was not satisfied with Clement, who had been delivering his accounts with so little detail that it was impossible for Babbage to judge the accuracy of the charges. Clement had built a set of tools for making the parts of the engine and had trained his workmen to use them, and for several years he had been receiving £30 a month from Babbage, in addition to his other expenses. As all the tools were built to new levels of precision, the process was both slow and expensive, and Babbage put down the slow progress to Clement's apparently extortionate charges.[47]

In April 1829 it was decided that Clement's bills should be examined by Government-appointed engineers prior to being paid by the Government. In practice the bills were not actually paid by the Government during this period; in the short term Babbage had to pay Clement from his own means in order to keep him happy, and would later regain the money from the Government. The Treasury were not efficient enough in their payments to satisfy Clement, though it is worth pointing out that Babbage was repaid all the money which he had paid to Clement once the necessary bureaucracy had been dealt with. Two of the engineers who examined the bills were John Rennie and Bryan Donkin.[48] In a bill which Babbage sent to Donkin to be examined in April 1829 he lamented the fact that an item was charged at £77, despite having

[46] *The Exposition of 1851*, p.177.
[47] Moseley, *op.cit.* (note 23), p.113.
[48] Babbage to G.Rennie, 11 April 1829, British Library Add. Ms. 37184 f.252.

been previously agreed on by Babbage and Clement at £50.[49] Clement assured Babbage that £77 was the cost price to himself. Donkin replied to Babbage's letter with the information that the item in question, a drawing board, had in fact cost Clement £50, and that it could be considered Babbage's property at that sum, or Clement would keep it.[50] Babbage hoped that treating the engine as his own private property, as far as Clement was concerned, would keep the charges lower (Clement was likely to overcharge if he knew the Government were paying), but the above incident shows that Clement was not averse to inflating his charges nonetheless. Even so, in the same letter as Babbage was criticising Clement's charges, he was full of praise for his work:

> I believe you are perfectly aware of my feelings for Mr.Clement, he is a most excellent workman and draftsman and ought to be well paid.[51]

Clement's reply to Babbage's charge of the lack of detail in his accounts was that the previous bill of which Babbage was critical was only meant as a statement of what was due and that he would in time furnish him with a full bill.[52] Clement's bill of 29 May 1829, however, contained little more

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[49] Babbage to B.Donkin, 11 April 1829, British Library Add. Ms. 37184 f.253.

[50] Donkin to Babbage, 22 April 1829, British Library Add. Ms. 37184 f.266.

[51] Babbage to Donkin, 11 April 1829, British Library Add. Ms. 37184 f.253.

[52] Donkin to Babbage, 22 April 1829, British Library Add. Ms. 37184 f.266.

than the amount due - the work carried out from 1 January to 9 May 1829 was charged at £730.12.0 for "Labour, Materials, Drawings etc." and the balance due to Clement was £2051.19.6.[53] Babbage did not settle this account promptly, so that Clement wrote on 18 November 1829:

> It is now upwards of six months since you informed me that you should be prepared to settle my account in about ten days, or fortnight. Since that time I have scarce had the pleasure of seeing you. You now impose on me the unpleasant task of demanding the money of you. I therefore request that you will not exceed ten days (from the above date) in settling my account with you.[54]

Clearly there was a certain degree of misunderstanding between the two men, which was compounded by their lack of personal contact. According to Babbage, on receiving the above bill, he submitted it to Rennie and Donkin prior to payment. They said it was not properly made out and asked Clement to give it in detail, a proposition which he agreed to, but did not carry out. After Clement's letter of 18 November 1829 Babbage saw him in person and "he then declined making such bill because it is not the custom of engineers to do so".[55] Personal relations between the two men went from bad to worse, Babbage telling Clement that he thought it best that all correspondence between

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[53] J.Clement to Babbage, 29 April 1829, British Library Add. Ms. 37184 f.291.

[54] Clement to Babbage, 18 November 1829, British Library Add. Ms. 37184 f.419.

[55] *Ibid.*

them should pass through the medium of Rennie and Donkin.[56] Yet on the very same day, in a rather pessimistic letter in which Babbage feared having to give the machine up altogether after some discussion with the Government, he was able to say:

> Mr.Clement has worked well and notwithstanding the fact that I am now dissatisfied with some part of his conduct he ought to be paid liberally.[57]

Work, however, continued on the engine and a suitable place was soon required in which to erect it. There was no suitable building at Clement's premises in Southwark, and with Babbage now having to see more of Clement in person, it became awkward for him having to travel the four miles between their respective residences, and work for long periods at a time to make such journeys worthwhile. This was preferable to making several trips in the day but a much better solution from Babbage's point of view was to erect the engine where it was being made. Therefore he proposed the transfer of tools, drawings, and completed parts to a site in East Street, near his home in Dorset Street, where a fire-proof building was to be erected (it is interesting to note that Rennie and Donkin found out that Clement insured neither his house nor its contents, and wisely suggested that Babbage took steps to insure the Government's property in Clement's workshop).[58] While the arrangements for erecting the fire-proof

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[56] Babbage to Clement, 18 December 1829, British Library Add. Ms. 37184 f.463.

[57] Babbage to Rennie, 18 December 1829, British Library Add. Ms. 37184 f.464.

[58] Donkin to Babbage, 22 April 1829, British Library Add. Ms. 37184 f.266.

building were being made, work continued at Southwark. Clement, being "not a man of large capital",[59] had to be advanced money by Babbage to prevent him discharging his men who obviously had to be paid. In July 1831 Babbage complained to the Treasury that he was in advance £1000, a totally unacceptable state of affairs which the Treasury ought to remedy[60]; their last grant (of £600) had been made on 31 December 1830, and they made the further grant of £1000, what Babbage had effectively asked for, on 14 July 1831.[61] Besides the Government grants totalling £7600 with the inclusion of this latest offering, £36 had been made from the sale of old tools, which may show that Clement found uses for the majority of the old tools in his workshop, as £36 can scarcely represent much of the total value of the tools of which the Government had financed the manufacture.[62]

Babbage gave an interesting account of the arrangements, on Clement's part, for keeping a record of the sums expended:

> ..during about three or four years many men were employed but subsequently only a few occasionally. Books were kept in each room for which to enter the sums paid both for labor and materials. In attending to the construction of the engine many escaped being entered and it appears in some instances that entries have been made without any sums against them. Other books...for each workman

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[59]Babbage to Treasury, 5 July 1831, British Library Add. Ms. 37186 f.5.

[60]*Ibid.*

[61]Treasury to Babbage, 13 July 1831, British Library Add. Ms. 37186 f.14.

[62]British Library Add. Ms. 37186 f.25.

> in which the time he worked was noted each day and his payment at the end of the week. These books have not all been preserved.[63]

So it appears that the method of bookkeeping may have been in a great measure responsible for the lack of detail in the bills, even if Clement did think that it was not the job of an engineer to make a detailed bill. Whatever Babbage's opinion of Clement's bookkeeping abilities at this time, he continued to be full of praise for the standard of work displayed by the artisan. Referring to the difference engine, Babbage stated:

> ...to preserve the life of Mr.Clement is the first necessity towards its completion. It would be extremely difficult if not impossible to find any other person of equal talent both as a draftsman and as a mechanician....[64]

Babbage wrote to Clement on 18 May 1832, in compliance with instructions from the Treasury, asking what arrangements Clement considered necessary for the removal of the tools, drawings, and completed parts of the engine to the new premises in East Street. He also asked for an estimate of the probable expense of the removal.[65] Clement enclosed two memoranda relative to the arrangements needed for the removal, on 6 July 1832.[66] His demands were considerable. Clement demanded that prior to

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[63] Explanation of the accounts relative to the calculating engine, British Library Add. Ms. 37186 f.18.

[64] Babbage's Report on his Calculating Machine, 1830, British Library Add. Ms. 37185 f.264.

[65] Babbage to Clement, 18 May 1832, British Library Add. Ms. 37186 f.400.

[66] Clement to Babbage, 6 July 1832, British Library Add. Ms. 37187 f.4.

removing his tools etc. to East Street he wished to have a lease of the premises for some certain time, say 2, 3, or 4 years "..before the expiration of which I shall not be required to quit the said premises". He also asked to be allowed to carry on any other business of his in East Street to which the tools there might usefully be adapted. Besides financial claims for removing all the tools, and the expense of moving back to Southwark at the end of the work (an expense for which he wished to be paid at the outset), Clement also demanded £660 per annum as compensation for having to run a divided business. His other demands concerned alterations to be made to the house in East Street for the convenience of him and his family. For example the attic should be divided into two rooms, there should be bells to the kitchen in each room, the walls should be papered, and there should be a bell and a knocker on the inner house door.

Not surprisingly the Lords of the Treasury found such demands rather unreasonable, especially considering that a very large amount of profit had been derived by the artisan in constructing the calculating machine.[67] Clement's reaction when his demands were not met was to cease work and to dismiss his workmen, which he communicated to Babbage in a letter of 26 March 1833.[68] He was not only aggrieved that his demands for the arrangements for the removal to East Street had not been met, but

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[67] Treasury to Babbage, 14 September 1832, British Library Add. Ms. 37187 f.136.

[68] Clement to Babbage, 26 March 1833, British Library Add. Ms. 37187 f.453.

was also unhappy that Babbage had refused to settle the latest account because he had not received the money from the Government. Clement pointed out that Babbage, not the Government, was his employer with regard to the machine, and thus he expected to be paid by his employer irrespective of that employer's source of finance. Clement's letter of 26 March was by no means a final withdrawal from the difference engine project, however - he considered that a misunderstanding had occurred and "I hope for the sake of my men and the machine that you will be pleased to take the earliest opportunity of arranging things in a more satisfactory manner". This was certainly attempted, but work was never to begin again on Charles Babbage's difference engine.

On 13 May 1833, Clement wrote to Babbage to suggest that as so many problems had arisen concerning the removal, and "as I am anxious to finish the Calculating Machine", he should be allowed to continue with it under Babbage's direction at the Southwark premises, with the parts, when finished, being removed to the fire-proof building in East Street.[69] Clement also suggested in this letter that his bills be paid direct by Government, rather than go through Babbage. By early June 1833 it seemed as if things were moving once more - the Treasury had been persuaded of the need to approve the removal plan (as otherwise Clement seemed adamant he would not proceed with the work).[70] Also the

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[69] Clement to Babbage, 13 May 1833, British Library Add. Ms. 37187 f.534.

[70] Babbage to Clement, early June 1833, British Library Add. Ms. 37187 f.556.

suggestion that bills be paid direct to Clement by the Government was welcomed. However, on Babbage communicating to Clement the Treasury approval, Clement made it clear that he would not move even a single drawing until his demands were actually paid, and this was a demand to which the Treasury surely could not agree.[71]

Clement's suggestion that Babbage be relieved from the obligation to pay the latest account, i.e. that the Government should pay it direct, really meant little as Clement would only make such an exception when the bill had actually been paid.[72] On 22 July 1833 he also wrote:

> I have stated a proposition to their Lordships for the further construction of the Calculating Machine, hoping it will meet their Lordships' approval and also yours.[73]

So, although work had ceased, Babbage and Clement were both keen that it should begin again. However, the motives of philosopher and artisan for this were rather different, as pointed out by C.G.Jarvis, Clement's head draftsman, a man who hated his master:

> ...the inventor of a machine and its maker have two distinct ends to obtain. The object of the first is to make the machine as complete as possible. The object of the second - and we have no right to expect he will be influenced by any

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[71]Babbage to Donkin, 4 June 1833, British Library Add. Ms. 37187 f.557.

[72]Clement to Babbage, 22 July 1833, British Library Add. Ms. 37188 f.14.

[73]*Ibid.*

> other feeling - is to gain as much as possible by making the machine, and it is in his interest to make it as complicated as possible.[74]

Such a statement embodies one of the distinctions I wish to emphasise in this thesis between the philosopher and the artisan. The philosopher was motivated by a desire to be seen to contribute to scientific progress and the benefit of mankind, and an important part of his code of conduct was an effort to be free from financial greed, although of course the idea that self-aggrandisement might follow from a successful invention was also a motive for the philosopher. As we shall see on many occasions in this thesis, the primary motivation for an artisan was a desire to make money, rather than to contribute to scientific advance. As an artisan, Jarvis was in a position to recognise this, as was Babbage, as an employer of artisans. Babbage was thus able to see himself as of somewhat higher status than one whose goal was making money.

Plans went ahead for the removal. When all the parts of the calculating engine, the drawings, the rough sketches, and the patterns of the engine from which castings had been made (and which would not be required again) had been removed to East Street, Babbage wanted a list of all the property belonging to the Government which was still in Clement's possession.[75] Clement wrote to the Treasury telling them that plans were complete for

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[74] C.G.Jarvis to Babbage, February 1831, British Library Add. Ms. 37185 f.419.

[75] Babbage to Clement, 11 December 1833, British Library Add. Ms. 37188 f.96.

the removal, on 30 January 1834, but he was adamant that he would not move anything until the demands up to 30 March 1833 were fully paid. When that was done he was quite happy to move everything.[76]

By July 1834 all the drawings and parts of the engine were in the fire-proof building, though Clement had taken the tools, which, as their maker, it was his legal right to do. Work on the engine could not continue without a major reorganisation, given that it seemed impossible to effect a reconciliation between Babbage and Clement, and the finances of this would have been considerable. Thus no further work took place.

Babbage always regarded Clement's conduct as deplorable, although, as we have seen, he considered his workmanship to be excellent. In 1864, Babbage wrote in his autobiography of the failure of the difference engine project:

> The first and greatest cause of its discontinuance was the inordinately extravagant demands of the person whom I had employed to construct it for the Government.[77]

But as stressed before, Babbage used superlatives to describe the level of the workmanship:

> It would be extremely difficult to find any other person of equal talent both as a draftsman and as a mechanician.[78]

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[76] Clement to J.Stewart, 30 January 1834, British Library Add. Ms. 37188 f.186.

[77] *Passages*, p.449.

[78] Babbage's Report on his Calculating Machine, 1830, British Library Add. Ms. 37185 f.264.

Clement's skill was recognised by others too; for example the Duke of Somerset wrote to Babbage in 1829:

> I saw Mr.Brunel yesterday, and he spoke with admiration of the accuracy of the workmanship, which, he said, surpassed every thing that he had seen even from the hand of Troughton. This is much to the credit of your workman...[79]

Joseph Clement was not the only skilled artisan to work on the difference engine project. One of his employees was Joseph Whitworth, who was later to be knighted and who was the leading figure in the British machine tool industry of the mid-century. On Clement laying off his men in 1833, Whitworth returned to Manchester, rented a room with steam power, and put up a sign: "Joseph Whitworth, tool-maker, from London".[80] We have already come across C.G.Jarvis, better educated than his master Clement, and who was later to work for Babbage on the analytical engine, along with Creedy, a keen mathematician and analyst.[81] Jarvis was not averse to communicating to Babbage his suggestions regarding the construction of the calculating engine, though he was keen to point out that he always mentioned to Clement any proposals that he was going to put to Babbage.[82] For example in March 1832 he wrote to Babbage concerning what he saw as ill-advised proposals for making the cover of the machine part of the machine itself.[83]

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[79] Duke of Somerset to Babbage, 1829, British Library Add. Ms. 37184 f.337.

[80] "Whitworth, Joseph", in *Dictionary of National Biography.*

[81] Moseley, *op.cit.* (note 23), p.167.

[82] Jarvis to Babbage, September 1832, British Library Add. Ms. 37186 f.345.

[83] Jarvis to Babbage, 19 March 1832, British Library Add. Ms. 37186 f.295.

Then he referred to the "drop-pins", and suggested that it would be better if they operated without springs. So he was advising Babbage on technical matters. Jarvis saw himself as being at least equal in ability to Clement (certainly the drawings which he did for the analytical engine support him in this view) and he entertained hopes of completing the difference engine after Clement had stopped work on it:

> ...one practical mechanic of talent and ability is sufficient to superintend the finishing of the machine, and that talent and ability ought to be possessed by the head of the practical department.[84]

So while Babbage may have had doubts whether he could find anyone else of Clement's ability, Jarvis did not.

It would be misleading to describe the difference engine as an instrument - it was rather a work of precision engineering. However, Joseph Clement, the artisan who was in charge of its construction, had made precision instruments himself, such as microscopes and reflecting telescopes,[85] and it is not easy to make a definite distinction between what constitutes large instrument making and what constitutes engineering. Certainly there were men who classed themselves as instrument makers in the period but who diversified into engineering. For example John Norton, a mathematical instrument maker of the early nineteenth century, took out a patent for a new mill and pump, and another, Peter Burt, took out a patent for an improved steam

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[84] Jarvis to Babbage, 3 May 1833, British Library Add. Ms. 37187 f.520.

[85] Smiles, *op.cit.* (note 28), p.238.

engine in 1827.[86] Rather than saying that Clement was an engineer or an instrument maker, we can more appropriately class him as an artisan. We have seen that his standard of workmanship was very high, and any account of the workings of the difference engine will give a flavour of the technical difficulties that he had to cope with in the construction of that machine.

It would be difficult to deny that Clement possessed very great technical knowledge, but this did not make him a philosopher, and Babbage, who applied the principles of abstract, mathematical science in order to design the difference engine, classed himself as that. The philosopher deserved to belong to a recognised elite, an elite which as far as Babbage was concerned had emerged, and which should be honoured by the Government (which by implication, had not realised this and, like the rest of society, saw it either as still emerging, or else ignored it completely) but the artisan ought only to be praised for the standard of his work, and ought not to be so honoured. After all, as Jarvis pointed out, the artisan's desire was to make money, not to advance knowledge.[87]

After work on the difference engine had ceased, a review concerned with its principles was published by Dionysius

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[86] W.D.Hackmann, "The Nineteenth Century Trade in Natural Philosophy Instruments in Britain", in P.R.De Clercq (ed.), *Nineteenth Century Scientific Instruments and their Makers,* (Amsterdam, 1985), pp.53-91, on p.79.

[87] Jarvis to Babbage, February 1831, British Library Add. Ms. 37185 f.419.

Lardner.[88] On reading this review "Mr.Deacon, of Beaufort House, Strand, whose mechanical skill is well known, made, for his own satisfaction, a small model of the calculating part of such a machine, which was shown only to a few friends...".[89] A difference engine, of the same principle but an entirely different construction to Babbage's, was made by George and Edward Scheutz of Sweden, and was to win a medal at the Paris Exhibition of 1855. "An exact copy of this machine was made by Donkin and Co. for the English Government, and is now in use in the Registrar-General's Department in Somerset House".[90] Ironically the completed portion of Babbage's difference engine was not exhibited until 1862, when it was given a small space 4 feet 4 inches in front by 5 feet deep at the International Exhibition of that year, a fact about which Babbage was very angry, as shown by the chapter he devoted to that exhibition in his autobiography.[91]

Babbage's most cherished ideas of economy would have been fulfilled by the difference engine had it been completed; it would have reduced mistakes, and it would have been economical of human time, as there would have been no need to employ human computers. This would also have saved Government money. But Government money was not wasted on the project. Babbage claimed that it had often been commented that the advances in tool design and manufacturing techniques which resulted from the project

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[88] Lardner, *op.cit.* (note 21).
[89] *Passages*, p.48.
[90] *Ibid.*
[91] *Ibid.*, pp.147-67.

were more than sufficient to justify the Government's expenditure of £17000, and they kept Britain at the forefront of world industrial practice for many years to come.[92] From a personal point of view the difference engine was a failure, but from the political economy point of view so important to Babbage, it was a success. I would like to show in the succeeding sections of this chapter the way in which the concern with demonstrating the importance of his work for political economy pervaded other areas with which he was concerned. As with Wheatstone and the electric telegraph, the work on the difference engine represented Babbage's most public face, but the wider concerns which he displayed in his work on it are reflected in his other interests. In the next section I would like to consider his work on the analytical engine, before going on to discuss those other interests relating to instruments, and his dealings with their makers.

3. The Analytical Engine.

During the time that Babbage was deprived of his tools and drawings by Clement, he had been considering the possibility of constructing a different kind of machine, a machine which would be capable of performing the most complicated operations of arithmetic. Babbage published no account of it himself, but an account of the principles of this analytical engine was published in French in 1842 by L.F.Menabrea, a young military man destined to become Prime Minister of Italy, and a fine mathematician.

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[92] *The Exposition of 1851*, p.176.

Menabrea's memoir was translated into English, with extensive notes, by Ada, Countess of Lovelace, the only legitimate daughter of Lord Byron.[93]

The attempts to construct the analytical engine followed a very different pattern to those to construct the difference engine. Most importantly, Babbage did not try to gain Government support for the new project; his experiences had perhaps taught him that it was futile to attempt to make the Government understand the project, and after all he was still engaged until 1842 in negotiations with the administration as to whether the difference engine was to be proceeded with. So Babbage financed the construction of the analytical engine himself, and work proceeded rapidly in the years following 1834. About 300 drawings came from Babbage's office.[94] His chief draftsman, as we have seen before, was Jarvis, who had at one stage been offered employment elsewhere which "in justice to himself and his family he could scarcely have declined".[95] As always, Babbage was aware of the importance of a good workman, and raised Jarvis' salary to one guinea a day in order to induce him to stay, which he did.

Work proceeded on the analytical engine until around 1847, when the novel techniques that had been developed for this engine suggested that a second difference engine was a possibility. So

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[93] "Sketch of the Analytical Engine invented by Charles Babbage, by L.F.Menabrea. With Notes upon the Memoir by the Translator, Ada Augusta, Countess of Lovelace", *Scientific Memoirs,* 1843 (3), pp.666-731.

[94] Hyman, *op.cit.,* (note 10), pp.166-7.

[95] *Passages,* p.113.

Babbage prepared a new design and a set of drawings for this difference engine no.2, which was to be lighter, simpler, and quicker in action than the first.[96] In fact Jarvis was now so experienced that he was capable of working out the technical details of the notations and of the design, so that Babbage did not have to do this himself.[97] However, neither the analytical engine nor the second difference engine were ever constructed. A concerted attempt was never made to construct either, and it seems that Babbage was well aware that financially it would not have been possible for him to complete such a project. Hyman suggests that all Babbage wanted was to carry out experiments, prepare plans and notations, and establish manufacturing techniques sufficient to satisfy himself that an Analytical Engine could be constructed if anyone chose to do so.[98] An offer of liberal help in constructing an analytical engine as a contribution to pure science was forthcoming from Joseph Whitworth in 1855,[99] which perhaps shows that the engine was seen as a practical possibility if funds were available. After all, having worked with Clement on the first difference engine, Whitworth was in a good position to decide whether an analytical engine was a real possibility, and given that he made the offer of help, it seems that he did consider it a practicable proposition.

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[96] Hyman, *op.cit.* (note 10), p.208.
[97] *Ibid.*
[98] *Ibid.*, p.209.
[99] Babbage to J.Whitworth, 9 July 1855, British Library Add. Ms. 37196 f.266.

Babbage's general attitude to the men who worked on the analytical engine was similar to that towards the difference engine workers. As we have seen with Jarvis, he was ready both to praise the ability of the artisan and to let him carry out some of the technical work himself, as this produced a saving of Babbage's own time. In his autobiography he stated:

> Draftsmen of the highest order were necessary to economise the labour of my own head; whilst skilled workmen were required to execute the experimental machinery to which I was obliged constantly to have recourse.[100]

The theme of economy of effort and time recurred as Babbage stated:

> Having myself worked with a variety of tools, and having studied the art of constructing each of them, I at length laid it down as a principle - that, except in rare cases, I would never do anything myself if I could hire another person who could do it for me.[101]

In other words, Babbage recognised that for maximum efficiency in the fulfillment of his project a division of labour should operate, with the instrument maker providing the manual labour and the philosopher, Babbage himself, providing the more demanding intellectual labour. The idea of division of labour *per se* originated with Adam Smith, but Babbage's work on political economy extended it by including the principle that as the majority of manufacturing processes were made up of components requiring different levels of skill, so maximum efficiency could be attained by dividing the workers into groups according to

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[100] *Passages,* p.112.
[101] *Ibid.,* p.113.

their ability, and assigning the most difficult work to those most able.[102] This was to become known as the "Babbage Principle". The significance of the principle for Babbage as a philosopher was that, although it was usually applied in the context of purely manual processes, there was equally a sense in which it applied to any practical project, so that in a project where intellectual labour was needed along with manual labour, it could be argued successfully that the labour of the philosopher represented the upper level in the hierarchy of degrees of ability displayed. Along with the Smithian concept of productive and unproductive labour, which gave the philosopher a claim to be performing more important work than, for example, a politician or a lawyer, because his work added to the value of the subject upon which it was bestowed, the Babbage principle provided the philosopher with an argument that he was *the* most important menmber of society, at least with regard to national wealth and progress. While it would not be correct to assume that the philosophical elite were so arrogant as to emphasise this constantly, political economy did provide them with a suitable means to articulate their claims to status, when they wished to emphasise that men of science seemed to be neglected by society. Babbage's role in this was of course prominent, though his motivations were shared by the other members of the scientific elite as well.

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[102] R.M.Romano, "The Economic Ideas of Charles Babbage", *History of Political Economy,* 1982 (14), pp.385-405. See also *On the Economy,* pp.175-6.

4. Babbage's Interest in Instruments.

Apart from his work on the difference engine and analytical engine, Babbage did not publish much on instruments and mechanical contrivances, although, as we shall see, he was very much concerned with these. He did produce an article in 1825, published the following year, concerning a zenith micrometer which he had designed.[103] The measurement of minute portions of angular space had usually been accomplished either by the use of a well-divided circle, or by the movement of a wire regulated by a carefully controlled screw. However in Babbage's instrument the accuracy did not depend so intimately on the skill with which the divisions had been made, and he went on to give a description of its construction, which, significantly, included mathematical equations which demonstrated that the arrangement caused the angle read off to be so greatly multiplied "as to equal in accuracy the highest optical means which may be employed to determine it". Babbage recognised the problem that in many situations where "the ratio between the power and the effect produced is infinite", and which could thus be used as magnifiers of small quantities, "the softness of the materials, their flexure, the necessary imperfection of the work" were also magnified, and this could mean that errors crept in larger than the quantity to be measured. However this difficulty, he claimed, did not arise with the zenith micrometer. This instrument, then, represents an early display of his concern with mechanical

[103] C.Babbage, "On a New Zenith Micrometer", *Memoirs of the Astronomical Society,* 1826 (2), pp.101-3.

devices, and the published account of it also furnishes us with an example of his belief in the importance of mathematics in the analysis of the working of such devices. As I have stressed in the chapter on the Royal Society, this increase in the use of complicated mathematics in the analysis of the design and use of instruments was a feature of the early nineteenth century, and meant that the makers of the instruments were unable to contribute substantially to their designs, owing to their relative ignorance of the mathematics required.

Whilst the article on the zenith micrometer was Babbage's first published account of an instrument, his interest in mechanical contrivances and instruments went back much further. We have already seen that as a child he would not be content on receiving a new toy unless he could ascertain how it worked - the first question he usually asked was "What is inside it?".[104] During his boyhood he was taken by his mother to exhibitions of machinery, one of which was by a man who called himself Merlin. After explaining the workings of some of the objects on display to the young Babbage, he took him and his mother up to his workshop where there were more automata to be seen, including two female figures, each about a foot high.[105] Babbage was fascinated by these figures, so much so that he would acquire one of them in later life, when it would become an attraction at his famous parties.[106]

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[104] *Passages,* p.8.
[105] *Ibid.,* p.17.
[106] Moseley, *op.cit.* (note 23), p.35.

This interest in things mechanical, and more specifically instruments, was more formally displayed in a course of lectures on astronomy which he was invited to give at the Royal Institution in 1815, and which included a discussion of the instruments used in astronomy.[107] After a lecture devoted to a discussion of the history of astronomy, Babbage gave the whole of the second lecture over to a description of astronomical instruments. Unfortunately the greater part of the draft of this lecture has been lost and all that remains is a few pages on the sextant, which nonetheless convey what the rest of the lecture must have been like: the emphasis is on the way in which the sextant is economical in as much as the distance of two celestial objects can be ascertained by its use without the trouble of any calculation, and there is also emphasis placed upon the practical import of the instrument, namely that it is "constantly employed by seamen for determining their longitude". The content of the lecture course was set at a fairly popular level, consistent with the ideology of the Royal Institution, and the description of the sextant given was non-technical, so we may assume that Babbage's treatment of the other instruments used in astronomy was similarly orientated. The desire to spread the knowledge of instruments and mechanical contrivances was manifested rather later on in his habit of taking a selection of instruments with him on his travels, particularly in Italy. For example on his first tour he took the parts of an instrument which could be used

[107] British Library Add. Ms. 37203.

"either as a syringe, a stomach pump, or for cupping",[108] and he was certainly not without telescopes and barometers, though this may be less remarkable as these were common impedimenta for a gentleman. At the time of the formation of the British Association, Babbage saw that a similar display of novel instruments would be an interesting feature of its meetings. He wrote to Harcourt expressing not only the wish that instruments be displayed, but also that there be an exhibition of manufactures at each meeting - the concern with the applications of scientific principles that would be expressed more fully in *On the Economy* published a year later in 1832:

> ...it is extremely desirable that every member should be urged to bring with him such portable instruments as he may employ in experimenting, specimens of the results of any experiment, specimens of anything curious from his own district either in nature or art, specimens of any foreign instruments or objects of the above kind, including of course manufactures. Might it not be possible to have an exhibition of manufactures at each meeting?[109]

On a later tour to Italy in 1840, when Babbage took drawings, models, and notations of the analytical engine with him, it is known that he also brought some models and scientific and mechanical instruments to court and showed them to the princes.[110] Babbage pointed out the use and structure of the instruments, some of which "belonged to mechanical art, such as

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[108] *Passages*, p.373.

[109] Babbage to W.V.Harcourt, 31 August 1831, Harcourt Papers, xiii, 239-42, cited in J.B.Morrell and A.Thackray (eds.), *Gentlemen of Science. Early Correspondence of the British Association for the Advancement of Science*, (London, 1984), pp.49-51.

[110] *Passages*, p.304.

patent locks and tools; a few were related to the Fine Arts".[111]

The account of the events of this tour in Babbage's autobiography contains some interesting tales of his dealings with Italian instrument makers. For example he speaks of one man in Bologna who made barometers and thermometers, who "had a very respectable knowledge of such subjects", and "conversed very modestly and very sensibly upon various mathematical instruments". This "humble constructor of instruments" was clearly admired not only by Babbage, but by his fellow-citizens as well, being "known to most of the professors", and it was not thought out of order for Babbage to invite him to his evening party, where various instruments were to be displayed.[112] Babbage's admiration for this humble artisan contrasts with his opinion of the chief instrument maker of Bologna who it seems had pretensions to being a philosopher, rather than a mere artisan:

> ...there was a certain air of presumption about him, which seemed to indicate a less amount of knowledge than I should otherwise have assigned to him.[113]

Babbage asked the maker if he was acquainted with a certain process for punching a hole in glass, to which the maker replied: "Yes, we do it every day". Needless to say, the result of this story is that the chief philosophical instrument maker of Bologna had knowledge of no such process, and "smashed the glass into a hundred pieces". The artisan with higher aspirations was not invited to the philosopher's party that evening, but "the

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[111] *Ibid.*
[112] *Ibid.*, p.380.
[113] *Ibid.*, p.381.

unpretending maker of thermometers and barometers" was present and was well liked by members of university and city.[114]

The favourable attitude to the quality of humility in the artisan is shown further by an appeal which Babbage received from the Watch and Clockmakers' Benevolent Institution in 1832, a fund "...for the relief of industrious decayed mechanics, whose chief recommendation is their good character...to promote the welfare of the humble artisan".[115] Babbage's reply is thoroughly generous to the artisan's skills:

> ...in endeavouring to direct the attentions of all classes of Society for the admirable skill and unrivalled perseverance of the English artisan I hope I have taken full share in pointing out his merits, his difficulties, and his wants.[116]

The pretentiousness of the chief philosophical instrument maker of Bologna can be contrasted not only with the humility of the watch and clockmakers, and the maker of barometers and thermometers, but also with the high praise given to Edward Troughton. For example, there is a discussion in *Reflections* concerned with the adoption of a six-inch circle in preference to larger ones on the advice of Colonel Edward Sabine - advice which Babbage saw as being quite obviously ill-founded.[117] Babbage cites Troughton as one of his authorities for the view that bigger circles must be more accurate:

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[114] *Ibid.*, p.383.
[115] Watch and Clockmakers Benevolent Institution to Babbage, 20 December 1832, British Library Add. Ms. 37187 f.287.

[116] Babbage to Watch and Clockmakers Benevolent Institution, December 1832, British Library Add. Ms. 37187 f.297.

[117] *Reflections*, pp.77-100.

> nor ought the opinion of a Troughton, that instruments of less than one foot in diameter, may be considered "for astronomy, as little better than playthings", to have been rejected without the most carefully detailed experiments.[118]

Marc Isambard Brunel's opinion of Troughton was indirectly stated in a letter from the Duke of Somerset to Babbage, referring to Clement's work on the difference engine:

> I saw Mr.Brunel yesterday, and he spoke with admiration of the accuracy of the workmanship, which, he said, surpassed every thing that he had seen even from the hand of Troughton.[119]

It is worth mentioning that, although Babbage's opinion of Troughton may have been high at this time, the long and acrimonious dispute between Troughton and Simms and James South, concerning an instrument which they had made for South which was apparently seriously defective, caused Babbage's opinion of the firm to deteriorate. He was closely involved in this dispute in favour of South, who was a close friend.[120]

Two areas not normally associated with Charles Babbage are acoustics and optics. However, the fact that he did have interests in these fields is borne out by his concern with acoustical and optical instruments. For example, it is known that he borrowed a kaleidophone from its inventor, Charles

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[118] *Ibid.*, p.98.

[119] Duke of Somerset to Babbage, 1829, British Library Add. Ms. 37184 f.337.

[120] M.A.Hoskin, "Astronomers at War: South v. Sheepshanks", *Journal for the History of Astronomy*, 1989 (20), pp.175-212.

Wheatstone,[121] although Babbage knew of the existence of the instrument long before this. It would be reasonable to suggest that he read Wheatstone's published description of it,[122] but if he did not, then he certainly received a letter from Edward Cowper in October 1827, which gave a description of the structure of the instrument and the effects one could obtain with it. Cowper also suggested that, although he did not know where "this little philosophical toy" was sold, Newman's in Regent Street was a possible outlet.[123]

In optics, Babbage was most notable for his invention of the ophthalmoscope, the instrument through which the back of the eye can be seen. The first idea of an instrument with which one could see the interior of the eye came from William Cumming, a surgeon at the Royal London Ophthalmic Hospital. His work on the luminous appearance of the human eye was read by Babbage.[124] It was stated by Wharton Jones that:

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[121] C.Wheatstone to Babbage, n.d., British Library Add. Ms. 37201 f.586.

[122] C.Wheatstone, "Description of the Kaleidophone, or Phonic Kaleidoscope", *Quarterly Journal of Science, Literature, and the Arts,* 1827, pp.344-51.

[123] E.Cowper to Babbage, 17 October 1827, British Library Add. Ms. 37184 f.89.

[124] W.Cumming, "On a Luminous Appearance of the Human Eye", *Medico-Chirurgical Transactions,* 1846 (29), pp.283-96.

> ...in 1847 Mr.Babbage showed me the model of an instrument which he had contrived for looking into the interior of the eye. The reflector was a small plane glass mirror, with a part of the silvering rubbed off to look through.[125]

In an 1854 article Wharton Jones described the instrument as being an internally blackened tube with a mirror at one end such that the light as it was directed into the object eye, fell on it through an opening in the side of the tube.[126] According to Lyons,[127] Babbage failed, along with Cumming, to bring much attention to the ophthalmoscope because it was regarded as only a toy by those to whom it was shown, and the "principles of the illumination of the depths of the eye had to be rediscovered by Helmholtz". Babbage showed his invention to an English ophthalmic optician who was unable to perceive its value as a research tool, and thus the contrivance was laid aside.

Babbage had an interest in more conventional optical instruments as well as in completely new ones - in particular microscopes. Undoubtedly he would have possessed a microscope or microscopes of his own, and was also interested in seeing new developments. In 1830 he received an invitation to view "half a dozen of the best microscopes in London".[128] Babbage also carried

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[125] T.Wharton Jones, *A Manual of the Principles and Practice of Ophthalmic Medicine and Surgery,* (London, 1847).

[126] T.Wharton Jones, "Report on the Ophthalmoscope", *British and Foreign Medico-Chirurgical Review,* 1854 (14), pp.549-57.

[127] H.G.Lyons, "Charles Babbage and the Ophthalmoscope", *Notes and Records of the Royal Society of London,* 1941 (3), pp.146-8.

[128] N.B.Ward to Babbage, 18 January 1830, British Library Add. Ms. 37185 f.17.

on correspondence with the foremost amateur microscope maker of the day, Joseph Jackson Lister, the father of the famous surgeon and a wine merchant by trade. Lister began experimental optical work in middle life,[129] and was most notable for his investigation of objective lens systems with a view to reducing both chromatic and also spherical aberration in the microscope, an account of which was published in 1830.[130] Turner argues that the position of the microscope manufacturers of London, by the mid-nineteenth century, as the most highly skilled in the world was due in large measure to Lister's empirical work, which resulted in the first design of an optical system that was scientifically based.[131] The status of Fellow of the Royal Society was bestowed upon Lister in 1832, though it is important to remember that he was not an instrument maker by trade, but rather a gentleman with an interest in optics and the development of optical instruments, mainly microscopes, and such a title was rather less remarkable for a gentleman such as Lister than it would have been for a "humble artisan" whose trade was microscope manufacture. No such microscope makers became Fellows in this period.

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[129] E.G.R.Taylor, *The Mathematical Practitioners of Hanoverian England, 1714-1840,* (Cambridge, 1966), p.368.

[130] J.J.Lister, "On some properties in achromatic object glasses applicable to the improvement of the microscope", *Philosophical Transactions of the Royal Society of London,* 1830, pp.187-200.

[131] G.L'E.Turner, *Nineteenth Century Scientific Instruments,* (Berkeley, 1983), p.167. The development of the microscope and the part in this which was played by microscope makers and by Lister will be discussed in more detail in Chapter 8.

It is known that Lister was interested in new technical developments in microscope manufacture. He wrote to Babbage in 1830: "I am much obliged by the invitation to the sight of one of Amici's latest and most improved microscopes...I am very desirous to compare the present state of his instrument with our own".[132] In the same letter he proposed bringing his microscope to Dorset Street so that Babbage could see it. We also know that Babbage invited Lister to his famous parties.[133] The important point to be made about Lister is that he was not an artisan with aspirations to become a philosopher (such as the chief philosophical instrument maker of Bologna whom Babbage ridiculed), but rather he was a gentleman with the time to devote to philosophical inquiries concerning microscopy, and who also possessed manual skill. Thus there is nothing inconsistent about Babbage's opinion of Lister: he was a philosopher, not an artisan, but he happened to possess practical skill as well, in common, as I have argued, with most philosophers of the period. Lister valued Babbage both as a gentleman friend and as a source of general advice on ways in which his instruments might be improved.

As I have suggested, then, Babbage was, in the vast majority of cases, generous in his praise of the quality of the workmanship of engineers and instrument makers. However, we have

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[132] J.J.Lister to Babbage, 2 June 1830, British Library Add. Ms. 37185 f.208.

[133] Lister to Babbage, 1831, British Library Add. Ms. 37185 f.558.

also seen that in the case of the Bologna instrument maker, Babbage did not like artisans having pretensions towards philosophical knowledge. We might then ask to what extent practical men such as Whitworth and the Brunels, to name some of his closer acquaintances who were engineers, and Francis Watkins, to name an instrument maker who corresponded with Babbage, saw themselves as possessing such knowledge, as well as how Babbage regarded their knowledge.

Watkins seems to have made two models to show the principle of the difference engine;[134] these may have been intended for the use of Dionysius Lardner, to help in his writing of his review. In the same letter in which Watkins told Babbage of these models, he said he would be returning Dr.Lardner's astronomical drawings and diagrams, and also the sectional model of the steam engine. In a subsequent letter he enquired of the name of the journal in which he could find an account of an experiment by Plateau concerned with induction,[135] and later on again a letter to Babbage showed that Watkins was interested in mechanical notations of the condensing steam engine.[136] All this correspondence indicated an interest in the more philosophical

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[134] F.Watkins to Babbage, 15 January 1834, British Library Add. Ms. 37188 f.160.

[135] F.Watkins to Babbage, 7 March 1834, British Library Add. Ms. 37188 f.240. Watkins thanked Babbage for his advice in the paper with which this request was concerned: F.Watkins, "On Magneto-Electric Induction", *Philosophical Magazine,* 1835, pp.107-13, on p.108.

[136] F.Watkins to Babbage, 24 May 1844, British Library Add. Ms. 37193 f.69.

aspects of his work (after all, Watkins and Hill were famous for their electrical apparatus, construction of which required a fair amount of theoretical knowledge), but did not make Watkins, in Babbage's eyes, anything other than an artisan who wished to extend his knowledge into areas which men of science such as Babbage saw as their province.

Marc Isambard Brunel, meanwhile, although an engineer, who might have been expected, like Watkins, to have aspirations to status in the scientific community, tended to agree with Babbage's own opinion that only men of science should express opinions on scientific matters. For example in 1831 he stressed the need for the difference engine to be moved to a place where Babbage could oversee the work being done on it, and where men of science could come and give their suggestions.[137] Brunel's famous son, Isambard Kingdom Brunel, also had an interest in subjects outside the traditional boundaries of his discipline. He showed Babbage an experiment concerning the musical note produced by a blow on a bar of iron. Babbage excited Brunel's philosophical curiosity by his reply:

> A blow produces on a bar of iron a musical tone. It also produces magnetism. Both these were known facts. But will that vibration alone necessary for a musical note without the ruder shock produce magnetism? This should be tried by making the iron bar vibrate by some external means without contact by blow, as by a string or a reed placed adjacent to it.[138]

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[137] Notes by M.I.Brunel, 1831, British Library Add. Ms. 37186 f.186.

[138] Babbage to I.K.Brunel, 1844, British Library Add. Ms. 35155 f.155.

However, it was apparent that Babbage regarded himself here as a philosopher, and as such above Brunel in terms of the value of his expertise to mankind. Whitworth, as a practical man (the term *artisan* is somewhat anachronistic for capitalist engineers such as Whitworth), can be seen to have disputed Babbage's implied hierarchy, in that he did not have a high opinion of so-called philosophers who encroached upon what he saw as his area of expertise, just as Babbage, as a philosopher, was not impressed with artisans who tried to be philosophers. Hence to decide which of the two classes was more expert and of more value to the State was, in Whitworth's eyes, rather subjective. He wrote to Babbage concerning the British Association Committee on Standards, which he hoped to see adopt "end measure" for Britain. The Rev. Richard Sheepshanks was one of those to whom Whitworth was opposed - Sheepshanks wanted to adopt "line measure" (measuring between two points *on* a bar, rather than between its ends). Sheepshanks had prepared 30 line standards which he proposed to send to foreign governments, but the philosopher had blundered badly, according to Whitworth:

> It is admitted that each of the thirty standards require to be of a different temperature to be the proper length which does appear to me to be sad bungling...[139]

Babbage would certainly have gone along with Whitworth's criticism of Sheepshanks, as the latter was a great rival of Babbage's, notably in taking the side of Troughton and Simms in

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[139] J.Whitworth to Babbage, 22 February 1853, British Library Add. Ms. 37195 f.254.

the dispute with James South to which I have referred earlier, but the point to be made here is that Whitworth disputed the omniscience of the philosopher in areas of science which were not his concern.

However, in keeping with his general opinion of instrument makers and engineers, Babbage's attitude to Whitworth's skill was thoroughly generous, but Whitworth could only be seen by Babbage as a practical man, not as a philosopher. Babbage refers to Whitworth as a practical man "known to the public for [his] profound skill in mechanical art".[140] On congratulating Whitworth on his knighthood in 1869 Babbage paid him a great compliment: "The art of contriving tools is in my opinion the highest department of practical mechanics".[141] By implication Whitworth was its leading exponent. However he, the Brunels, and Watkins could not claim to be philosophers, only highly skilled practical men, as far as Babbage was concerned. In the final section of this chapter I shall continue the development of this theme of the philosopher's perception of his own status as higher than that of practical men, with particular reference to his potential utility to the State, a theme which will be prominent throughout the succeeding chapters as well, for while the

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[140] Babbage to Whitworth, 25 June 1855, British Library Add. Ms. 37196 f.255.

[141] Babbage to Whitworth, 4 October 1869, British Library Add. Ms. 37199 f.488.

philosophers had emerged as an elite in their own eyes, they still saw themselves as being regarded only as an *emerging* group by society.

5. Political Economy and the Status of the Philosopher.

On the Economy of Machinery and Manufactures was one of the first works in which the vital importance of applying the principles of abstract science to manufacturing industry was stressed. The work made it clear that in the new industrialised Britain it was absolutely essential that such a connection should be fostered and made explicit:

> ...it is impossible not to perceive that the arts and manufactures of the country are intimately connected with the progress of the severer sciences; and that, as we advance in the career of improvement, every step requires, for its success, that this connexion should be rendered more intimate.[142]

The importance of skill in mathematical science to achieve this end was also stressed:

> ...the applied sciences derive their facts from experiment; but their reasonings, on which their chief utility depends, are the province of what is called abstract science.[143]

Hence the most important contribution to the progress of the applications of science would be made by someone who was an expert in the department of abstract science, namely a philosopher (such as Babbage himself), though he acknowledged that the artisan also had a role to play:

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[142] *On the Economy,* p.379.
[143] *Ibid.*

> ...efforts for the improvement of...manufactures ...must arise from the combined exertions of all those most skilled in the theory, as well as in the practice of the arts; each labouring in that department for which his natural capacity and acquired habits have rendered him most fit.[144]

He also admitted that the man of science had important practical information to gain from the great manufacturers, but these admissions notwithstanding, it is clear that, for Babbage, the philosopher's role was the most important one.

The situation which Babbage hoped to create in Britain, of the philosopher playing the key part in the progress of manufacturing industry, was of course already seen as existing in the France which Babbage so much envied, and prior to the publication of *On the Economy,* as I have mentioned earlier in this chapter, it had been suggested at the York British Association Meeting that an essay on the best practical method for making science available for the improvement of the mechanical arts in England was badly needed.

Babbage was still preaching the same necessity of the application of the principles of abstract science some two decades later, when he published *The Exposition of 1851.* We can see in the Preface his concern with political economy again being manifested in his attitude to industry:

> The triumph of the industrial arts will advance the cause of civilisation more rapidly than its warmest advocates could have hoped, and contribute to the permanent prosperity and strength of the country, far more than the most splendid victories of successful war.[145]

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[144] *Ibid.*
[145] *The Exposition of 1851,* pp.xii-xiii.

However it was also made apparent on what this progress in industrial, mechanical arts and manufactures depended - the principles of science provided by philosophers such as Babbage:

> Each of the purely mechanical arts is allied to one or more of the sciences; almost all their various processes are amenable to, and explicable by, known laws...[146]

These published works, *On the Economy,* and *The Exposition of 1851,* separated by twenty years, explicitly gave Babbage's views on the importance of the application of scientific principles to manufacturing industry, views which he held over the remainder of his life as well. The fact that Babbage published a work such as *The Exposition of 1851,* and included his views on the science/mechanical art relationship, showed that the doctrines expounded in *On the Economy* were sadly (as far as he was concerned) not being heeded. Hyman puts the problem down to Herschel's defeat in the Royal Society Presidential Election which was a setback to the reform movement of which Babbage was a part. With Babbage being removed in this way from the centre of power,

> The development of professional science in England took place on the basis of an exaggerated separation between pure science and applied technology, and this separation, which is in strong contrast to Babbage's doctrine of the union of theory and practice, became one of the most marked characteristics of English science.[147]

My argument in this thesis strongly disputes this analysis of the future course of English science. I wish to maintain that the men of science of this period expressed their claims to

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[146] *Ibid.,* p.131.
[147] Hyman, *op.cit.* (note 10), p.102.

membership of what came to be seen as a professional class of "scientists" later in the century by reference to just the sort of relationship between their work and its applications that Hyman assumes was no longer relevant in the "professionalisation" process after Herschel's defeat. Hyman's argument seriously over-simplifies Babbage's own views, which, although favourable to the workmanship of practical men, did not grant them an equal status with men of science whose skills were seen as the most vital link in any chain of the application of pure science to technology. As far as instrument makers were concerned, this implied inferiority was very apparent, as we have seen earlier in this thesis that where an eighteenth-century instrument maker could hope to become a Fellow of the Royal Society, and to publish work on the theoretical innovations he had made in instrument design, by the early nineteenth century such ability in makers was rare. and innovations were generally in terms of materials used and manufacturing processes. One of the main reasons for this was the increasingly mathematical route which science was taking, a route of which Babbage would have approved, and yet which left the makers being unable to contribute to the design of new instruments, as they tended not to have been educated in the new areas.

The makers, then, became separated from the sort of scientific elite which Babbage himself, despite his doctrines of the union of theory and practice, was so keen to cultivate (seeing himself, of course, as a member). Instrument makers began to be regarded as just another type of artisan, who happened to be working in a field where particular precision was required, rather than as craftsmen who made contributions to scientific research, as they were in the eighteenth century. Works by instrument makers were very rare in Babbage's library, while works by men of science concerned with instruments were fairly common, a reflection of the amount of written work done by these two classes. Whilst the greatest proportion of the library catalogue[148] was given over to pure mathematics (61 pages), there were 33 pages of titles on astronomy, 17 pages of titles on mechanics, 13 pages on optics, and 9 pages on electricity. However all the works by instrument makers in the astronomy section dated from the eighteenth century: Ramsden's *Description of an Engine for Dividing Mathematical Instruments*,[149] Bird's *The Method of Dividing Astronomical Instruments*,[150] published in 1767, and his *On Construction of Mural Quadrants*[151] of one year later. On mechanics there was W.J.Frodsham's 1838 Royal Society paper on the vibration of pendulums with different suspending springs,[152] and Thomas Jones' 1817 work on the

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[148] *Mathematical and Scientific Library of the Late Charles Babbage*, (a catalogue).

[149] J.Ramsden, *Description of an Engine for Dividing Mathematical Instruments*, (London, 1777).

[150] J.Bird, *The Method of Dividing Astronomical Instruments*, (London 1767).

[151] J.Bird, *On Construction of Mural Quadrants*, (London, 1768).

[152] W.J.Frodsham, *Results of Experiments on the Vibrations of Pendulums with Different Suspending Springs*, (Read to the Royal Society, 21 June, 1838).

mountain barometer.[153] Whilst the library catalogue is not a very reliable method of ascertaining Babbage's views on the status of works by artisans, it does suggest that he considered these less authoritative than works by philosophers such as himself. It may also be surprising to note that for one who preached the importance of the mechanical arts, there are only 3 numbers of the *Mechanic's Magazine,* and these deal with a mathematical subject - a practical method of forming logarithms.[154]

The idea of the primacy of a scientific elite in Babbage's eyes did not apply only to the status of the philosopher over the artisan; the status of the philosopher as the only person qualified to express opinions on matters of science was also contrasted with the right of a military man such as Colonel Edward Sabine to express such opinions. He had been appointed as one of the Government's scientific advisers, a sad mistake according to Babbage, who discussed the appointment in *Reflections*.[155]

The Act of Parliament which established the Board of Longitude allowed for the appointment of three Royal Commissioners "well versed in the sciences of mathematics, astronomy, and navigation". On the abolition of the Board in 1828, the posts of these three Commissioners were to be retained, and they were to become scientific advisers to the Admiralty. The three men appointed were Young, Faraday, and Sabine, and it was

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[153] Thomas Jones, *A Companion to the Mountain Barometer,* (London, 1817).

[154] *Mechanic's Magazine,* nos.1212-14.
[155] *Reflections,* pp.66-100.

the latter whom Babbage mercilessly attacked in *Reflections.* The thrust of the argument he used depended on the manner in which Sabine had taken the observations contained in some of his work on the pendulum. The agreement which was found to exist amongst each class of Sabine's observations was remarkable, and "unexpected by those most conversant with the respective processes",[156] especially as he was using some of the instruments concerned for the first time in his life. As Babbage ironically commented:

> On whatever subject Captain Sabine touched, the observations he published seemed by their accuracy to leave former observers at a distance.[157]

Sabine's transit instrument, 30 inches in length, performed much better than Kater's 42-inch transit, and Bessel's 60-inch transit made by Fraunhofer. A 6-inch circle which he possessed performed much better than a 16-inch circle. The reason for these performances, to which, seemingly, Sabine was oblivious, was that the instrument's level had been divided wrongly: instead of the divisions being each a second, they were nearer to eleven seconds, so that the apparent accuracy obtained by Sabine was illusory - on correcting the division the deviations became "nearly ten times larger than before".[158] Babbage's argument, in short, was that Sabine was not a scientific man. That was why he could not detect his oversight. The qualities necessary for a

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[156] *Ibid.,* p.78.
[157] *Ibid.,* p.79.
[158] *Ibid.,* p.93.

military commander, according to Babbage, were defects in a man of science:

> If a military chief commit an oversight or an error, it is necessary, in order to retain the confidence of those he commands, to conceal or mask it as much as possible. If an experimentalist make a mistake, his only course to win the confidence of his fellow labourers in science, and to render his future observations of any use, is to acknowledge it in the most full and explicit manner.[159]

As Sabine was not considered by Babbage to have been one of the rare cases in which the force of genius was able in the soldier to rise above his military instincts and contribute to science, he should have kept to his duties as an officer, and left science to the philosophers such as Babbage. The scientific elite not only excluded the artisan, then, but also excluded the pretender from other professions.

It was seen by members of this scientific elite, other than Babbage himself, that scientific research was the highest sort of work to which one could aspire. *On the Economy of Machinery and Manufactures,* whilst a very profound work, could not be classed as scientific research, and as Lubbock pointed out to Babbage, having read the work, although he liked it very much, "to speak the truth I should like to see your name connected with some scientific + original research better".[160] The idea that mathematical science was the most important and impressive form of knowledge to which one can aspire was a feature of

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[159] *Ibid.,* p.58.
[160] J.W.Lubbock to Babbage, 10 February 1832, British Library Add. Ms. 37186 f.248.

Reflections, and also of the *Ninth Bridgewater Treatise,* where for example Babbage expressed the need for subjects such as chemistry to become part of mathematical science:

> Hence the whole of chemistry...would become a branch of mathematical analysis, which, like astronomy, taking its constants from observation, would enable us to predict the character of any new compound...[161]

The philosopher, especially if gifted in mathematical science, was seen as deserving to be encouraged in his pursuits by the possibility of an Order of Merit awarded by the Government. Particularly in *Reflections,* Babbage was an ardent campaigner for such a reward for scientific endeavour. Other members of the elite shared these views, as I will show for example with Michael Faraday, though Babbage, as might be expected, tended to be more vocal in his support for these ideas. If such an award had been granted, it would have given to its holders an explicit membership of the scientific elite whose superior intellectual status and utility to the nation Babbage believed in. As it was we had a Sir Joseph Whitworth, and would have had a Sir Charles Babbage had he not declined the offer of a knighthood (presumably on the grounds that such awards were seen to be in general for political reasons), and practical men were eligible for the same honours as philosophers.

It would not be correct, however, to say that this was far from Babbage's initial aim, as he was always keen to ensure that artisans such as Whitworth and Clement on the engineering side,

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[161]C.Babbage, *The Ninth Bridgewater Treatise,* (London, 1837), p.167.

and makers of precision mathematical, philosophical, and optical instruments, were given full credit for the high level of their workmanship. What should be argued is that despite being the leading campaigner for the application of science to industry, and preaching the doctrine of the unity of theory and practice, Babbage regarded himself as a philosopher, and regarded the artisan as excluded from the scientific elite which he saw as the key contributor to the economic and industrial progress of the nation.

CHAPTER FIVE

GEORGE AIRY AND THE ROYAL OBSERVATORY

In this chapter I shall examine the career of a philosopher educated, like Babbage, at Cambridge in the Mathematical Tripos - George Biddell Airy. Unlike Babbage, however, Airy performed his scientific work with a firm institutional base for much of his career, being in charge of Cambridge Observatory from 1828-35, and of Greenwich Observatory from 1835 onwards. The chapter seeks to illustrate the nature of his relationship with instrument makers, particularly by analysis of his correspondence while at Greenwich, Airy being a most appropriate choice as a case study because his dealings with makers were more extensive than any other man of science in the period. I also aim to show the way in which he perceived his role both as a Government employee and as a member of an elite group in society, and the manner in which this view of his position shaped his activity. Unlike Wheatstone, who could be seen to have attained membership of the philosophical community from humbler origins, Airy's Cambridge education placed him in a context where he could more easily be seen to be part of that community. On becoming Senior Wrangler he would have considered his status as assured:

> On Saturday, Jan.18th, the degrees were conferred in the usual way...I, as Senior Wrangler, was led up first to receive the degree, and rarely has the Senate House rung with such applause as then filled it. For many minutes, after I was brought in front of the Vice-Chancellor, it was impossible to proceed with the ceremony on account of the uproar...[1]

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[1] G.B.Airy, *Autobiography of Sir George Airy, Edited by Wilfrid Airy,* (Cambridge, 1896), p.40.

Such was Airy's recollection of the day in 1823 upon which he proceeded to his Cambridge degree, having entirely distanced all other men of his year in the examinations for the Mathematical Tripos. While it is impossible to dispute the accuracy of the picture of the degree ceremony which he gives, the manner in which it is given provides us with a revealing insight into his character. Airy, born in 1801, entered as a Sizar (i.e. an undergraduate who received an allowance from the college to enable him to study) at Trinity College in 1819, and distinguished himself in mathematics by the end of his first year, to the extent of receiving requests for tutoring from men in the years ahead of him.[2] Circumstances such as this contributed to the formation of a high opinion of his own abilities, to some extent shown by quotations such as the above. He was elected a Fellow of his college in 1823, and became Lucasian Professor of Mathematics in 1826, after an election in which his opponent was Charles Babbage. After holding this chair for two years, Airy offered himself as a candidate for the vacant Plumian Professorship of Astronomy, a post which included amongst its responsibilities the care of the Cambridge Observatory, but at the same time he made it known that he would not accept the post unless he were granted an increase of salary over the £300 per annum which was then allotted to the Professorship.[3] This was generally agreed to by the electors, and on February 6th 1828, Airy became Plumian Professor.

Such determination is characteristic of Airy even in these

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[2] *Ibid.*, p.28.
[3] *Ibid.*, pp.79-80.

early years. He was aware that an institution such as the Cambridge Observatory needed someone of an established scientific background and with considerable philosophical expertise to pursue scientific research there and to run it efficiently, and he saw himself as such a person: being a Senior Wrangler, and such a convincing one at that, was impeccable proof of his pedigree. When he was offered the position of Astronomer Royal in 1835, he held out again for an increase of salary, demanding £800 p.a. as compared with the £600 p.a. he was then earning at Cambridge, if he was to move.[4] Again he was successful in his demands; Airy, for his part, had increased his personal income so that he and his family might live more comfortably, whereas the Admiralty (as the Astronomer Royal's employer) had secured a man with the philosophical expertise to control an establishment whose successful running was essential to the nation as a whole.

1. Political Economy and The Royal Observatory, Greenwich.

The Royal Observatory, Greenwich, was originally founded, and was rigidly maintained, certainly up to Airy's time, for a strictly practical purpose - to assist navigation. Walter Maunder, who served as an assistant at the Observatory in the latter part of the nineteenth century, declared that without the Royal Observatory and its contribution to facility of navigation, "Great Britain could never have obtained her present commercial position and world-wide empire".[5] The Observatory had been

[4] A.J.Meadows, *Greenwich Observatory. Volume 2: Recent History (1836-1975),* (London, 1975), p.1.

[5] E.W.Maunder, *The Royal Observatory, Greenwich,* (London, 1900), p.16.

founded in response to a concrete problem - the finding of one's longitude at sea, an issue that was of crucial import to the nation, unlike some of the problems of abstract science which were being pursued in other observatories by the mid-nineteenth century - for example investigating the cloud changes on Jupiter or the constitution of nebulae.[6] Airy kept this point firmly in his mind when he took up the post of Astronomer Royal in 1835. While areas of inquiry such as the above may have interested him as a man of science, he was aware that he was a government employee and that his duty in this capacity was to pursue only those lines of investigation which would assist in the original purpose of the Observatory. Airy always kept this duty to his employer at the front of his mind when proposals were made to extend the work at Greenwich into new areas.

Although the staple work of the Royal Observatory on Airy's succession to the Astronomer Royalship was astronomical, with observation of the Moon as the first object, the regular observation of planets as the next, and the observation of extensive numbers of catalogued stars as the third object, he was quick to extend the work of the Observatory by the proposal for the establishment of a Magnetic and Meteorological Department. This suggestion was made to the Board of Visitors early in 1836,[7] and while such investigations did not lie within the original

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[6] For a general overview of nineteenth-century astronomy, see A.M.Clerke, *A Popular History of Astronomy during the Nineteenth Century*, (Edinburgh, 1885).

[7] G.B.Airy, *Report of the Astronomer Royal to the Board of Visitors, 1836*, (London, 1836).

purpose of the establishment, magnetic and meteorological studies were so intimately connected with this purpose, from their importance in navigation, that there was no question as to the suitability of such a department for the Observatory. The role of the Magnetic Observatory was to observe changes in the earth's magnetism, both its force and its direction, a problem of the utmost importance for ships using magnetic compasses, especially with the advent of iron ships. A Meteorological Department was not only necessary for the obvious purpose of observation of atmospheric movements with the object of forecasting potentially dangerous storms, but it was also necessary in astronomical terms, as a knowledge of atmospheric pressure and temperature is required in order to correct astronomical observations for the effect of refraction.[8] So from the point of view of benefitting the State, economically and politically, the creation of such departments was only natural in the nation's first scientific establishment.

It is important to realise that, as with astronomy, the reduction of the magnetic observations was as necessary a skill as the making of the observations themselves. Indeed, with astronomical and magnetic observations, Airy considered that the work could be done by anyone. Rather more skill was needed for reduction, whose ultimate purpose was to reduce the phenomena to their physical causes, certainly a task for the philosopher, though once the basic pattern had been set on which the

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[8] See Maunder, *op.cit.* (note 5), pp.228-50, for a semi-technical discussion of the Magnetic and Meteorological Department and its work.

reductions were to be made, they too could be accomplished by human computers of limited ability.

Airy's proposal for the establishment of the "Mag and Met" Department, as it was to become known, was sanctioned in 1837, and the first magnet was suspended on 21 May 1838. By 1840 a system of regular two-hourly observations was under way.[9] Later in his career, Airy founded other departments whose connection with the original purpose for which the Observatory was established was not so explicit, for example the Spectroscopic Department and the Heliographic Department. Airy considered originally that spectroscopy could not be introduced to an institution whose purpose was to ascertain the movements of the heavenly bodies, rather than their constitution, which was of no interest from a navigational point of view. However, with Sir William Huggins' investigations of the blue and red shift of stars, it was shown that the spectroscope was of use in determining stellar motions and therefore could legitimately be used in the Royal Observatory. Thus Airy's reservations were eliminated and the new department was founded in 1873.[10] The Heliographic Department had as its justification the belief that there seemed to be a causal connection between the appearance of sunspots and the magnetic intensity on the earth, a connection which if rendered explicit could provide vital information for practical use in navigation.[11]

The guiding principles in Airy's introduction of these new areas of inquiry - magnetical, meteorological, spectroscopic, and

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[9] *Ibid.*, pp.112-13.
[10] *Ibid.*, pp.268-71.
[11] *Ibid.*, pp.251-2.

heliographic, were that the original purpose of the Observatory - to aid navigation - should take precedence over the pursuit of abstract scientific problems, and secondly that the "cause of science" always be promoted, provided of course that it was science with a potential practical application. For example he wondered if "the cause of science might not gain if... the higher branches of mathematical physics should take their place by the side of Observatory routine",[12] indicating that although he still kept the original purpose of the institution firmly in mind, he felt that it was more important that the Observatory be willing to modify its activity if this could be seen to provide advantage to the nation.

The concern with political economy and advantage to the State in general is a paramount theme in perhaps the best known of the Greenwich departments - the Time Department. In the early years of Airy's tenure as Astronomer Royal a considerable amount of Observatory business involved the rating of chronometers for the Admiralty,[13] it being essential to know accurate time in order to determine one's longitude at sea (one could determine local time by star observations, and with a knowledge of Greenwich time, from an accurate chronometer, one could determine longitude). In fact Airy held that too great a proportion of the Observatory's effort was being made in this elementary branch of science, chronometer rating, and not enough

[12] G.B.Airy, *Report of the Astronomer Royal to the Board of Visitors, 1870,* (London, 1870).

[13] Airy, *op.cit.* (note 1), p.124.

was being devoted to astronomy.[14]

During Pond's reign as Astronomer Royal, in 1833, a time ball was erected, which was dropped by hand at exactly one o'clock each day. This apparatus, constructed by the eminent engineering firm of Maudslay and Field, was modified in 1852 at Airy's suggestion so that the moment of drop be controlled electrically to coincide exactly with the time at which the master-clock in the Observatory reached one o'clock. The Greenwich master-clock was also connected with a time ball on the offices of the Electric Telegraph Company in The Strand, first dropped on 28 August 1852, and one at Deal, Kent, first dropped on 1 January 1855, from which passing shipping could adjust their chronometers.[15]

Airy often stated that he regarded it as the duty of the national observatory to provide Greenwich time to the nation.[16] The procedure on the part of the Observatory was firstly to find the time by astronomical observation using Airy's newly-erected transit circle on certain stars, secondly to correct the standard clock in accordance with the time found using the transit circle, and thirdly to send out the time signal, hourly. Such a procedure involved a simple application of scientific ideas, but its value in real terms to the nation, especially to the railways and to business in general (most large towns were receiving time signals

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[14] *Ibid.*

[15] D.Howse, *Greenwich Time and the Discovery of the Longitude*, (Oxford, 1980), esp.pp.94-9 on time balls.

[16] *Ibid.*, pp.89-92, discusses the start of the Greenwich time service.

by the 1860s), was considerable. As Airy declared in his autobiography:

> I cannot but feel a satisfaction in thinking that the Royal Observatory is thus quietly contributing to the punctuality of business through a large portion of this busy country...[17]

Airy saw himself as the main agent of this benefit to the nation, and while he is often regarded as selfish and arrogant,[18] it is undeniable that he always considered himself as a government employee with a duty to apply his philosophical expertise for the good of his country. In this chapter it will be argued that Airy saw his philosophical expertise as of considerably more benefit in terms of the generation of national wealth and prestige than was the effort of men such as instrument makers, whom in general he regarded as not much more than workmen carrying out a job, however skilfully. In the rest of this chapter I would like to demonstrate the way in which Airy was able to use his institutional bases to articulate his claims to intellectual authority and to emphasise the importance of his work to the nation.

2. Airy and the Utility of Philosophical Knowledge.

On succeeding to the position of Astronomer Royal in 1835, Airy could claim to be the only man of science employed by a Government department. Besides his official duties as Astronomer Royal, however, he was always keen to help the nation in regard to scientific matters in which he knew his knowledge was more

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[17] Airy, *op.cit.* (note 1), p.216.
[18] See Maunder's discussion of Airy's treatment of his employees in Maunder, *op.cit.* (note 5), pp.116-18.

considerable than that of any other government employee on whom they could call. Thus it is fair to say that he attempted to carve out a role for himself as the Government's, indeed the nation's, chief scientist. For example, at the behest of Thomas Spring-Rice, the Chancellor of the Exchequer, he served on the Weights and Measures Commission in which his recommendation that the yard should be defined by the distance between two marks on a standard bar was adopted,[19] and he served, in 1845, on the Railway Gauge Commission, which took up a considerable amount of his time away from the Observatory.[20] Airy was also called upon, in 1842, to advise the Government on the wisdom of expending further sums of money upon Babbage's difference engine: "I replied, entering fully into the matter, and giving my opinion that it was worthless".[21] By creating this role for himself as the Government's chief scientist, Airy underlined the importance of philosophical knowledge, and of its possessors, to the nation.

It is important to realise, however, that for Airy, as for the other philosophers discussed in this thesis, the only philosophical knowledge which could be of any economic and political value was that which had practical applications - mere theoretical investigations were worthless (though studies which might in the future be practically applicable could also be considered to be important - hence Faraday's usual reply when asked what use such studies were: "What is the use of a baby?"). Airy's position here is interesting in that there were many parts of his education in the Cambridge Mathematical Tripos which

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[19] Meadows, *op.cit.* (note 4), pp.107-8.
[20] *Ibid.,* pp.108-9
[21] Airy, *op.cit.* (note 1), p.152.

involved very little in the way of potential practical application, and in fact this dislike of mere theoretical problems and investigations "put him continually in dissent with some of the resident Cambridge mathematicians".[22] As his son mentioned, "...every subject of a distinctly practical nature, which could be advanced by mathematical knowledge, had an interest for him...".[23]

An analysis which misunderstands Airy's own motives in this area is by Mayr, and concerns the subject of speed regulation.[24] In Airy's case, the interest in speed regulation had to do with the problem that equatorial telescopes, to remain focussed steadily on a star, had to rotate at a constant speed around the polar axis. Mayr argues that involvement of men such as Airy with problems like this is "remarkable" as we would expect them to incline towards the "pure" side of science. He also argues that it is "curious" that those such as Airy were not involved with these problems exclusively on a theoretical level, but actually contributed on the level of concrete invention as well.[25] The article fails entirely to place Airy in context - he did not see himself as a "pure" or "theoretical" scientist, but rather as someone with philosophical knowledge who could and should use that knowledge in any practical situation where there was a demand for it. If this is borne in mind there is nothing

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[22] "Airy", in *Dictionary of Scientific Biography.*
[23] Airy, *op.cit.* (note 1), p.3.
[24] O.Mayr, "Victorian Physicists and Speed Regulation", *Notes and Records of the Royal Society of London,* 1971 (26), pp.205-28.

[25] *Ibid.,* on p.205.

unusual in Airy's involvement in such areas.

Some have gone so far as to describe Airy as "an engineer by instinct", who "only placed complete trust in theory after it had been shown to work in practice".[26] It is certainly true that he was on friendly terms with the leading engineers such as Isambard Kingdom Brunel and Robert Stephenson, and also his liking for engineering and practical contrivances was manifested in many of his inventions which were spread around the Observatory:

> ...doors which shut by contrivances of his own, arrangements for holding papers, for making clocks go simultaneously, for regulating pendulums, for arranging garden beds, for keeping planks from twisting, for every conceivable thing from the greatest to the smallest.[27]

However these interests merely emphasise that he was always interested in applying his philosophical knowledge, rather than meaning that he was more of an "engineer" than a "scientist".

Airy was closely involved with the Great Exhibition of 1851, particularly with astronomical instruments and philosophical instruments generally, and in his dealings concerning the classification of the instruments there we can see how highly he viewed the need for a *practical* classification. Two possible classifications were sent to him for perusal. One divided the instruments as follows:

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[26] A.Chapman, "Sir George Airy and the Concept of International Standards in Science, Timekeeping, and Navigation", *Vistas in Astronomy* 1985 (28), pp.321-8. See also A.Chapman, "Science and the Public Good: George Biddell Airy (1801-92) and the Concept of a Scientific Civil Servant", in N.A.Rupke (ed.), *Science, Politics and the Public Good. Essays in Honour of Margaret Gowing,* (London, 1988), pp.36-62, esp.p.40.

[27] J.Stuart, *Reminiscences,* (London, 1911), p.159

A. Astronomical and Subsidiary Instruments
  1. Astronomical + Mural Circles, Meridional Instruments... Zenith Sectors...
  2. Altitude and Azimuth Circles... Globes, Orreries
B. Astronomical and Other Instruments used for Nautical Purposes, for Measuring Depths, Velocities etc.
  1. Sextants, Quadrants etc.
C. Instruments applied to Trigonometery, and Land Surveying and Levelling.
  1. Theodolites, Circumferentors...
  2. For Drawing, Delineation, and Plotting
  3. For Measuring Surfaces...
D. Optical Instruments
E. Meteorological Instruments
F. Electricity
G. Magnetism
H. Instruments relative to Caloric...
...K. Hydrodynamics and Pneumatics...
...N. Acoustics[28]

This classification was preferred by Airy to a clumsy classification which grouped the instruments according to whether they measured positions, or forces, or quantities, and so on. The latter classification, drawn up by Wheatstone, was regarded by Airy and Lt.Col.J.A.Lloyd (the author of the former) as ultra-scientific,[29] and not suited to the practical demands of the Exhibition. Although Airy preferred Lloyd's classification,

> ...even it does not perfectly please... [My proposed order] is not founded on any *a priori* philosophical principles, but simply on this consideration - that a manufacturer would expect to sell to one person the apparatus I have placed in juxtaposition - and that a buyer would expect to find them at one shop.[30]

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[28] J.A.Lloyd to Airy, 4 February 1850, Royal Greenwich Observatory, Airy Papers, file 441, leaf 106. Hereafter abbreviated to, for example RGO6 441 f.106, in accordance with the Observatory's own classification.

[29] Lloyd to Airy, 8 February 1850, RGO6 441 f.111.

[30] Airy to Lloyd, 6 February 1850, RGO6 441 f.109.

So Airy's classification was based on a thoroughly practical (indeed, commercial) consideration, rather than what might be a more difficult to grasp theoretical one, and ran thus:

> 1. Miscellaneous Optical Instruments, excluding Telescopes and Microscopes
> 2. Telescopes and Microscopes
> 3. Fixed Astronomical Instruments
> 4. Instruments for Nautical Astronomy
> 5. Survey Instruments
> 6. Draftsman's Instruments
> 7. Time-measurers
> 8. Instruments for the measure of gravity
> 9. Instruments for weights and measures
>
> Then the printed paper [Lloyd's] beginning at E.[31]

This attitude to a classification that was fine in theory but impractical, namely Wheatstone's, is mirrored in the one-time Senior Wrangler's attitude to mathematics. Airy saw mathematics as no more than a useful machine for the solution of practical problems and the arrival at practical results. Whilst he had a profound respect for pure mathematics and algebra, this only went so far as they were able to help him in solving problems in practice. Although some mathematicians, such as Cayley, regarded mathematics as justified in itself simply because it was a useful training for the mind, Airy was unable to accept that mere elaborate mathematical investigations without any practical application could be worth pursuing.[32]

It is important however not to stress Airy's admiration for practical things too much, for the use of mathematics was a key criterion for giving a particular subject area the status of a science. Subjects that had that status, rather than a rule of thumb empirical basis, were of greater value to the nation,

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[31] Airy to Lloyd, 6 February 1850, RGO6 441 f.110.
[32] Airy, *op.cit.* (note 1), p.273.

simply because a firm theoretical foundation was a safer basis for action than mere conjecture. Thus Airy held meteorology in low esteem as a branch of science. As far as he was concerned, until a firm theoretical basis for the subject could be established, much of the effort put into the observations in the Meteorological Department was bound to go to waste. He did accept that useful empirical results could be derived from the local statistical regularities which might emerge,[33] but for meteorology to be a truly worthwhile pursuit for the nation's chief observatory it was desirable that it be established on a philosophical basis, and this would have to be achieved by philosophers, rather than observers not versed in philosophical principles. In the next section I would like to consider Airy's activity after graduating as Senior Wrangler, when he was still without a specific institutional base but was in a position to articulate his claims to intellectual authority, before going on to consider how his perceived membership of an elite group and his strong sense of duty to his country and employer (when in charge at Greenwich) affected his treatment of others.

3. Airy as a Member of a Scientific Elite.

Before he graduated in 1823, Airy had begun to pay some attention to astronomy, which he first considered from an optical standpoint. His interest in optics predated his arrival at Cambridge - for example he read some lectures on optics which his uncle possessed.[34] As Airy related in his autobiography:

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[33]Meadows, *op.cit.* (note 4), p.103.
[34]Airy, *op.cit.* (note 1), p.18.

> The acquisition of an accurate knowledge of the effect of optical constructions was one of the most charming attainments that I ever reached. Long before I went to College I understood the action of the lenses of a telescope better than most opticians.[35]

Airy made a telescope with his own hands at Colchester, had a stand made for it by a carpenter at Cambridge, and he made repeated observations of Jupiter and Saturn with it during his undergraduate days.[36]

On 13 July 1822, Airy drew up an article about his plan for correcting both spherical and chromatic aberration in a telescope. This was to be his first printed paper,[37] though characteristically, before actually allowing it to be printed and acknowledging that he placed full trust in his theory, he arranged a practical trial of the scheme. This brought him into correspondence with Robert Bancks, an optician trading at 441 Strand, London, who constructed the optical part, which Airy tried, but was not satisfied with. He believed the fault depended in some way upon the crystallisation of the mercury silvering.[38] The lenses had been completed by 4 October 1822,[39] with the exception of two that required silvering. From a comment in Bancks' letter to Airy of 1 December 1822 it is clear that not only had Airy complained strongly about a defect in one of the

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[35] *Ibid.*, pp.18-19
[36] *Ibid.*, p.30.
[37] G.B.Airy, "On the use of Silvered Glass for the Mirrors of Reflecting Telescopes" (1822), *Transactions of the Cambridge Philosophical Society,* 1827 (2), pp.105-18.

[38] Airy, *op.cit.* (note 1), p.38.
[39] R.Bancks to Airy, 4 October 1822, RGO6 805 f.5.

lenses, he had adopted a superior manner, with this, the first London instrument maker that he employed:

> ...I feel some difficulty in replying in the same scientific manner you do. My best way is to be frank, and plead ignorance, but what I cannot do scientifically, I will endeavour to do ingeniously...[40]

Bancks went on to describe the troublesome process which he, as a first-class optician, used to produce perfect lenses, but he was at a loss to explain,

> ...how the circular scratches arise, when the lens is finished horizontally, and the strokes carried hundreds of times in every possible direction over its surface... As I expect Sir to be in the hands of a Gentleman and Scholar of very superior education and ability to myself, so let me hope all errors will be overlooked and obliterated...[41]

Such correspondence would have done nothing to lower Airy's own opinion of his status as a philosopher, of superior knowledge to a mere instrument maker. For Bancks' part, this subservience would not cause any lasting pain as it was merely good business practice to compliment one's customers in such a way. However the problem with the lenses was not solved either by philosopher or by artisan, and Airy was advised by the noted amateur astronomer James South to write to Tulley, another celebrated practical optician, who made him some new reflectors, but practically, no success was achieved, and he abandoned the project, never to resume it again.[42]

It was James South who gave Airy his introduction to practical astronomy late in 1823. Airy was invited to London by South and had a little practice with some of South's own

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[40] Bancks to Airy, 1 December 1822, RGO6 805 f.9.
[41] *Ibid.*
[42] Airy, *op.cit.* (note 1), p.38.

instruments.[43] However, besides these optical and astronomical interests it is evident that he was developing interests in other areas as well, and for example ordered a wide variety of instruments from W.and S.Jones in 1827[44] (whilst Lucasian Professor, so they may have been used to illustrate lectures). W.and S.Jones point out on their bill head that all instruments of their manufacture are "of the best possible workmanship and constructed after their genuine theories". The bill for £24 which Airy received includes, for example, a box wood jointed scale thermometer at 15s., a large size best Phantasmagoric lantern at £3 13s.6d., an hydrostatic paradox at £4 14s.6d., and a large model of a diving bell at £1 8s.. Given the content of the Cambridge Mathematical Tripos, it seems that at least some of these instruments will have been for Airy's private use.

The correspondence with Bancks shows that Airy, even before graduating as Senior Wrangler, regarded himself as a member of an elite, and that Bancks, at least outwardly, acknowledged this. When he became Astronomer Royal, not only did Airy continue to see himself as a member of such a philosophical elite, he also wished to employ members of that group as his immediate subordinates. For example, the First Assistant, in his opinion, ought to be a man capable of running the Observatory in the absence of the Astronomer Royal, and would thus necessarily have to be a man of science.[45] In addition a particular problem for the First Assistant was that he might be susceptible to monetary

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[43] *Ibid.*, p.54.
[44] W.and S.Jones to Airy, 29 September 1827, RGO6 805 f.30.
[45] Meadows, *op.cit.* (note 4), p.2.

corruption by makers of chronometers who sought an Admiralty Commission. So it was partly for this reason and partly to "raise the tone of science" in the establishment that a man of high mathematical ability i.e. a Cambridge man, was selected - Robert Main of Queens' College.[46] However, other assistants at the Royal Observatory did not have the pedigree of Airy and the First Assistant. James Glaisher, for example, who became the first Superintendent of the Magnetic and Meteorological Department in 1838, had worked his way through the ranks of the Observatory; as William Simms wrote to Airy at Cambridge Observatory in 1832:

> I have heard through my brother at the Royal Observatory of a young man, Mr.Glaisher, who, if report says true, is I think calculated to make you a very careful and industrious assistant - he has qualified himself as a computer by pure perseverance, and is now employed in that capacity by Mr.Richardson...[47]

The other employees at the Observatory were no more than "mechanical drudges",[48] Airy and Main being the only men with a Cambridge education and consequently a philosophical mind.

It would be wrong, however, to think that Airy considered himself as an authority on *all* scientific questions. He had a strong sense of his limitations in any field which other men had made their explicit subject of study. Such was the case with Faraday and electricity and magnetism - Airy regarded Faraday as an unofficial elite adviser on these subjects. For example, he corresponded with Faraday concerning the initial setting up of the Magnetic Department, asking whether the Royal Institution had

[46] Airy, *op.cit.* (note 1), p.109.
[47] W.Simms to Airy, 9 December 1832, RGO6 806 f.43.
[48] Maunder, *op.cit.* (note 5), p.117.

any powerful horseshoe magnets which could be lent to magnetise large iron bars for the Observatory.[49] Throughout the early years of the Magnetic Department Airy sought Faraday's advice on practical matters of magnetism, and as if to prove his acceptance of Faraday's position as elite philosopher in this and other areas, he wrote to him in 1853:

> You know that in all matters magnetical, meteorological and chemical, I consider myself and Co. here as mere machines, fit to act up to other people's ideas, but having no ideas of our own..[50]

Needless to say, in Airy's case, these remarks did not extend to astronomy and optics in which he regarded himself as a member of the philosophical elite.

4. Astronomy at Cambridge: the Northumberland Equatorial.

As the Professor in charge of the running of Cambridge Observatory, Airy had come to possess the institutional justification for his claims to authority on astronomical and other scientific matters. The work which he did to equip the Observatory during his time in charge also provided him with his first really extensive collaboration with instrument makers. These collaborations set the pattern for his dealings with such artisans in the rest of his career, the only difference at Greenwich being that he became even more involved with instrument makers on a day to day basis. It is therefore important that his

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[49] Airy to M.Faraday, 2 December 1835, Faraday Papers, Institution of Electrical Engineers, cited in L.P.Williams (ed.), *The Selected Correspondence of Michael Faraday,* (2 volumes, London, 1971), pp.298-9.

[50] Airy to Faraday, 25 October 1853, Faraday Papers, Institution of Electrical Engineers, cited in *ibid.,* p.696.

original involvement on large astronomical instruments, i.e. those at Cambridge, be considered here. Until 1831 Cambridge Observatory was only equipped with one large instrument - a 10 foot transit instrument. In 1831 Thomas Jones mounted an equatorial which he had been commissioned to make, and in January 1833 a mural circle by Simms came into operation.[51] The most significant addition made to the Observatory instruments during Airy's time as Plumian Professor was the Northumberland Equatorial which he designed. An equatorial telescope, generally speaking, is one that rotates about a polar axis parallel to the Earth's axis and with which one is able to follow the apparent motion of the heavenly bodies by rotation about this axis. Airy was first informed of the Duke of Northumberland's interest in providing an object-glass for such a telescope in August 1833, by John Herschel.[52] The object-glass was one of 12 inches aperture made by Cauchoix, one of the leading French opticians.

Although the instrument was substantially Airy's design, correspondence shows that some technical input was made by the instrument maker, Simms; for example he was able to use his experience to advise on dimensions etc.:

> ...with respect to the end frames I think a depth of 6 inches at the centre... but now all this is mere guess work and I have derived the notion from looking at the fragment of a pattern that I found among our oddments...[53]

Simms also agreed with the choice of Ransomes and May, of Ipswich, to perform the engineering work; the carpenter, William

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[51]Airy, *op.cit.* (note 1), p.94.
[52]J.F.W.Herschel to Airy, 15 August 1833, RGO6 157 f.162.
[53]Simms to Airy, 29 August 1835, RGO6 157 f.259.

Quinsee, the smith, Shallow and Coleman, and the stone-mason, Swinton, all came from central Cambridge.[54] By September 1835 Simms was lamenting the fact that he could not make a serious start on the work till the following month because he was very busy with work on 56 theodolites which had been ordered by the East India Company.[55] Airy constantly pressed Simms as to how the work was progressing, and by December 1837 it was nearing completion: "...we inserted the declination axis, fixed the object end on to the tube and put the tube into its place...".[56] In June 1838 Simms informed Airy that "The eyepieces for the equatorial are not yet finished... in other respects, I believe, every thing is pretty nearly complete",[57] and by October, Airy, thinking that the work was very nearly finished, asked for Simms' bill.[58] The total bill for the work done on the telescope came to £1940 (not including the 15,000 francs spent on the object-glass). This was received from the Duke of Northumberland in two instalments, £1500 on 20 January 1836, and £440 on 20 April 1839. Simms proportion of the £1940 amounted to around £572; the carpenter's work was rather more expensive at nearly £700.[59] The interesting point about this work on the Northumberland Equatorial is that nearly all of it was carried out when Airy was no longer in charge at Cambridge Observatory, and yet it was all accomplished under his direction; he only resigned it to Challis, the new Plumian Professor, on 19 August 1839,[60] when it was

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[54] Airy, memorandum, 1836, RGO6 157 f.275.
[55] Simms to Airy, 4 September 1835, RGO6 157 f.261.
[56] Simms to Airy, 1 December 1837, RGO6 157 f.289.
[57] Simms to Airy, 9 June 1838, RGO6 157 f.306.
[58] Airy to Simms, 22 October 1838, RGO6 157 f.318.
[59] See RGO6 157 ff.436-451 for details of bills.
[60] Airy, *op.cit.* (note 1), p.138.

complete and the accounts were in process of settlement. This indicates a great concern that the job be carried out effectively, as Airy must have found it increasingly difficult to devote time to such a project in addition to his duties as Astronomer Royal.

Airy was aware that additional benefit would come from his design, in terms of stimulating interest in astronomy and astronomical instrumentation, if he could provide a printed account of the Northumberland Equatorial. Thus in 1842 he applied to the Duke of Northumberland to provide funds for the expense which would be incurred in the printing and distribution, and the drawings for such an account; these expenses Airy estimated at £120, to which the Duke agreed.[61] Besides many philosophers who received copies of the work, some of the elite instrument makers such as Dollond, and Merz and Repsold, the German makers, were sent copies. Thomas Jones received a copy bound up with the Cambridge Observations. Of those who had worked on the equatorial, Ransomes and May were given a copy and William Simms two copies.[62]

While the account[63] was a lengthy one, running to 39 pages of text in addition to 67 figures, it was remarkable in its total omission to mention the contribution of William Simms. Simms did not get any recognition by name at all, and is only mentioned indirectly once, viz. "I may mention here that the artist, by

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[61] Airy to Duke of Northumberland, 1842, RGO6 158 f.5.
[62] Airy, memorandum, 1843, RGO6 158 f.13.
[63] G.B.Airy, *An Account of the Northumberland Equatorial and Dome,* (Cambridge, 1844).

departing from my instructions, has made this part of the apparatus a little more complicated than is necessary...". It is surely inconceivable that Airy should simply have forgotten to mention Simms; a more plausible explanation is that he regarded himself, having provided the innovative element in the design of the instrument, as the main agent in its realisation, and Simms, as he was not providing any innovation in philosophical principle but only in engineering practice, did not warrant a mention as his role could have been played by any skilled instrument maker. If this is a correct appraisal of Airy's motives in this situation, then it ought also to be said that in future published accounts he did mention Simms by name,[64] perhaps appreciating that Simms' level of workmanship was in fact not a widely available commodity. But in this case it seems that Airy considered that his own intellectual and philosophical expertise had been the major element in the realisation of the project, and as such the only role worth mentioning. I would now like to extend this discussion of Airy's perception of the relationship between philosopher and artisan, in the design of astronomical and other instruments, by reference to his career as Astronomer Royal.

5. Airy's Treatment of Instrument Makers.

The predominant theme in Airy's correspondence with instrument makers is that they are seen as *workmen,* and as they have been favoured with his custom and that of the Royal

[64] See for example G.B.Airy, *History and Description of the Water Telescope,* (London, 1871).

Observatory, so they are expected accordingly to deliver their work to a high standard. Airy, for his part, was a Government employee with an important job to do in controlling the nation's first observatory, and his dealings with instrument makers illustrate that this was uppermost in his mind. The efficient running of the institution demanded that he receive well prepared bills from makers, that makers be constantly urged on with their work, that any charges that seemed exorbitant be disputed, and that an instrument maker's theoretical knowledge always be treated with reservations (thus all instructions should be given explicitly). Any judgement as to whether Airy was "unfair" to makers can only be made bearing in mind that the decisions which he took were based on what was deemed best for the Observatory and for the nation by whom he was employed.

The way in which even a Fellow of the Royal Society had to comply with Observatory protocol is well illustrated by Airy's correspondence with Thomas Jones:

> Mr.Airy presents his compliments to Mr.Jones, and begs to mention that all letters and parcels relating to the Royal Observatory should be addressed to Mr.Airy and not to any one of my assistants. In consequence of the last parcel from Mr.Jones being addressed to Mr.Richardson, great delay in the use of it and much inconvenience have been caused.[65]

With William Simms, a problem involving bills arose when Simms failed to divide his bills into two parts headed "The Royal Observatory" and "The Royal Observatory Magnetic Department",[66] and Negretti and Zambra were reprimanded for not dating their

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[65]Airy to T.Jones, 20 July 1836, RGO6 715 f.637.
[66]Airy to Simms, 6 June 1841, RGO6 718 f.767.

letters.[67]

John Newman, who described himself as "Maker to the Royal Institution", provides an example of the way in which Airy would press a maker in order to get his work completed in the shortest possible time. Newman specialised in meteorological instruments, and it was mainly these that he made for the Observatory. For example Airy wrote to Newman on 6 October 1840:

> I request that you will furnish to the Observatory with all possible dispatch the following instruments.
> A standard Barometer
> A standard Thermometer
> A wet and dry bulb thermometer
> An atmospheric Electrometer
> A thermometer for the Sun
> A thermometer in a mirror for radiation
> The instruments to be of the same class as those already furnished under the direction of the Royal Society to the Magnetical Observatories.[68]

Clearly the standard of Newman's workmanship was not in question, but Airy regarded it as his prerogative, as customer, to question him as to when they might be ready, which he did on 21 October,[69] and Newman, as a tradesman, had to comply with the wishes of his customers: "I flatter myself they will be ready the middle of the next month. I am aware they are very much wanted and will do my utmost to complete them as easily as possible".[70] Airy had also included an anemometer in the list of requests in an earlier letter, and when he heard of some improvements to it which Follett Osler had made - relating to pressure indications and the facility with which some of the springs could be examined, he made sure that Newman was aware of their existence and that he

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[67]Airy to Negretti and Zambra, 6 October 1853, RGO6 726 f.663.
[68]Airy to J.Newman, 6 October 1840, RGO6 715 f.25.
[69]Airy to Newman, 21 October 1840, RGO6 715 f.26.
[70]Newman to Airy, 23 October 1840, RGO6 715 f.27.

should incorporate them in the anemometer he was constructing.[71] It seems that despite Newman's assurances that the instruments would be ready by the middle of November, they were delivered no earlier than the middle of December,[72] which to some extent vindicates Airy's pestering of instrument makers to speed up the jobs they were commissioned to do for the Observatory.

Airy also employed Newman to make some electrometers for the Observatory, and in these dealings he was careful to be explicit with the instrument maker as to exactly what he wanted lest the instrument maker's philosophical knowledge not be equal to the task. On 9 December 1843 he wrote:

> I am endeavouring to make some of our electrical apparatus more efficient, and shall be glad if you will furnish us with some electrometers similar to those which you (I believe) have constructed for the Kew Observatory. I should be glad to have the following.
>
> A Coulomb's electrometer.
>
> Two or three expansion electrometers, one the most delicate possible, and the other less delicate by degrees to Coulomb's electrometer. These I wish to be mounted in the same way as those at Kew...[73]

By 23 December, Airy was wondering if his terminology had confused the instrument maker:

> I perceive that in my letter of Dec 9 I have spoken of the electrometer as Coulomb's, and have thereby probably confused you. It was entirely my blunder, the electrometer which I meant was that in which a ball is repelled from a vertical rod of metal, and not the torsion electrometer.[74]

However Newman replied that in fact he had understood perfectly well what Airy meant, as a drawing of the arrangement in the

[71] Airy to Newman, 5 November 1840, RGO6 715 f.28.
[72] Airy to Newman, 14 December 1840, RGO6 715 f.29.
[73] Airy to Newman, 9 December 1843, RGO6 718 f.29.
[74] Airy to Newman, 23 December 1843, RGO6 718 f.30.

desired electrometer had been enclosed with the first letter, and this served much better to explain what was wanted than any name could.[75]

Newman did not provide Airy with any great trouble as an instrument maker; he was reasonably prompt with his orders, and he was as we have seen technically competent. Seemingly the most difficult maker to deal with the Royal Observatory was John Bennett, of Cheapside, who made two horological movements and some thermometers for Airy. Bennett sent a bill to the Observatory in April 1849 which included some items dating as far back as 1847. Airy, furious that his careful system was being upset in this way, wrote back immediately:

> It is entirely contrary to the system of the Royal Observatory that bills should be suffered to run to their length. Upon referring to our Register I find that applications for your bill have been made on the following days:- 1847 Dec 31 1848 March 31 July 1 Sept 30 1849 Jan 1 so that I ought to have had six accounts instead of the one which is now sent.
> I cannot allow this interruption of our system to continue longer, and I therefore beg to acquaint you that you can have no further transaction with the R.Observatory unless you engage to send in your accounts quarterly, complete to the end of every quarter.
> You will have the goodness to acquaint me in writing whether you will comply with this condition.[76]

Bennett's reply to this threatened withdrawal of Observatory business was what one would expect from a shrewd businessman:

> I regret deeply that the accounts have not been delivered according to the Observatory regulation. I see fully the necessity of the most strict exactness in every transaction connected with such an establishment. I feel it a great honour to be

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[75]Newman to Airy, 27 December 1843, RGO6 718 f.31.
[76]Airy to J.Bennett, 10 April 1849, RGO6 722 f.49.

> entrusted with any work under your control and most sincerely pledge myself to observe whatever directions or regulations you may think proper to lay down.
> Permit me to add that my experience during my Trade in London has enabled me to collect such means of manufacture that I hope to be able to produce mechanical instruments in every way satisfactory as to price, time, and execution.[77]

The following day Airy was complaining again, this time about items in Bennett's lengthy bill which had not been received.[78] It emerged that one of these was a model which Bennett had kept, and several others were received a few days later. Despite Bennett's assurances, however, Airy continued to have trouble with him, writing on 25 September 1849: "On June 21 I addressed to you a letter of inquiry respecting certain points in the construction of our photographic time-pieces to which I have received no answer. I beg now to inquire whether it is in your power to give me an answer".[79] No answer was forthcoming, and by the start of November Airy had had enough, and asked Glaisher to reclaim all thermometers and other instruments which Bennett was constructing or repairing for him, as the Observatory would have nothing more to do with him.[80]

Remarkably, this was not the last Airy heard of Bennett, for Bennett exhibited some of his instruments at the Great Exhibition of 1851, with which Airy was closely involved. What was unusual was that Bennett actually described himself on the dials of his clocks as "Maker to the Royal Observatory". E.B.Denison (one of the jurors) wrote to Airy to ask to what extent this title was

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[77]Bennett to Airy, 11 April 1849, RGO6 722 f.50.
[78]Airy to Bennett, 12 April 1849, RGO6 722 f.52.

[79]Airy to Bennett, 25 September 1849, RGO6 722 f.66.
[80]Airy to J.Glaisher, 1 November 1849, RGO6 722 f.67.

justified, as if Bennett were *the* maker to the Royal Observatory he would deserve some sort of special notice above the other makers who described themselves as at most "Makers to the Admiralty".[81] Airy's reply was indicative of his attitude to a maker who had disturbed the efficient running of the nation's chief observatory by his conduct:

> The assumption by Mr.Bennett of the title "Maker to the Royal Observatory" is most impudent and most unwarrantable.
> Mr.Bennett was never employed on any fine clock-work or watch-work, requiring great accuracy of movement, at the Royal Observatory. He was employed to make two of the horological movements for the revolving cylinders carrying the photographic paper of the self-registering apparatus in the Magnetic Meteorological Department. But he was so extremely unpunctual in business and so unmanageable that he was dismissed from the Observatory, and will certainly never be employed there again.
> It is so much the custom for persons who have once been employed under any department of the Government to retain the title of "Makers to that department" to the end of their lives that I have thought it useless to interfere with Mr.Bennett. But I have no hesitation in saying that this is the most impudent appropriation of the title that I have ever known.[82]

Bennett's initial letter to Airy attempted to gain the favour of the Astronomer Royal by assurances that in future he would find no reason to criticise Bennett's conduct or workmanship. The way in which Henry Barrow tried to maintain the Astronomer Royal's custom was by playing on the reputation of his predecessor, Thomas Robinson, most famous for his construction of precision balances. Robinson had made a vertical force magnetometer for the Observatory in 1840, Airy, as ever, urging

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[81]E.B.Denison to Airy, 16 June 1851, RGO6 441 f.255.
[82]Airy to Denison, 16 June 1851, RGO6 441 f.257.

him on with this job (even though Robinson at some stages of the work pleaded that ill-health had been slowing him down),[83] and on Robinson's death in 1841, Barrow, who succeeded to his business at 38 Devonshire Street, Portland Place, was keen that his master's connections with the Royal Observatory be maintained. Barrow also made use of Robinson's reputation by using his master's name extensively, for example he sent Airy a "Catalogue of Mathematical Optical and Philosophical Instruments made by the late T.C.Robinson", which, besides the standard magnetic instruments and balances for which he was noted, included microscopes at £4, £3, and £2 10s., and a fine universal equatorial by Ramsden at £63, this being the most expensive instrument in the catalogue.[84] In August 1842 Airy received a letter from Barrow which asked for a continuation of the custom of the Royal Observatory which had been so much valued by his master:

> Having succeeded to the business of the late Mr.Thos.Robinson, Astronomical, Surveying, and Mathematical Instrument Maker, I most respectfully beg a continuance of your liberal patronage, and to assure you that my utmost exertions will at all times be used, to merit a succession of those favors with which he was so highly honoured.[85]

Attached was a catalogue of instruments made by the firm under the new name of Robinson and Barrow, which included, besides barometers, thermometers, and dipping needles, traditional

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[83] See the correspondence between Airy and T.C.Robinson, 1840, RGO6 716 ff.226-232.

[84] Catalogue of Mathematical, Optical, and Philosophical Instruments made by the late T.C.Robinson, 1842, RGO6 718 f.266.

[85] H.Barrow to Airy, 30 August 1842, RGO6 718 f.269.

astronomical instruments such as transit instruments, reflecting circles, altitude and azimuth circles, and portable telescopes. The Royal Observatory however continued to do business with the company only in those areas in which Robinson had already proved himself, and did not venture into the field of astronomical instruments.

The following year Barrow continued to sign his letters "Successor to the late T.C.Robinson", and after moving his premises to 26 Oxenden Street, Haymarket, he did not even sign his own name, but only "Robinson + Barrow".[86] Having established his reputation by use of his master's name, Barrow eventually dropped the word "Robinson" from the company name in the late 1840s. However he retained the name on his bill headings, describing the company as "H.Barrow + Co. Successors to the Late T.C.Robinson. Mathematical + Optical Instrument Makers to the Lords Commissioners of the Admiralty".[87] Whilst Airy was generally satisfied with the high standard of Barrow's workmanship, there were occasional aberrations, for example when the "tumbling bucket" of an Osler's anemometer which Barrow had made would not act, Airy wrote:

> I am really quite surprised that you should make such a bungling business of a thing as simple as this. I will send it again to you, and if you cannot make it go right, I will if I have to make it right with my own hands...[88]

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[86] Barrow to Airy, 29 September 1843, RGO6 718 f.277.

[87] Bill from H.Barrow and Co. to Airy, 1850, RGO6 723 f.29. For further details of Barrow's career, see J.T.Stock, "Henry Barrow, Instrument Maker", *Bulletin of the Scientific Instrument Society,* 1986 (9), pp.11-12.

[88] Airy to Barrow, 26 May 1852, RGO6 725 f.47.

The message was clear - Barrow was accepted as being in general a good workman, but he was fallible, and occasionally Airy's demands would fail to be met by a man who was not a philosopher and thus could not be expected fully to comprehend the scientific enterprise. Sometimes this lack of expertise in instrument makers was more serious. Watkins and Hill were extensively employed by Airy on magnetic work. For example in the early days of the Magnetic Department Francis Watkins himself magnetised several bars,[89] and a decade later they were still the most employed single company on electrical and magnetic apparatus for the Observatory. However they were liable to make grave errors which the philosopher could detect, as Glaisher (who with ten years experience as superintendent of the Magnetic and Meteorological Department could claim that status) wrote to them in August 1846:

> The Instrument you had lately to repair, was returned by you in a defective state, and it is useless. I fear that you have used sealing wax varnish in its repair, and not the wax itself. You are particularly requested to use the wax itself in the making or repairing of any Instruments you may have to do for the Royal Observatory...[90]

In his dealings with all the instrument makers mentioned, then, the primary motive in Airy's attitude was that he had a job to do, and that he must do his utmost to ensure the smooth running of the Observatory. If he managed to provide an economy of time then financial economy would follow, and this would be advantageous to his employer, the State. This often meant repeatedly encouraging makers to hasten jobs which they were carrying out, and it also meant bringing them into line with the

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[89] F.Watkins to Airy, 14 March 1837, RGO6 716 f.784.
[90] Glaisher to Watkins and Hill, 27 August 1846, RGO6 720 f.737.

administrative procedures of the Observatory regarding the preparation and submission of accounts. Occasionally his tone in describing technical details to makers may seem patronising to those who made attention to technical details their profession. Still, this action was effective in ensuring that the Observatory became equipped with instruments in the most efficient manner. The makers, for their part, did not seem to resent being regarded as workmen, for a commission from the Royal Observatory was not only a great reward in direct financial terms, but also in terms of reputation. Only in isolated cases, as with Bennett, did the makers act in any way detrimental to their livelihood; in general, the high standard of their workmanship and their good trade practice were acknowledged by Airy as sufficient reasons to continue Observatory custom.

6. Astronomy at Greenwich: the Large Instruments.

The greater part of Airy's dealing with instrument makers at Greenwich was concerned with the large astronomical instruments of his own design with which he re-equipped the Observatory. The Northumberland Equatorial which he designed for the Cambridge Observatory has already been mentioned. Whilst at Greenwich he designed and supervised the construction of an altazimuth instrument (brought into use in 1847), a transit circle (1851), and a large equatorial (1860), in addition to two smaller instruments, a reflex zenith tube (1851), and a water telescope (1870). The instrumental work for all of these was performed by Troughton and Simms, with the engineering work for the large instruments being done by Ransomes and May (later Ransomes and Sims) of Ipswich.

The purpose of an altazimuth instrument is to determine altitude (height above the horizon) and azimuth (the angle measured on the horizontal from north). It consists of a telescope revolving on a horizontal axis, and both the telescope and the piers which carry its pivots can be rotated so as to point in any direction. The purpose of Airy's altazimuth was to provide more frequent observations of the Moon and as such its introduction was fully in accord with the original purpose of the Observatory - to aid navigation. The need for altazimuth instruments arises because with meridian instruments it is impossible to observe the Moon for 4 days either side of a new Moon due to glare from the Sun.[91]

Airy's guiding principles in the design of the instrument were characteristically *practical* - for example he was concerned mainly with providing firmness by making it in as few pieces as possible, by using bolts and not screws to join the principal parts, and so on.[92] The initial orders for the instrumental work with Troughton and Simms, and the engineering work with Ransomes and May, were placed in 1843. Throughout the correspondence with the two firms it is apparent that there was considerable interactive discussion of the details of the design. Simms and Airy for example discussed some of Charles May's proposals for the altazimuth:

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[91] See Maunder, *op.cit.* (note 5), pp.205-27 for a semi-technical discussion of the altazimuth.

[92] D.Howse, *Greenwich Observatory. Volume 3: The Buildings and Instruments*, (London, 1975), pp.53-4.

> Simms: I do not at all approve of Mr.May's plans for turning the pivots of the vertical circle...
>
> Airy: I agree entirely with Mr.Simms in this matter. All my own experience, and my observation of the practice of others (as in the construction of engineering machines) have impressed on me that where the object to be attained is defined by a simple geometric principle, that principle ought to be followed in the manner of working for the object...[93]

However, Airy would have felt that Simms and May were contributing, by their advice, to problems of engineering and instrument making practice only, and that the important element, the innovative element, in the design, was due to him as the philosopher. He was nonetheless generous in writing to Ransomes and May concerning their contribution, when it was enquired whether their name should be inscribed on part of the instrument:

> When Mr.Jones was here a few days since he inquired whether there would be anything wrong in your putting the name "Ransomes and May, Engineers" or something equivalent, upon some part of the Altitude and Azimuth Instrument. I replied that certainly there would be nothing wrong in it: that you have been consulted confidentially on almost every part of the construction, and have in fact as great a share in the plan as either Mr.Simms or I, and therefore that it is quite proper that your name should appear if you think fit.[94]

By December 1843 Simms had undertaken to construct a model of the proposed instrument, though he wrote to inform Airy that this model was not so far advanced as he would have liked on account of his normal workman for such jobs having died of a fever, and the new workman not being so well acquainted with the

[93] Notes by Airy and Simms, 1843, RGO6 718 f.150.
[94] Airy to Ransomes and May, March 1845, RGO6 720 f.75.

task.[95] By the end of February 1844 the model was ready,[96] and by March Simms was at work upon the object-glass.[97] It had been the original intention to use a German glass by Merz, but those which Simms received from him were all too long in focal length, and so Simms set about working some glasses of his own. The engineering work was under way by July 1844, though Airy, typically, was rushing Ransomes and May along - even with a firm with which he had family ties, the needs of the Observatory were paramount:

> Pray give me a line to say how the work is going on. I did not expect, after your promise of expedition, that it would have been so very tardy. It is really breaking to find oneself stopped thus without any cause of delay attributable to self...[98]

By December 1846 Simms was ready to proceed with the circle division for the altazimuth,[99] and by mid-January all the necessary preparations had been made for hoisting the heavy parts.[100] The circle division was completed before the end of January, though there were still problems with some of the workmanship: for example, the telescope would not turn round due to the lip of the clamp of the vertical circle being too long.[101] The first observation with the new instrument was made on 16 May 1847, though the initial set of observations did not satisfy Airy, who complained to Simms:

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[95] Simms to Airy, 23 December 1843, RGO6 718 f.898.
[96] Simms to Airy, 29 February 1844, RGO6 718 f.925.
[97] Simms to Airy, 21 March 1844, RGO6 718 f.939.
[98] Airy to C.May, 2 July 1844, RGO6 718 f.176.
[99] Airy to Simms, 3 December 1846, RGO6 720 f.252.
[100] Airy to Simms, 18 January 1847, RGO6 720 f.258.
[101] Airy to Simms, 12 March 1847, RGO6 720 f.262.

> I have carefully examined the observations with the Altitude and Azimuth Instrument. They are none very good, and they lead me to hope that if one fault is corrected they will be unusually good. That fault is in the levels. <u>You must alter your construction of levels entirely</u>: the English levels, as a class, are not fit to be mentioned on the same day with the Hamburgh or Pulkowa Circles.[102]

The transit circle which Airy introduced in 1851 superseded both Troughton's transit instrument and mural circle (i.e. one mounted on a wall, with telescope attached) at the Royal Observatory. Whereas the transit instrument measured right ascension (the co-ordinate on the *celestial* sphere corresponding to longitude on the *terrestrial* sphere) and the mural circle measured declination (the celestial co-ordinate equivalent to latitude), the transit circle was able to combine these two observations on the same meridian transit. The transit circle had the additional advantage over the mural circle that it was supported equally on both sides and thus was not susceptible to the same bending that was liable to occur in the mural circle which was supported on one side only.[103] Airy first proposed the introduction of a transit circle in the report which he gave to the Board of Visitors of the Observatory in 1847:

> Our present instruments were, at the time of their erection, the best in the world, but they are not so now, and we actually feel this in our observations.[104]

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[102]Airy to Simms, 29 September 1847, RGO6 720 f.294.

[103]See Maunder *op.cit.* (note 5), pp.181-204, for a semi-technical discussion of the Transit and Circle Departments at the Royal Observatory.

[104]G.B.Airy, *Report of the Astronomer Royal to the Board of Visitors, 1847,* (London, 1847).

In January 1848 Airy enquired of Merz whether he had any 8 inch aperture object-glasses in stock, or if not, when he could undertake to finish any. He was also keen to know if any such object-glasses could be tested, say in Munich or the surrounding area, by himself or a deputy.[105] By April, however, it had become apparent that there was no need for the Observatory's custom to be given to a foreign maker as a perfectly acceptable object-glass was available in London, in the hands of Simms. Simms tested it on 31 March 1848, and regarded it as "the best that I have yet made",[106] and he offered it to Airy at £300, claiming that "even this is not remuneration as a matter of business".[107] However Airy characteristically questioned this charge and, under pressure, Simms reduced the asking price to £275, at which sum Airy agreed to purchase it. The transit circle was then designed around this 8 inch object-glass, with Simms, and Ransomes and May, providing the same degree of technical input which they had done with the recently completed altazimuth. May's plans for the instrument were ready by June 1848, and work then proceeded relatively smoothly so that by the end of May 1849 the engineers were hopeful that their initial part of the work would be finished within 2 or 3 months and they would be ready for Simms.[108] By March 1850 the work was nearing completion:

> The Circle must be mounted... on its proper axis and while there four points will be marked...
> The Circle must then be separated and sent to Simms in London, the four points above mentioned

[105]Airy to G.Merz, 31 January 1848, RGO6 721 f.438.
[106]Simms to Airy, 31 March 1848, RGO6 721 f.841.
[107]Simms to Airy, 3 April 1848, RGO6 721 f.844.
[108]May to Airy, 28 May 1849, RGO6 722 f.609.

> will give him the means of planting it properly on his dividing engine, and he will mark it off in no time...[109]

The mapping of the cardinal points was carried out at Ipswich on 16 May 1850,[110] and Ransomes and May began to erect the instrument on 1 July, expecting this to take a few days.[111] The first observation with the new instrument was made on 4 January 1851, Airy being thwarted, by poor weather, in his desire to make the observation on the first day of the new half-century.

Airy's opinion as to the excellence of the workmanship displayed in the transit circle was illustrated in his Presidential Address to the British Association for the Advancement of Science in 1851; in this address he also acknowledged that the instruments could have an influence on the future course of instrument design:

> ...for the admirable proportions of its various parts, for the firmness of fitting of the few portions of which it is composed, and for the accuracy of the external forms of pivots etc. it may well be considered as one of the finest specimens of engineering that has ever been produced. As an example of an excellent mechanical structure carrying a large object glass, I think it probable that the Greenwich transit circle may have a great influence on the construction of future instruments.[112]

The account of the instrument in his report to the Visitors hints at the relative importance of the roles of philosopher and engineer/instrument maker in the realisation of the design; for

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[109] Airy to May, 11 March 1850, RGO6 723 f.604.
[110] May to Airy, 16 May 1850, RGO6 723 f.614.
[111] Airy to Simms, 1 July 1850, RGO6 723 f.763.
[112] G.B.Airy, "Presidential Address", *B.A.A.S. Report, Ipswich, 1851,* pp.xxxix-liii, on p.xli.

Airy's part, much "thought" was required, but on the part of the artisans, "anxiety" was required:

> ...no other existing meridional instrument can be compared with it. This presumed excellence has not been obtained without much thought on my part and much anxiety on the part of the constructors of the instrument...[113]

The Reflex Zenith Tube, designed by Airy and constructed by Simms, with a 1793 object-glass by Peter Dollond, removed from an existing 10-foot transit instrument at the Observatory, was completed in the same year, 1851, as the transit circle, and its function placed it squarely within the practical purpose of the Observatory. The star which this instrument was arranged to observe was Gamma Draconis, which passes almost directly overhead in London (there is no such star passing over the Paris Observatory, for example). So by the end of his first two decades as Astronomer Royal, all additions which Airy had made to the major astronomical instruments at the Observatory had been firmly in keeping with the original requirements of the institution. In 1855, however, in his address to the Visitors, after describing deficiencies in the present equatorials at Greenwich, he proposed the construction of a new equatorial with an object-glass of 12.8 inches by Merz of Munich, with a mounting similar to that of the Northumberland Equatorial which he had designed.[114] This equatorial was to become known as the "Great" or "South East" Equatorial, and as with previous large scale projects, the

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[113] G.B.Airy, *Report of the Astronomer Royal to the Board of Visitors, 1851,* (London, 1851).

[114] G.B.Airy, *Report of the Astronomer Royal to the Board of Visitors, 1855,* (London, 1855).

general optical work was done by Troughton and Simms, with the engineering work by Ransomes and Sims (as the Ipswich firm had been known since 1852); it also included a water-driven clock by Edward Dent.

The truth of the matter was that by the mid-1850s the trend in astronomy was towards studies not related to meridional measurements, and in order that the nation's chief observatory keep pace with other countries, Airy reluctantly decided that a first-class equatorial be introduced at Greenwich, while giving assurances that its introduction would not affect the standard work for the pursuit of which the Observatory existed.[115] Although Airy had approached Merz of Munich regarding object-glasses for both the altazimuth and the transit circle, the equatorial was the first of these instruments at Greenwich for which the object-glass was furnished by a foreign maker. Airy had first approached Merz and Sons in September 1855, asking for details of their large object-glasses.[116] Merz quoted prices of from £430 for a glass of 8 French inches aperture to £2660 for one of 16 French inches, and said that an objective of 12 inches aperture was available for test at Munich.[117] By mid-November Airy asked for this object-glass to be sent to Greenwich for testing, but Merz, evidently reluctant to part with it, said that another was being made and that Airy would be sent whichever was the superior.[118] Airy, typically, made repeated enquiries as to the state of progress of the object-glass, and even threatened by

[115] See Maunder, *op.cit.* (note 5), pp.221-2.
[116] Airy to Merz, 14 September 1855, RGO6 729 f.558.
[117] Merz to Airy, 24 September 1855, RGO6 729 f.559.
[118] Merz to Airy, 22 November 1855, RGO6 729 f.567.

August 1856 "...that some limit be understood as to the time in which you can supply it".[119] As Merz had originally quoted 12-18 months for the supply of the objective, Lowne suggests that Airy's approach after less than a year was "a trifle high-handed", but as we have seen, it was no more than characteristic.[120] The objective was eventually finished, more or less within the estimated time, in June 1857, and by mid-October Airy had tested it:

> With a bright star there are a few faint rays of which I do not know the origin (I do not see the veins in the glass) but the central image is small and good and surrounded by well-formed rings. There is a little secondary colour... on the whole I think it will prove a very fine object-glass.[121]

The first observation with the new equatorial was made on 24 May 1860, and the instrument remained in its original position until the 1890s, when it was replaced by an equatorial of 28 inch aperture object-glass.

The Great Equatorial was the last of the large instruments which Airy designed for Greenwich Observatory, and in all such instruments he had relied upon the high standard of workmanship of Troughton and Simms, and of Ransomes and May, rarely being dissatisfied with their ability. Sufficient testimony to the excellence of these instruments is their longevity; the altazimuth continued in use until 1910, and the transit circle made some 600,000 observations over a period of 103 years until its last in 1954. Given such long use, Airy's faith in the firm

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[119]Airy to Merz, 20 August 1856, RGO6 730 f.734.

[120]C.M.Lowne, "The Object-Glass of the Greenwich "Great Equatorial Telescope"", *Journal for the History of Astronomy,* 1988 (19), pp.169-82.

[121]Airy to Merz, 14 October 1857, RGO6 732 f.634.

of Troughton and Simms can be seen to have been well-founded. In the next section I would like to consider more fully the nature of the symbiotic relationship which existed between Airy and Simms during the former's time as Astronomer Royal. Though unique among philosopher/artisan relationships, its analysis will provide valuable illumination of the connections between men of science and instrument makers in the nineteenth century.

7. Airy and William Simms, Philosopher and Skilled Artisan.

William Simms, having served his apprenticeship and worked for a time on his own account, went into partnership with Edward Troughton in 1826, immediately taking on the greater part of the responsibilities of the firm of Troughton and Simms, due to the advancing years of his partner. While Simms' own background did not make him anything other than a knowledgeable artisan, the reputation of his partner ensured that he worked on the most prestigious commissions, and these gave him valuable experience so that on Troughton's death in 1835 he could claim to be one of the top instrument makers in the land. Only George Dollond and Thomas Jones could lay claim to a title which Simms could not - that of Fellow of the Royal Society, though Simms would underline his position among the elite of the instrument makers by acquiring that title in 1852, after completing work on the transit circle and the reflex zenith tube the previous year. Interestingly, although Simms did not have the educational background that Airy had, he fully appreciated the value of such a grounding, for he enrolled his own son, William Henry, at

Pembroke College, Cambridge, in the Michaelmas term of 1838.[122] The younger Simms is described as "Eldest Son of William, merchant, of London", although there is no record of his ever having graduated, so it must be assumed that he did not proceed to his B.A. Degree.[123]

Simms was the last British instrument maker to become a Fellow of the Royal Society (Thomas and Howard Grubb were Irish), an indication that the makers as a group had lost their former status in the new century. To some extent this must be put down to an increasing use of industrial methods and the neglect of the traditional craft skills in which the individual maker could excel. If a maker wished to work to the highest standards of accuracy and to deliver his products in the shortest possible time, which he must if he were to maintain business, then there was no option but to expand one's enterprise and to adopt those new industrial techniques which were available (though instrument making became *mechanised* to a far lesser extent than most other manual trades). With this increasing stress on the business side of things from the point of view of the maker, which I will discuss in more detail in Chapter 8, the innovative element in

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[122] Simms to Airy, 8 October 1838, RGO6 716 f.328.

[123] "Simms, William Henry", in J.Venn (ed.), *Alumni Cantabrigiensis: A Biographical List of All Known Students, Graduates, and Holders of Office at the University of Cambridge*, (Cambridge, 1922). The younger Simms did not work for his father's firm, but is known to have published several articles on astronomy and instrumentation, for example W.H.Simms, "Formulae for Deducing the Latitude of an Observatory, from Observations of Stars with a Transit-Instrument placed in the Prime Vertical", *Memoirs of the Royal Astronomical Society,* 1858 (26), pp.1-8; W.H.Simms, "On Corrections to be applied to Observations made with the Sextant", *ibid,* pp.19-44.

instrument design almost always came from the side of the philosopher, with the maker being reduced to a workman carrying out instructions.

With Simms this was not quite true, for as we have seen his experience in engineering and instrument making was such that he could at least provide advice and innovation in technical detail if not in philosophical principle, and this was advice which Airy valued greatly. For example in his first year as Astronomer Royal, Airy corresponded with Simms not only concerning the Northumberland Equatorial then in progress at Cambridge Observatory, but also concerning some of the existing instruments at Greenwich: "We are in a terrible quandary with the Zenith Tube from which we cannot deliver ourselves without your assistance".[124] However, the symbiotic business relationship between the two went further than that - it is true to say that Airy and Simms were actually personal friends. Often in their correspondence Simms can be found enquiring of Airy's health and giving his regards to Airy's family, and we can also find them describing to each other their family holidays.[125] In some cases, even in business letters, Airy's tone is relaxed and even witty, for example referring to Bradley's Sector which Simms was adjusting for the Observatory in 1837, among his instructions were:

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[124] Airy to Simms, 8 November 1836, RGO6 716 f.263.
[125] See for example Airy to Simms, 13 January 1837, RGO6 716 f.269.

> ... push on the repairs now in hand... inform me when the repairs will be finished... inform me when you can undertake, under a penalty of £50,000, that the revolving stand will be completed...[126]

Such flippancy is not typical of Airy's business practice; it seems that he felt that in Simms he had an artisan whom he could trust completely. When Simms went to Munich in 1846 to examine a telescope which Merz was preparing for the United States, he was quick to ask Airy whether there was any useful business he could accomplish for him whilst there.[127] Airy took advantage of this offer to increase his knowledge of the German firm, asking Simms if he would:

> 1. Look at our 8 inch telescope (which you are to mount). Merz knows nothing about this in connection with Liverpool, he only knows it is for me. If possible, get a trial of it, at any rate get its size and length so that our mounting can proceed.
> 2. If you can see, learn, examine, or try, any thing about the mounted 4 inch equatorial which Merz is making at Sir John Herschel's order, pray do so.
> 3. I should be very glad to have (on private account) two large prisms, one of Merz's heaviest glasses (I mean most dispersive) and one of his least dispersive.
> 4. If you can find any thing... of his different instruments, I should like to have them.
> 5. Pray do not fail to bring a few copies of his priced catalogue...[128]

Clearly, then, Simms was able to be of more benefit to Airy and to the Royal Observatory than was the typical instrument maker whom he employed; he was not only employed to construct astronomical instruments for the Observatory but was commissioned on one occasion to procure for Airy a Wertheimer's adding

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[126] Airy to Simms, 26 March 1837, RGO6 716 f.279.
[127] Simms to Airy, 24 April 1846, RGO6 720 f.229.
[128] Airy to Simms, 27 April 1846, RGO6 720 f.230.

machine,[129] and Troughton and Simms were regularly asked to supply ether for the use of the Meteorological Department. The range of the products which Troughton and Simms themselves manufactured, as shown in their catalogue,[130] was very extensive, and even then did not include the products which they were able to procure from other retailers, for example the adding machine and the ether that have been mentioned. Besides standard instruments such as telescopes and microscopes (31 varieties), navigational instruments such as sextants, compasses etc. (24 items), surveying instruments such as levels, theodolites etc. (71 items), barometers, thermometers, air pumps and electrical machines, one could purchase "instruments" as diverse as "A Guinea and Feather Experiment, receiver included", for £2 15s., "brass hemispheres, to demonstrate external pressure", for between 20s. and £1 18s., and even "a small head with hair", for 8s., and "an artificial spider", for 1s.6d.. It seems reasonable to assume from the catalogue that most of the instruments therein were actually manufactured by the firm, rather than that they were acting as retailers for the majority of the instruments (as will have been the case with other prominent makers with larger catalogues, such as Negretti and Zambra):

> Troughton and Simms beg to caution those who may have occasion to write from abroad, that no reliance can be placed on the genuineness of the Instruments they obtain, unless the application be made direct, or through the most respectable channels.

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[129] Airy to Simms, 17 June 1850, RGO6 723 f.760.
[130] Catalogue of Instruments made by Troughton and Simms, RGO6 160 f.147.

If there could be no doubt as to the *extent* of their enterprise, Airy, when the occasion arose, was ready to advocate the *quality* of Troughton and Simms' workmanship to those not so well acquainted with instruments as he was. Sometimes the work undertaken by Simms had a greater personal relevance to Airy, who suffered from astigmatism, and who investigated this condition extensively. In 1858 he wrote to another sufferer who had enquired as to the possibility of obtaining correcting spectacle lenses: "None but a first rate optician can undertake such a manufacture. Mine have been made by Mr.Simms, of Troughton and Simms...and are made to perfection". Airy had ordered 12 new spectacle lenses from Simms in 1847, and on receiving the lenses, pronounced their appearance as excellent. When asked in the context of spectacle lenses why one should use Simms rather than a workman from one's own locality, Airy's response was to state that in such a situation one needed a workman who could be trusted to follow one's directions precisely, and such workmen were not easily found.[131]

The usual situation in which Airy was called upon to recommend instrument makers was in the context of his position as Astronomer Royal, in which capacity he received repeated requests from private gentlemen wishing to purchase telescopes. For example in reply to a request for advice from R.H.Williams in 1843, he mentioned Dollond's name, but recommended Simms; this

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[131] J.R.Levene, "George Biddell Airy and the Discovery and Correction of Astigmatism", *Notes and Records of the Royal Society of London*, 1966 (21), pp.180-99.

individual had also enquired about microscopes to which Airy replied:

> ...in regard to microscopes of the best construction I think that Ross... has the highest reputation, but I have never purchased any of him. Common microscopes may be had equally good from any of the good opticians.[132]

Airy's opinion on telescopes was much the same: that small telescopes could be procured of equal quality from almost all the opticians. But as he wrote to J.F.Miller:

> ...for large ones... I should think it hardly safe to apply to any but the opticians who are considered to have devoted themselves rather to telescopes than to varied philosophical apparatus. Thus considered, Dollond... or Troughton + Simms.. .. would be better than Watkins + Hill. I am giving this opinion entirely on general considerations, for I never looked through a telescope of Watkins + Hill's, and do not know their reputation in regard to telescopes.[133]

Presumably Airy, if pressed, would have recommended any electrical apparatus by Watkins and Hill over that of Troughton and Simms, in that electrical apparatus was their area of expertise - the situation here is rather like Airy's acknowledgement that Faraday's expertise in magnetism was greater than his, though Airy was the leading authority on matters astronomical and optical. Airy's recommendation to Henry Tyson, who wished to purchase a reflecting telescope, was more straightforward: "...I can at once state that I know no artist that I could recommend so strongly as Mr.Simms...".[134] One of the interesting requests that Airy received for information concerning telescopes ran thus:

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[132]Airy to R.H.Williams, 25 September 1843, RGO6 159 f.213.
[133]Airy to J.F.Miller, 13 June 1846, RGO6 159 f.363.
[134]Airy to H.Tyson, 5 May 1848, RGO6 159 f.411.

> I am a working man with small income, have a little knowledge of mathematics and am fond of astronomy. I have lately read your Ipswich lectures, several of Professor Nichol's works and a few others and have a desire to know more on this interesting subject - would you therefore be kind enough to tell me where I could best buy a small telescope, but sufficiently powerful to observe some of the nebula, double stars, Saturn's rings etc...[135]

Airy as usual recommended Simms, though this correspondent was more fortunate than most as the Astronomer Royal agreed to try one of Simms' small telescopes on his behalf.[136] Generally his duty to devote his time to Observatory work would have precluded such an act of generosity.

Recommendations from Airy undoubtedly helped spread Simms' name in amateur circles, and prestigious commissions from the Royal Observatory and from other observatories were always forthcoming for larger instruments which were of value both in direct financial terms and in terms of reputation. However, Simms was deeply upset that in the most visible testing ground of all, the Great Exhibition of 1851, he was not granted the highest award, a Council Medal, for the instruments which he displayed. He felt that the effect of not receiving such an award was bound to be prejudicial to him, especially abroad, whence he derived the greater part of his business.[137] The most inexplicable part of the affair was that Simms' collection of astronomical instruments at the Exhibition had been unanimously pronounced worthy of a Council Medal by a jury of those men most competent to decide, namely philosophers of the pedigree of Herschel and

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[135] L.Barker to Airy, 6 November 1852, RGO6 164 f.136.
[136] Airy to Barker, 8 November 1852, RGO6 164 f.138.
[137] Simms to Airy, 20 October 1851, RGO6 441 f.269.

Brewster, as well as Glaisher, Mathieu, and W.H.Miller. The jury even stated, when the Council Medal had initially been denied to Simms, that

> ...Mr.Simms' exhibition of Astronomical Instruments is not only the finest in the Exhibition, but there are more important inventions in their construction than in all the other exhibited Astronomical Instruments put together.[138]

The Council still ignored the opinion of a jury of elite men of science on a question of science, and no Council Medal was awarded. As Charles Pritchard pointed out, no loss of custom would result to Simms from the direction of those philosophers who fully understood and appreciated his ability, but he was likely to suffer loss of custom from "the far greater multitude who, though quite competent to purchase, are not able fully to appreciate the real merit of astronomical instruments".[139] Airy would certainly have agreed with the opinion of the jury as to the standard of workmanship in the Simms instruments, though on at least one occasion around this time he disputed Simms' claim to having made "important inventions" such as the jury referred to. Simms wrote to Airy in October 1851 referring to a transit circle which he had made for Mr.Pentland, with interior illumination similar to that within the Greenwich transit circle, except that "one prism only is employed".[140] This part Simms believed to have been an invention of his own, and he wished to publish as much, asking Airy if he had any documentary evidence

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[138] C.Pritchard to The Late Council of Chairmen at The Great Exhibition, *The Times*, 25 October 1851.

[139] *Ibid.*

[140] Simms to Airy, 31 October 1851, RGO6 164 f.29.

that the principle was indeed Simms'. Unfortunately for the maker, Airy was able to demonstrate that "the fixing of prisms within the eye end of the telescope" occurred to him before Simms mentioned his method,[141] and so the innovative element had, as was generally the case, been provided by the philosopher, the artisan having been thwarted in his claims to originality. Simms submitted to Airy's better recollection without dispute.[142]

It is reasonable to assume that any damage which Simms' reputation incurred through not winning a Council Medal at the Great Exhibition would have been compensated by his election as a Fellow of the Royal Society the following year. It is equally reasonable to assume that this would not have changed the way in which he was regarded by Airy. Thomas Jones, for example, who became a Fellow in 1835, aged 60, received the same type of instructions as to promptness of bills etc. as did less eminent makers. Jones had proved a troublesome maker to Airy in his days at Cambridge Observatory, when he constructed some pendulums to aid in the observation of the diminution of gravity in a deep mine. The pendulums were set up on 14 July 1828 and investigations proceeded, though on 10 August the calculations which Airy made on his observations showed that something was wrong.[143] Three days later he "perceived an anomaly in the form of the knife edge of one pendulum, and of its agate planes", and as the water in the mine was rising it was imperative to take more results quickly, though these were unsuccessful and Airy gave up the observations, "with the feeling that our time had

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[141]Airy to Simms, 3 November 1851, RGO6 164 f.31.
[142]Simms to Airy, 5 November 1851, RGO6 164 f.34.
[143]Airy, *op.cit.* (note 1), p.83.

been totally lost, mainly through the fault of the maker of the pendulum (T.Jones)".[144] However Jones was employed fairly extensively by Airy at Greenwich on astronomical instruments (among the large instruments at the Observatory was a Jones mural circle erected in Pond's time), though Simms was by far the preferred maker when it came to new jobs, and in fact he was even commissioned to sort out some problems with the Jones circle in preference to Jones himself.

In 1851 Jones was engaged in constructing a double eye tube for the Observatory, just around the time at which the transit circle and reflex zenith tube built by Simms were becoming operational, but was having considerable trouble in completing it, mainly due to problems of old age. Airy, keen that the Observatory should obtain the instrument as soon as possible, wrote concerning the instrument to Simms, a letter which magnificently illustrates not only his respect for the workmanship of a long-established maker, but also his generosity, and his reservations about the theoretical knowledge which artisans could possess compared to philosophers:

> I want you to take an extraordinary step, to aid us at the Observatory, and to save the reputation of an old man.
> I cannot get Thomas Jones to put his double eye-piece in such adjustment that it can be used. Beyond the mere general conception of the thing, the old man does not understand the theory a bit, and for want of this he cannot be made sensible of the adjustments of the merest common workmanship which are necessary for it. It is the most spirit breaking work that I have ever tried.
> I very much want the thing in use at the R.Obs. and yet I cannot deprive him of the credit of the proposal.

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[144] *Ibid.*, p.84.

> Now what I want is, that you should make one for us (I can show you in two or three minutes what adjustments it requires and what its general dimensions ought to be) and then engrave as maker's name "Thomas Jones, 4 Beaufort St., Haymarket". Pray tell me whether you have any objection to this.[145]

Simms performed his task as desired, and the eye tube was operational by January 1852, when Main mentioned it in a meeting of the Royal Astronomical Society, giving full credit of the invention to Jones.[146]

This episode is an excellent illustration of the high trust which Airy placed in Simms, regarding him as much superior in technical ability even to makers who were established Fellows of the Royal Society. However, as we have seen, it was no more than technical expertise which Simms possessed; it was left to the philosopher himself, Airy, to make innovations in philosophical principle in the domain of instrument design.

8. English and Foreign Instrument Makers.

Although Simms did not win a Council Medal at the Great Exhibition, one was awarded to Merz, suggesting that the elite German makers were at least the equal in quality of workmanship and originality of principle in their astronomical instruments to the best that England could offer. However, as we have seen, a jury of philosophers felt that the instruments displayed by the most eminent English maker were the most impressive in the Exhibition, and their opinion cannot be ignored. It must also be remembered, however, that this jury was substantially British, and it was desirable from the point of view of national prestige,

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[145] Airy to Simms, 15 November 1851, RGO6 724 f.749.
[146] R.Main to Simms, 7 January 1852, RGO6 164 f.112.

(as well as convenient when buying an instrument), that a maker from one's own land be preferred to one from Germany or France. In Airy's case, only if the commodity he sought was unavailable from an English maker would he try to obtain it from abroad - in the case of the instruments at Greenwich, the only items he obtained from abroad were the large Merz object-glasses, and some magnetic instruments by Meyerstein,[147] who had equipped the most famous magnetic observatory of all, that of Gauss and Weber.

In published writings on the Exhibition's instruments, the authors generally saw their task as to dispel any doubts that the British reader might have that foreign instrument makers were equal in any way to those from his own land. John Drew, for example, stressed the dominant position of Troughton and Simms' collection: "The Westbury Circle... For elegance and beauty of finish in every part, it will bear comparison with any work sent out by English or foreign mathematical instrument makers...", and then went on to dismiss the French makers: "Here we find theodolites, levels, and sextants, which do not appear superior to those which we may meet with in any ordinary optician's shop in London".[148] In discussing the German makers at length, although he did not concede that the English makers had lost their primary status, he did warn that, if denied encouragement, they might do so:

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[147] Airy to M.Meyerstein, 5 July 1836, RGO6 715 f.778.
[148] J.Drew, "Astronomical Instruments at the Great Exhibition", *The Architect: in Co-operation with The Civil Engineer and Architect's Journal*, 16 August 1851, pp.435-7.

> We now arrive at the only class of instruments which can in any way compare with those of English manufacture: they are supplied by two celebrated establishments in Munich, whence it seems to be the fashion to obtain all the larger telescopes. It is stated, and we fear with truth, that the performance of the Munich glasses is far superior, both from the nature of the glass and the perfection of the grinding, to those of the same size made in England. We trust this reflection on our skill in the manufacture of achromatic object glasses - an art invented by an Englishman - will soon be shown to have no foundation by the skill and energy of our opticians, and that their efforts will be supported by the ready sale of their products in this country - where, alas! the science of astronomy has but comparatively few cultivators. Let the wealthy of our land devote a small proportion of their surplus income to the encouragement of the optical instrument maker, and the disgrace of being surpassed by foreigners may yet be averted...[149]

The same themes occur in Airy's own account, some years earlier, of the progress of astronomy since 1800: displaying the superiority of English to foreign instrument makers is one of his primary purposes in this report.[150] He considers Troughton's method of dividing circles as "the greatest improvement ever made in the art of instrument making".[151] In describing the principles upheld by the German school of instrument making, namely that telescopes are always supported at the middle, not at the ends, and that every part is, if possible, supported by counterpoises, he points out a deficiency in their approach, that an equatorial thus mounted is very liable to tremor, and concludes, in accordance with what a British audience would like to hear, "...the Germans have made no improvements in instruments except

[149] *Ibid.*
[150] G.B.Airy, "Report on the Progress of Astronomy during the Present Century", *B.A.A.S. Report, Oxford, 1832,* pp.125-89.

[151] *Ibid.,* p.132.

in the excellence of the workmanship".[152] He goes on to say: "Our instruments I conceive (though a German would not allow it) to be superior to those of any other nation".[153] Reports such as this cannot, of course, be taken at face value; this one for example was written at the height of the decline of science debate and Airy felt a need elsewhere in the article to assure his audience that there was not the decline of science in England which many claimed. He agreed that England may have dropped behind other nations in some branches of science around 1800, but stated that recently, in those branches with which he was acquainted, a rapid progress had been made.[154] This kind of argument could be extended to English instrument making, to convince the audience that it still held the dominant position that it had in the eighteenth century, when names such as Ramsden, Bird, and Dollond were indisputably the most famous in the world. As early as 1825 however we can find correspondence between philosophers that demonstrates that, whatever the published opinions of experts, all was not well with the English instrument making trade:

> The instruments made by Fraunhofer and Reichenbach are but little known in this country and in fact are known only by <u>description</u>; for I do not think that even one of his celebrated instruments was ever <u>seen</u> here. Under these circumstances, we are anxious to obtain all the information respecting them that we are able, in order that we may stimulate our countrymen to similar excellence.
> With respect to the manufactory of glass, we are under considerable difficulties, on account of the high duty which is paid to Government, and which prevents our manufacturers from making experiments on the quality of such as is fit for telescopes. But, we have recently obtained a licence from Government, for a remission of duties on such

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[152] *Ibid.*, p.134.
[153] *Ibid.*, p.182.
[154] *Ibid.*, p.185.

experimental enquiries; and the Royal Society has appointed a Committee to superintend the execution of some specimens of large glass for the object end of telescopes. I hope, in a short time, we shall rival Fraunhofer's manufactory.[155]

In general, however, Airy promoted English instrument making as the best in the world. Only occasionally would he be swayed from this standpoint, as for example in 1842, when a request for advice concerning the purchase of a large equatorial came from a representative of Harvard University. Airy was unusually reserved in the information he gave regarding the skill of English makers:

> ...as to the division of the graduated limbs, any person who will read Troughton's paper on division (Phil.Trans.) and will set himself up with the cutting tools will do it as well as an instrument-maker.
> I do not think that large object glasses can be procured any where but at Munich, at present. Possibly however they may be found in Paris.
> ...I think it probable that Gambey at Paris or Merz at Munich would decide on a plan for mounting a large Equatoreal with less trouble of chance to the purchaser than any London artist.[156]

Such regard for foreign makers was not characteristic of Airy, and he rarely had anything but the highest respect for the best makers that England could offer, especially Simms. When he was granted the Freedom of the City of London in 1875, in order to accept the accolade Airy had to become a member of a City Company, and the Worshipful Company of Spectacle Makers proposed themselves. This guild could claim as its members all manufacturers of mathematical and philosophical instruments

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[155] F.Baily to O.Struve, 12 June 1825, Tartu State University Archives, cited in P.Muursepp, "English Astronomers' Letters to their Russian Colleagues in the Scientific Library of the Tartu State University", *Vistas in Astronomy,* 1976 (20), pp.139-40, on p.139.

[156] Airy to J.Cranch, 26 May 1842, RGO6 159 f.106.

within the City, and no other body could have been more appropriate for a philosopher who had devoted so much time to instruments and their makers:

> I shall much value the association with a body whose ostensible title bears so close a relation to the official engagements which have long occupied me. I have had extensive experience both in arranging and in using optical and mathematical instruments, and feel that my own pursuits are closely connected with the original employments of the Company.[157]

<u>9. Conclusion.</u>

By the mid-nineteenth century the type of relationship between man of science and instrument maker that had existed in the work on the Northumberland Equatorial had become commonplace. The innovative element in the design of instruments was no longer provided by the makers themselves, as it had been in the eighteenth century by famous London makers such as Ramsden, Bird, Dollond, Sisson, and Graham, and John and Edward Troughton in the latter part of the century. Instead, it was provided by the philosopher, who designed the instruments, the maker becoming little more than a workman acting according to the philosopher's instructions. Airy, having designed several large instruments for Greenwich (and elsewhere) by mid-century, and William Simms, having executed these designs to the highest possible standard, provide perhaps the most visible example of this new philosopher/artisan relationship. As we have seen, Simms was able to provide valuable advice of his own during the design of the instruments, though as with other philosopher/artisan relationships, this advice was purely on a *practical* level, and

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[157] Airy, *op.cit.* (note 1), p.312.

did not touch upon the philosophical principles of any of the designs.

One of the reasons why this new type of relationship evolved was that the necessary industrialisation which the instrument making trade underwent in the early part of the century removed the individual element from instrument making, and instead of being able to concentrate on new developments in their instruments, makers were forced to devote their time to increasing the efficiency of their manufacturing processes and of their business in general. However, it would be wrong to think that this was the only factor at work in diminishing the makers' status in scientific terms. Airy's career shows us that philosophers themselves were keen to emphasise their expertise and value to the nation, and often this emphasis would implicitly exclude those such as instrument makers who might wish to aspire to elite status, and had indeed held such status in the past. Airy, as we have seen, endeavoured to make instrument makers subservient to the general goal of the advance of philosophical knowledge, in his dealings with them on behalf of the country's chief scientific establishment.

This characterisation by philosophers of themselves as of greater value to the nation and to mankind than other groups such as artisans is illustrated in many addresses to the British Association of the period, for example by Whewell, who made it clear that scientific discovery was the "preserve of the proficient".[158] It is often said that Airy was the first "professional scientist". Chapman, for instance, claims that "Airy went a long way towards creating the professional scientist...".[159] It is by no means clear that this was Airy's own intention so that such an analysis is misleading. However, it is true to say that Airy did carve out a role for himself as a member of an elite group of philosophers who could use their expertise for the benefit of their nation, and who therefore deserved to be rewarded in financial terms. The instrument makers' contribution in terms of the benefit which accrued to the nation was not so considerable or vital, and therefore they were excluded from this elite.

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[158] S.Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation", *Science in Context*, 1988 (2), pp.115-47.

[159] Chapman, *op.cit.* (note 26).

CHAPTER SIX

MICHAEL FARADAY AND THE ROYAL INSTITUTION

The previous two case studies in this work have been concerned with men of science who had an educational background in the Cambridge Mathematical Tripos - Charles Babbage and George Airy. As such the notion that they occupied a privileged position in society, regardless of their achievements in science, would not have been alien to them, and accordingly their self-characterisation as members of a vital group to society and to mankind, which I have argued for, need not be considered a giant ideological step. The earlier chapter on Charles Wheatstone showed that it was also possible for a man to attain membership of the scientific elite without having the initial advantages which a University education might provide, and that a strong motivation towards self-improvement could result in the achievement of a position in what one could then regard as one of the most important strata in society. The present chapter deals with a figure more akin to Wheatstone than to Babbage and Airy, in educational background at least, Michael Faraday.

In choosing to study Faraday my aim is to show the diversity of actors who were seen as members of the scientific elite, but also to show that, despite their major differences of outlook, certain views on science and the position of its cultivators (notably for this thesis with respect to instrument makers) drew them together into a recognisable social grouping. Faraday, however, presents a particularly special and problematic case for this thesis, not least because he has been so extensively studied by historians of science, and indeed general biographers. There is a danger to the modern historian of relying too heavily on the readily available hagiographical accounts of his life and disregarding his own writings.[1] The bicentenary of his birth in 1991 will bring, one expects, another set of articles of varying degrees of merit.[2] The way in which Faraday presents a problem for this thesis as a whole is that his religious and moral values, and the way these impinged upon all aspects of his life, make it difficult to envisage him, at least as he is usually characterised, craving after status in society as a member of the scientific elite. However I would not wish to maintain that he did in any way *crave* after status, only that it was a concern to him, as it was for other members of the elite.

Faraday is generally characterised as an individual driven to unlock the truths of nature by some higher calling - a devotion to his God, and as performing his life's work without personal motive or interest, this being the subject of much of the praise heaped upon him by hagiographers.[3] He was a member of the Sandemanians, a sect whose chief aim was to live their lives as closely as possible to the example laid down by Jesus in the

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[1] For example J.Tyndall, *Faraday as a Discoverer,* (London, 1868); H.Bence Jones, *The Life and Letters of Faraday,* (2 volumes, London, 1870). More recent examples are S.P.Thompson, *Michael Faraday. His Life and Work,* (London, 1898); R.Appleyard, *A Tribute to Michael Faraday,* (London, 1931); J.Kendall, *Michael Faraday. Man of Simplicity,* (London, 1955). A more balanced appraisal of Faraday is provided in L.P.Williams, *Michael Faraday. A Biography,* (London, 1965); J.Agassi, *Faraday as a Natural Philosopher,* (Chicago, 1971); and also the collection of articles in D.Gooding and F.A.J.L.James (eds.), *Faraday Rediscovered,* (London, 1985).

[2] One project whose fruition will be most welcome is that to publish the complete Faraday correspondence, undertaken by the staff of the Royal Institution.

[3] J.H.Gladstone, *Michael Faraday,* (London, 1871), pp.60ff.

Bible.[4] As this religion was very much a full-time occupation, it is not difficult to see that it would of necessity impinge upon his science, even though Faraday claimed to keep the two domains separate.[5] However, as might be expected, such a religion discouraged the kind of personal ambitions which I have argued were among the motivations of individuals such as Wheatstone, Babbage, and Airy, and which led them to characterise themselves as an elite group in society. Yet Faraday was clearly as integral a member of any scientific elite, in terms of work published, and in terms of the contemporary importance attached to his work, as these three were. Admittedly he avoided taking the role in scientific politics that some of his contemporaries did,[6] so that in some respects he was not a fully participating member of the scientific community, but any definition which cannot account for Faraday's position in the scientific elite must be incomplete.

My aim in this chapter is to demonstrate that Michael Faraday's ideology cannot be simply described as that of a God-driven individual engaged in a disinterested search for truth, but that he did have beliefs concerning the position men of science should hold in society, and to this extent, though he did not believe in *craving* after status (and I have not maintained that the other members of the elite did this either) he did believe in the importance to the State of men of science above

[4] G.N.Cantor, "Reading the Book of Nature: The Relation Between Faraday's Religion and his Science", in Gooding and James, *op.cit.* (note 1), pp.69-81, on p.69. Also Thompson, *op.cit.* (note 1), p.298.

[5] Cantor, *op.cit.* (note 4), p.70.

[6] Williams, *op.cit.* (note 1), pp.355-7.

other groups. Therefore he fits in with the characterisation of the emergent scientific elite with which this thesis is concerned, and the rest of this chapter will deal substantially with Faraday's position in that group and his perception of it. This study of my fourth individual member of the emergent elite will then lead on to a chapter concerned with what can be seen as the institutional manifestation of the motives of that group: the British Association for the Advancement of Science.

1. The Royal Institution.

Any consideration of Faraday's work must take into account the institutional context in which it was carried out, which for most of his life was the Royal Institution in Albemarle Street. Faraday's inextricable links with the Royal Institution are perpetuated to this day, most visibly in the Christmas lectures for young people for which he was so famous.

The policies of the Royal Institution Managers from day one emphasised the importance of science and its cultivators to society, through the medium of agriculture, with which the initial founders were concerned.[7] They saw "science" as being a vital agent of progress in agriculture and industry, whether it helped in experiments on new crops and machinery, construction of canals and bridges, or any other practical projects.[8] Thus their conception of science was much concerned with the *economic* advantage to be gained by its employment in the solution of

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[7] M.Berman, *Social Change and Scientific Organisation. The Royal Institution 1799-1844,* (London, 1978), p.45.

[8] *Ibid.*

practical problems, an ideology of the utility of science which was to gain wide currency slightly later in the century but which was rather unique in 1800 (the Institution was founded in 1799). Benjamin Thompson, Count Rumford, the prime mover in the foundation, recognised the central role which had to be ascribed to the man of science in economic development, while realising that other agencies besides pure thought were necessary to the fulfilment of practical projects:

> It is the business of these philosophers to examine every operation of nature or of art, and to establish general theories for the direction and conducting of future processes. Invention seems to be peculiarly the province of the man of science; his ardour in the spirit of truth is unremitted; discovery is his harvest; utility his reward. Yet it may be demanded whether his moral and intellectual habits are precisely such as may be calculated to produce useful practical improvements. Detached as he usually is from the ordinary pursuits of life, little if at all accustomed to contemplate the scheme of profit and loss - will he descend from the sublime general theories of science and enter into the details of weight, measure, price, quality? Are his motives and his powers equal to this task? Surely they are not. The practical knowledge - the stimulus of interest - and the capital of the manufacturer are here wanting, while the manufacturer on his part, is equally in want of the general information and accurate reasoning of the man of science.[9]

The recognition of the status to be accorded to one capable of undertaking scientific research with a view to practical application is clearly of some importance to this thesis, as I have argued that the members of the emergent scientific elite saw the value of their work in terms of economic advantage to the State as perhaps the vital legitimation of their work and worth.

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[9] Rumford's Prospectus for the Royal Institution, quoted in G.Caroe, *The Royal Institution. An Informal History,* (London, 1985), p.11.

I do not wish to maintain that the men of science immediately tailored Rumford's words to their needs and proclaimed a position for themselves as a superior group in society. The emergence of the scientific elite, both in the eyes of its members and of society, was much more gradual than that - I have argued that it spread over the first half of the century, and that it was contemporaneous with the decline in status of the scientific instrument maker. But Rumford's prospectus certainly provides us with a picture of the role perceived to be occupied by the man of science in the institution in which Faraday would come some years later to do his life's work.

While the initial objects of the Royal Institution were "diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements" and "Teaching by courses of philosophical lectures and experiments the application of science to the common purposes of life",[10] the importance of agriculture and industry in the Institution waned, and financial difficulties and Humphry Davy's own preferences turned it, after about a decade, primarily into a centre for chemical research and popular scientific lectures.[11] Also, as Berman shows, thanks to William Thomas Brande and the activities he encouraged such as a journal, textbooks, consultancy into problems like gas illumination, commercial chemical analyses and so on, the Royal Institution metamorphosed

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[10] H.Bence Jones, *The Royal Institution. Its Founder and its First Professors,* (London, 1871), p.121.

[11] Williams, *op.cit.* (note 1), p.19.

into a site for the scientific management of social problems.[12] Again, the man of science was accorded a primary position, and it was in this context and into this Royal Institution that Michael Faraday was to be appointed in 1813.

Before this, the teaching by courses of philosophical lectures, central to the initial objects of the Institution, provided Faraday's first contact with Albemarle Street, as he was able to attend a series of lectures given by Humphry Davy.[13] In order to carry out the lectures prescribed in the initial objects of the Institution, it had been resolved that:

> ...a lecture room will be fitted up for philosophical lectures and experiments, and a complete laboratory and philosophical apparatus, with the necessary instruments, will be provided for making chemical and other philosophical experiments.[14]

The room was never to be used for any purpose other than lectures in natural philosophy and philosophical chemistry. In May 1801 Rumford announced that he had found a "sober, steady, single mathematical instrument maker", who was to be given a room in the house and a salary of £80 a year.[15] In 1808 this post was filled by John Newman,[16] who, as we have seen earlier in this thesis, was later to obtain his own shop, though he remained maker to the Institution. I shall discuss Newman's work for the Royal Institution at more length later in this chapter.

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[12] Berman, *op.cit.* (note 7), p.155.
[13] Williams, *op.cit.* (note 1), pp.25-6.
[14] Bence Jones, *op.cit.* (note 10), p.121.
[15] *Ibid.,* p.181.
[16] F.Greenaway, M.Berman, S.Forgan and D.Chilton (eds.), *Archives of the Royal Institution in Facsimile. Minutes of the Managers' Meetings,* (London, 1971), entry for 7 April 1823, where Newman himself refers to his appointment of 1808.

In 1812 the apparatus and models belonging to the Institution, which had been under the care of Davy, became the responsibility of William Pepys,[17] who was designated "Honorary Inspector of the Models and Apparatus". Newman was put under him, and shortly afterwards it was decided that William Payne, formerly the laboratory boy, should be employed along with Newman in cleaning and repairing the apparatus.[18] It was in February 1813 that Payne was dismissed after Newman alleged that Payne had struck him, after being accused of neglecting his duties in being absent when he should have attended on Brande.[19] Payne's ten years of service thus came to an end, and he was replaced by Michael Faraday. Although the story of Faraday's appeal for a job is well known,[20] a less well known aspect of the matter is that he owed the appointment in some sense to the allegations of an instrument maker, John Newman.

2. Faraday's Education.

Most biographers of Faraday choose to emphasise that he was self-educated,[21] and therefore that his achievement was all the greater *because* he did not have the advantage of a prosperous background and a University education. This assumes that it is unproblematic that a privileged background would have made it easier for Faraday to do his work, and thus the fact that he

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[17] Bence Jones, *op.cit.* (note 10), p.305.
[18] *Ibid.*
[19] *Ibid.*
[20] Most of the early biographies of Faraday find his application for scientific employment of interest, for example W.L.Randell, *Michael Faraday,* (London, 1924), pp.33-40.

[21] *Ibid.,* pp.20-7.

achieved so much must mean that he possessed real "genius".[22] This thesis is not concerned with whether or not the Cambridge Mathematical Tripos gave one an automatic advantage in life if one wished to study science: it should be apparent that in certain types of experimental work a mathematical train of thought may be a hindrance, and so it *may* have been the case that Faraday's own relative ignorance of mathematics was a help to his electrical researches in that he was not *confined* to any scheme of thought by his education. The point to be made is that value judgements about the benefit to be gained from a given educational background are dangerous and potentially distorting. I simply wish to point out that membership of the scientific elite could be attained from a number of different starting points - rich and university-educated (Babbage), poor and self-educated (Faraday), or somewhere in between (Airy and Wheatstone), and that such attainment from any particular origins should not be judged to have represented greater "genius" than from any other origins. Indeed a university education was still comparatively rare in a man of science so that Faraday's "self-education" was by no means unique, and prosecution of scientific research by someone self-taught no guarantor of genius.

After learning the rudiments of reading, writing, and arithmetic at school, Faraday was apprenticed to a bookbinder, and it was his ingestion of the contents of books which he was asked to bind which provided him with his first scientific

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[22] One of the most blatant of eulogies is Tyndall, *op.cit.* (note 1), p.172.

knowledge.[23] The most important works which he claimed to have been influenced by were Watts' *The Improvement of the Mind,*[24] Marcet's *Conversations on Chemistry,*[25] and the electrical treatises in the *Encyclopaedia Britannica.* Later he read George Adams the younger's *Essay on Electricity,*[26] and performed the experiments on static electricity described in that work. More selectivity and coherence were given to his studies by his attendance at lectures, firstly at the City Philosophical Society, and later at the Royal Institution (leading to his appointment there in 1813 after attending a series of Davy's lectures and taking copious notes, which he showed to Davy as an introduction). The City Philosophical Society gave Faraday an opportunity to look at experimental apparatus which he could not hope to buy for himself.[27] It became his habit to draw any such apparatus along with his notes and to examine its construction at the end of the lecture, and it was at such lectures that Faraday first saw the action of the voltaic pile and the galvanic trough.[28]

So, to characterise Michael Faraday as a self-educated genius whose success arose from an immense curiosity, intuition, enthusiasm, and dexterity, is at best misleading, at worst untrue. His self-education, in terms of his selection of books to

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[23] Gladstone, *op.cit.* (note 3), pp.2-3.
[24] I.Watts, *The Improvement of the Mind,* (London, 1809).
[25] J.H.Marcet, *Conversations on Chemistry,* (3rd edition, London, 1809).

[26] G.Adams the younger, *An Essay on Electricity,* (London, 1784).
[27] L.P.Williams, "Michael Faraday's Education in Science", *Isis,* 1960 (51), pp.515-30, on p.526.

[28] *Ibid.*

read, was confined to a relatively short period after which his education "proper" could begin in the traditional manner, attendance at lectures.[29] Admittedly his attendance at lectures was motivated by the same desire for self-improvement, and while there was not the structure provided by examinations and degrees, attending a lecture at the City Philosophical Society or the Royal Institution represented education just as much as did going to a lecture at Cambridge with a view to taking the Mathematical Tripos, and perhaps more so as far as science was concerned, given the abstract nature of much of the mathematics taught at Cambridge at the time. In addition, what apprenticeship as a bookbinder provided Faraday with was the opportunity to develop his manual skill. Williams refers to Faraday's "extraordinary skill"[30] in laboratory manipulation and claims that his ability to construct instruments he required for his research with his own hands "contributed greatly to his success as a scientist".[31] I would like to challenge this view by emphasising the fact that Newman is known to have constructed many one-off instruments for him,[32] thus presumably giving Faraday *time* to concentrate on research, rather than be held back having to construct all his own instruments. Thus for Williams to assume that Faraday achieved more in science because he could work with his hands is an over-simplification. However,

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[29] *Ibid.*, p.530.
[30] Williams, *op.cit.* (note 1), p.11.
[31] *Ibid.*
[32] M.Faraday, *Faraday's Diary,* (7 volumes and index, London, 1934). The index gives some indication of the frequency of his dealings with Newman. Henceforth this will be referred to as *Diary.*

it is undeniable that the ability to work with his hands did get full opportunity to develop in the bookshop of Riebau, his master.

As we have seen in previous chapters, the early nineteenth century man of science was usually gifted with his hands, and Wheatstone, Babbage, and Airy are all known to have constructed their own instruments when they did not wish to employ an instrument maker. Faraday, however, found himself in a different institutional context to these three as, in Newman, he had a specific instrument maker on whom to call for any manual work which might be required. Even so, he seemed often to prefer doing such work himself to delegating it to an instrument maker. As Appleyard points out, in referring to Faraday's *Chemical Manipulation:*

> His notes on lighting, heating, and ventilation, the construction of shelves, cupboards, sinks, furniture, and laboratory supplies are all of permanent value. They indicate how little he employed an instrument maker when he could do things with his own hands... Glass blowing, glass grinding, and other laboratory arts were constantly being improved by him.[33]

Faraday's *Diary* contains many suggestions of the complexity of the manual work he was able to perform to assist with his researches - for example in discussing a balance being made to investigate terrestrial magnetism, he mentions that for the suspension of the balance "...I have made a bundle of 180 films of cocoon threads - it is two and a half inches long...".[34] The

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[33] Appleyard, *op.cit.* (note 1), p.146; M.Faraday, *Chemical Manipulation: Being Instructions to Students in Chemistry,* (London, 1827).

[34] *Diary,* V, p.352.

rest of this balance seems to have been made by Newman.[35] Among his contemporaries Auguste De La Rive noted his practical skill as something special:

> ...in him the hand marvellously seconded the head; he was of remarkable dexterity, and possessed a practical talent, rare and precious in men of science, which enabled him, when necessary, to construct and modify his apparatus for himself, with the view of attaining with more certainty the desired result.[36]

Faraday's education, then, may be considered to have been two-tiered: a philosophical education based on attendance at lectures, and a manual education based on his work as a bookbinder. His early manual education was furthered by his attempts to construct apparatus, which he was first stimulated to do by reading Priestley's *The History and Present State of Electricity.*[37]

Faraday's enterprise as a whole, however, along with his friends Abbott and Huxtable,[38] may be seen as one of self-improvement.[39] My contention in this chapter is that although Faraday can be seen as a deeply religious individual working in a disinterested way to uncover truth, he is more accurately seen as a typical individual of the early part of the nineteenth century seeking to realise his full potential by one of the many avenues

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[35] *Ibid.*

[36] A.De La Rive, "Michael Faraday, his Life and Works", *Philosophical Magazine,* 1867, pp.409-37, on p.434.

[37] Williams, *op.cit.* (note 1), p.14; J.Priestley, *The History and Present State of Electricity,* (2 volumes, London, 1767).

[38] Benjamin Abbott was Faraday's best friend as a young man. He worked as a clerk in a counting house. After Abbott, Thomas Huxtable was Faraday's next closest acquaintance.

[39] T.Martin, *Faraday,* (London, 1934), p.16.

by which it was possible to attain social status - namely scientific research. Thus I would argue that there *was* a personal motive in Faraday's scientific work - the motive of self-improvement which transcended any moral "quest for truth".

Like Babbage, for example, Faraday believed that the opportunity to improve oneself through science should be made available to all through scientific lectures, and not to a privileged few through a university education.[40] Faraday's own ideology with respect to the diffusion of science to the people thus corresponded to that of the Royal Institution itself, as one might expect. It was central to Faraday's conception of science that it should not be the sole property of the scientific community or elite. Something which provided a knowledge of God's creation, like science, had to be shared. But even though knowledge ought not to be confined to the elite, this should not diminish the importance of the elite, the *creators* of the knowledge, in the eyes of society.

In keeping with this ideology, Faraday did not advocate the continued dominance in the educational system of schools and universities by classics and mathematics, but instead emphasised the importance of a knowledge of the laws of science which when applied had contributed to the shaping of the modern world in the form of the electric telegraph (after 1837), the steam engine, and the railway system.[41] Science and its applications, not

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[40] Williams, *op.cit.* (note 1), pp.329-32.
[41] *Ibid.*

classics, he saw as the support of modern culture and as such it was ludicrous for there to be supposedly educated men who did not have a knowledge of the basic laws of nature.

3. Faraday and the Advantages of Science as a Career.

Having discussed how Faraday came to be involved in scientific work and dealt briefly with his ideology regarding scientific education and his desire for self-improvement, I would now like to concentrate on his perception of science as his life's work, and in particular how being a member of an elite group in society can be seen to fit in with his religious views rather than contradict them as might be expected.

It is not intended in this chapter to discuss the specific issues raised by Faraday's science, nor indeed to talk about the content of his theories. There is already a wealth of research on such topics, with several historians of science at the moment having as their main research interest the content of Faraday's science.[42] My aim is rather to place Faraday in his correct social context, which means characterising the position which he was seen as occupying, and which he saw himself as occupying, among the scientific elite. In placing him in this context one of my aims in this thesis as a whole - to account for the decline in status of the instrument maker in terms of the emergence of a new elite group in science and society - will be some way to being achieved.

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[42] The number of contributors to Gooding and James, *op.cit.* (note 1), shows this. Most of these authors have written other works on Faraday in addition to their contributions to this volume.

Faraday's initial choice to give up trade as a bookbinder and enter upon a life's work in science was effectively made in 1813 upon the dismissal of Payne and the offer of a job as laboratory assistant at the Royal Institution. It is likely that the mundane nature of his trade may have caused Faraday to be attracted to the variety he saw in scientific work, notwithstanding the way in which he perceived science to be morally uplifting and the quest for truth one of the most praiseworthy of occupations. He referred later to "Trade which I hated, and science which I loved".[43] However, later experience showed that science was not the *ideal* he had expected it to be in 1813:

> When I quitted business, and took to science as a career, I thought I had left behind me all the petty meannesses and small jealousies which hinder man in his moral progress; but I found myself raised into another sphere, only to find poor human nature just the same everywhere - subject to the same weaknesses and the same self-seeking, however exalted the intellect.[44]

Even so, the language in which the above is couched shows why Faraday *chose* science, even if the behaviour of those in the scientific community was not what he would have expected. One motive which one would assume did not characterise the scientific community as it did the business community was financial greed. The Sandemanian doctrines on indiviual wealth were fairly straightforward. They rejected the common ideology that accumulation of wealth ought to be man's aim.[45] This did not mean

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[43]Bence Jones, *op.cit.* (note 1), Volume 1, p.209.
[44]Faraday speaking to Mrs.Crosse, cited in Gladstone, *op.cit.* (note 3), p.89.

[45]Berman, *op.cit.* (note 7), p.157.

that Sandemanians could not engage in business activities, only that they should not regard their business as their *raison d'etre.* There was therefore nothing incongruous about Faraday pursuing a trade, and it is clear that he conformed to the Sandemanian guidelines concerning the non-accumulation of wealth whilst performing his work as a bookbinder. However, the pursuit of science as his life's work can be considered to have been a more satisfying occupation for a Sandemanian, in that it involved studying God's creation. Science provided him with enough money to live comfortably, which was all the worldly wealth a Sandemanian was expected to have, but its pursuit provided Faraday with a sense of moral and religious purpose which he could not attain through trade. When a publisher asked him in 1859 to publish some of his lectures to children, he replied that:

> ...money is no temptation to me. In fact, I have always loved science more than money; and because my occupation is almost entirely personal, I cannot afford to get rich.[46]

This picture of Faraday as divorced from any capitalist motives owing to his religious beliefs is not without its problems, either for the coherent interpretation of Faraday or for this thesis as a whole. I have shown in previous chapters that the men of science in this period used the notion of the accumulation of national wealth by the application of scientific knowledge as an important legitimation of scientific pursuits themselves, but it has just been stressed that Faraday

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[46] Faraday to W.Smith, 3 January 1859, cited in Bence Jones, *op.cit.* (note 1), Volume 2, p.423.

repudiated the idea of individual wealth on the basis of his Sandemanianism. It could also be pointed out that he was involved in a wide range of practical projects which served the capitalist economy, projects such as commercial chemical analyses, research on lighthouses for Trinity House, and so on.[47] The problems raised therefore, are two-fold; how to reconcile Faraday's pursuit of these projects with his belief in not accumulating wealth, and how to bring Faraday's ideology with regard to the use of science to create wealth into line with the other members of the scientific elite who saw this economic value as one of the great indicators of their worth.

Berman resolves the problem by postulating the existence of two Faradays, a worldly Faraday involved with these projects, and an inner, religious Faraday who presumably discouraged such involvement.[48] This resolution is unsatisfactory, and indeed unnecessary if it is realised that Faraday only repudiated the idea of the accumulation of individual wealth, so that utilising science for national economic gain was not necessarily a sin for him. Indeed, it was good that science could be made available for the benefit of mankind and the nation, provided the man of science did not wish to profit by its use. We can thus see that to utilise science for economic gain for the State, and the benefit of mankind is not a problem for the interpretation of Faraday's ideology, and as for the other men of science discussed in this thesis, the utility of science in economic terms could be an important legitimation of his activity. What

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[47]Berman, *op.cit.* (note 7), pp.162-84.
[48]*Ibid.*, pp.156-9.

Faraday did reject, however, was the market-place spirit which he thought could come to dominate science, if men of science were persuaded to be at the service of entrepreneurs. It was very important that, when God's creation was being studied, this did not happen.

So, although there is some validity in standard hagiographies which have stressed that Faraday was special even among men of science, in the disinterested and God-driven way he sought truth, in this chapter I have aimed to stress the similarity of his ideology to that of other members of the scientific elite. In common with Wheatstone, Babbage, and Airy, Faraday always styled himself as a *philosopher,* disliking words such as "scientist" and "physicist".[49] He always had a clear notion of what the *ideals* were for a *philosopher* - for example his early lecture notes for the City Philosophical Society included the following:

> The philosopher should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biased by appearances; have no favourite hypothesis; be of no school, and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature.[50]

This conscious idea of self-identity among the philosophical elite was paralleled in Faraday by his identity as a member of another group, the Sandemanian community. The religious group to which he belonged was given coherence by common goals and

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[49]Agassi, *op.cit.* (note 1), p.3.
[50]Early Lecture Notes for the City Philosophical Society, cited in Martin, *op.cit.* (note 39), p.47.

values in life, and the same could be said of the philosophical community which served him as a social, rather than a religious, network, and which Faraday saw as having certain values. Whether or not all members of the community subscribed to the values Faraday himself held is of course debatable, but the important thing is that Faraday believed that an elite did exist which ought at least to have the values he believed in as their ideal.

Above all, then, in the search for truth the philosopher ought not to crave worldly reward. One member at least of the philosophical elite had used science to achieve worldly ambitions, in Faraday's view - Humphry Davy.[51] The ideal for the philosopher had to be the engagement in the search for knowledge of God's creation without the goal of worldly reward. Science had to be its own reward. This does not mean that Faraday believed that science should not confer status. That would set him apart from every other philosopher I have discussed in this thesis. He did believe that science should not be abused, as he thought Davy had done, in order to achieve worldly ambitions, but the notion of status in society and recognition by one's peers for work one has done is something different to the realisation of worldly goals, and it was only the craving after such status which Faraday discouraged. If it was deservedly achieved, then one ought to be able to enjoy it.

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[51] For a recent study of their relationship, see D.M.Knight, "Davy and Faraday: Fathers and Sons", in Gooding and James, *op.cit.* (note 1), pp.33-49.

4. Faraday and the Instrument Making Community.

In the earlier chapters of this thesis I have referred to the views of certain members of the scientific elite with respect to honours conferred upon them by society, and I will discuss Faraday's view of scientific honours and their relation to the status of the philosopher later in this chapter. Having considered his perception of the identity of the philosophical community, I would like to concentrate on how this community related for Faraday to the instrument making community. Before considering his collaboration with instrument makers it is worth questioning the extent to which collaboration *per se* was a feature of Faraday's scientific career. The answer to this is that Faraday was very much a solitary worker in science. Many of his researches were in areas with which no other workers, British or foreign, were primarily interested. The main reason for this was that Faraday had a poor memory, particularly after his breakdown in the early 1840s,[52] so that it became difficult for him to keep abreast of new research in popular areas of inquiry, tending to forget what he had read and meaning that in areas where nobody else was involved he could more easily be sure that his work was original.[53] Even disregarding this reason for his researching as an individual, he preferred working on his own to collaboration. His researches he constantly found to be of too individual a character to allow him to delegate any part of them to another, and he was never

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[52]See for example Berman, *op.cit.* (note 7), p.160, and Williams, *op.cit.* (note 1), pp.358-9.

[53]Hence his work on subjects such as the magnetisation of light.

satisfied when an experiment of someone else was reported to him - he had to perform it himself.[54] Tyndall emphasised the power of "lateral vision" which Faraday possessed and which was responsible for much of his success, and a power such as this could not be delegated to any mere assistant.[55]

When we come to Faraday's collaboration with instrument makers, then, we might expect instances to be fairly rare, especially given his known manual skill, and this is indeed the case. Correspondence with instrument makers is almost non-existent in any of the collections of Faraday's letters, published and unpublished.[56] Part of the reason for this lies in the fact that the one instrument maker whom he did employ fairly extensively, John Newman, worked for him on a regular basis so that personal contact between the two obviated the need for written correspondence. But the main reason for the dearth of correspondence is simply that Faraday did not have dealings with large numbers of makers. A study of his relationships with those with whom he did do business does however provide some insight into his views on the group as a whole.

John Newman, as has already been mentioned, was directly involved in the incident leading to the vacancy at the Royal Institution which Faraday filled in 1813.[57] The relationship between the two seems to have become quite close even after a short time with the maker as Faraday's immediate

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[54] Thompson, *op.cit.* (note 1), p.242.
[55] *Ibid.*
[56] Manuscript Index of the Complete Faraday Correspondence, Royal Institution.

[57] Bence Jones, *op.cit.* (note 10), p.305.

superior, for when Faraday accompanied Davy on his European tour in 1813-15,[58] he wrote to his friend Abbott:

> There are two persons nearly strangers to my mother to which if you would go I would be much obliged Mrs.Greenwell... + Mr.Newman of Lisle St. to whom I feel grateful for his readiness in communicating to me such things as were useful and instructive and whose success in life is I hope proportioned to his merits.[59]

From this letter is apparent not only Faraday's regard for Newman but also the fact that the latter had acquired his own premises. Some months later, with a concern for the financial state of his parent body, Faraday wrote again to Abbott mentioning the maker: "I hope that if any change should occur in Albemarle Street Mr.Newman would not forget my books I prize them now more than ever".[60]

The frequency and familiarity with which Newman is mentioned in Faraday's *Diary* give some indication that this close relationship continued through each man's career - the last entry referring to Newman occurs in August 1857,[61] but indicates that he was still working for the Royal Institution then, even though he would have been quite old. As we have seen earlier in this thesis, Newman not only made instruments, he published accounts

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[58] For accounts of this tour, see Williams, *op.cit.* (note 1), pp.31-42, and L.P.Williams (ed.), *The Selected Correspondence of Michael Faraday,* (2 volumes, London, 1971), pp.59-92. Henceforth this will be referred to as *Correspondence.*

[59] Faraday to B.Abbott, 1 May 1814, *Correspondence,* p.64.
[60] Faraday to Abbott, 30 November 1814, *Correspondence,* p.78.

[61] *Diary,* VII, p.294, (17 August 1857).

of them as well,[62] and particularly useful to him as a publishing vehicle was the Royal Institution's own journal, the *Quarterly Journal of Science, Literature, and the Arts,* which usually ran to about 1000 copies.[63] It can therefore be assumed that his appointment as official maker to the Royal Institution benefitted Newman not only in that it was an impressive epithet for his trade card but also because through the journal his work could effectively be advertised to a large readership of potential buyers. Surprisingly, on his trade card, Newman only called himself "Maker to the Royal Institution" from 1823 onwards, having applied to the Managers in that year for permission to do so.[64]

Faraday's relations with makers other than Newman were mainly limited to those who had some interest in the burgeoning field of electrical research. Edward Montague Clarke represented one of the most visible branches of a community of individuals interested in electrical science in London in the 1830s, when Faraday was perhaps at his most prolific.[65] Clarke's instrument

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[62] J.Newman, "An Account of an Improved Blow-Pipe", *Quarterly Journal of Science, Literature and the Arts,* 1817, pp.379-82; J.Newman, "On a Mountain Barometer constructed with an iron cistern", *Quarterly Journal of Science, Literature, and the Arts,* 1823, pp.277-9.

[63] This journal was originally known as *Journal of Science and the Arts* from 1816-19, as *Quarterly Journal of Science, Literature, and the Arts* from 1819-30, and as *Journal of the Royal Institution* in 1831. The estimate of the circulation derives from Berman, *op.cit.* (note 7), p.143.

[64] Greenaway et al., *op.cit.* (note 16), entry for 7 April 1823.

[65] I.R.Morus, *The Politics of Power. Reform and Regulation in the work of William Robert Grove,* (Ph.D. Thesis, Cambridge University, 1989), pp.12-46 contains the most thorough account of this group.

maker's shop in Lowther Arcade was the site of meetings held by the group of practitioners who were to form the short-lived Electrical Society of London in 1837,[66] and although Faraday did not join the Society he did have some contact with certain of its members through the shared interest in electrical science. Clarke's pretensions to membership of the philosophical community, rather than the instrument making community or the community of electrical practitioners,[67] have been discussed at some length in the chapter on Wheatstone, but it is interesting to consider Faraday's isolated references to the maker's ideas:

> Mr.Clarke showed me to-day several of his results of shocks, etc. obtained by his Magneto Electric Machine. Most of them are related to the Magneto Electric Induction which I have given in to the Royal Society. He thought there was a shock both on making and breaking contact, but I tried by a cup of mercury , etc. and found there was <u>none</u> on making - only on breaking contact.[68]

This indicates that Saxton was not the only one to question Clarke's claims to knowledge and discovery, though clearly Faraday's criticisms were much less explicit and self-motivated. Interestingly Saxton's countryman Joseph Henry seems to have had quite a high opinion of Clarke's work and knowledge.[69]

One of the sites which the group of electrical practitioners frequented, besides Clarke's shop, was the Adelaide Gallery of Practical Science in The Strand. Faraday's *Diary* indicates that

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[66] *Ibid.,* pp.26-46 deals with the life of the Society.

[67] The contrast between the ideologies of the philosophical community and the community of electrical practitioners will be discussed later in this thesis.

[68] *Diary,* II, pp.357-8.

[69] B.Gee, "Joseph Henry's Trade With Instrument Makers in London and Paris", *Bulletin of the Scientific Instrument Society,* 1990 (25), pp.19-24, on p.19.

he was a regular visitor to see experiments performed there, especially in 1838, when he was involved with experiments with the Gymnotus (a type of electric fish)[70] and witnessed the attempted operation of an apparatus designed by Wheatstone,[71] which Bowers suggests also concerned the Gymnotus in some way.[72]

Present at each of these demonstrations was Francis Watkins, a maker who as we have seen had an interest in the theoretical side of electricity and a full interest in the principles behind the instruments he constructed. Watkins' *Popular Sketch of Electro-Magnetism*[73] was, like the work of Newman and Clarke, a typical instrument maker's written work in that it did not make new theoretical claims, but merely consolidated existing knowledge:

> The following pages are intended to convey to the reader a plain, clear, and concise account of the most important phaenomena of the science of Electro-Magnetism, or Electro-Dynamics; a science which ever since its discovery has engaged the attention of many eminently learned and scientific men in every civilised state. This science is of great importance to the philosopher and to the world; as it will probably lead to a more intimate, if not a perfect, knowledge of Electricity and Magnetism... No claim is made to originality in the science: all that is intended, is to lay the facts before the student in such language as will make the matter clear to him;

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[70] *Diary,* III, p.342.
[71] *Diary,* III, p.359.
[72] B.Bowers, *Sir Charles Wheatstone FRS, 1802-1875,* (London, 1975), p.66.

[73] F.Watkins, *A Popular Sketch of Electro-Magnetism, or Electro-Dynamics, with plates of the most approved apparatus for illustrating the principal phenomena of the science, and outlines of the parent sciences, electricity and magnetism,* (London, 1828).

> avoiding theoretical disquisitions, as well as those numerous theories which have been advanced, many of which, although ingenious and plausible, are very far from being satisfactory.[74]

Again at the end of the work Watkins stressed that only the *facts* of a science still in its infancy had been explained, and as one might have expected from an author who was also a tradesman, a catalogue of "Apparatus Constructed and Sold by Watkins and Hill for Illustrating the Most Striking Phaenomena of Electro-Magnetism" was appended, with descriptions of 24 different instruments made by the firm.[75]

Although there is not evidence that Watkins did much work for Faraday, then, he impinges on this work because they shared the social context of the Adelaide Gallery, and he is one of a number of makers (Clarke being the other obvious example) who saw participation in scientific investigations, even if only through watching, as an advantage to one's career.

The Adelaide Gallery, however, was not the most obvious site for the ambitious maker, because the Royal Institution itself could be seen to encourage some limited participation in its affairs by the artisan classes. Friday evening discourses were proposed, though seemingly without actually happening, by Charles Holzapffel on cutting tools and by Andrew Pritchard on microscopes.[76] A prominent maker who did deliver his discourses as promised was Edward Dent, who spoke on 7 February 1834 "On the Effects of Temperature on the Balance Springs of Time Keepers and the Means of Compensating the Errors",[77] and again on 7 April

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[74] *Ibid.*, p.1.
[75] *Ibid.*, pp.69-83.
[76] Lecture Note Book F4E, Royal Institution.
[77] *Ibid.*

1837 "On the Construction and Manufacture of Clocks and Chronometers".[78] These types of lecture were fully in accordance with the ideology that the Royal Institution had propounded since its foundation of making knowledge the property of everyone, an ideology that as we have seen was also Faraday's own. Among those who were invited to partake of the knowledge made available at the Royal Institution through the Friday Evening Discourses were prominent makers such as William Ladd, Hugh Powell, Andrew Ross, and Cornelius Varley,[79] in addition to those already mentioned.

The Friday evenings' entertainment was not solely confined to the lecture theatre, however, as many exhibits were on display in the library each week for the benefit of the assembly.[80] Such exhibits included not only samples of manufactured and natural products but also the latest wares of instrument makers, though it is important to realise that the items displayed did not necessarily have to bear any relation to the subject of the evening's lecture. Thus on 4 June 1847, Benjamin Collins Brodie discoursed "On the Polar Nature of Chemical Force", while a new construction of marine barometer was exhibited by Enrico Negretti.[81] In March 1848 R.G.Latham spoke "On the Ethnological Affinities of the Nations of Caucasus", and Watkins and Hill exhibited an aneroid barometer.[82] On 11 February 1848 there had been no lecture in the theatre, but there were still 6 exhibits in the library, including some microscopes by Cornelius Varley.[83]

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[78] *Ibid.*
[79] *Ibid.*
[80] Lecture Note Book JB2/3, Royal Institution.
[81] *Ibid.*
[82] *Ibid.*
[83] *Ibid.*

John Newman, as might be expected for the Royal Institution's official maker, frequently took advantage of these opportunities to advertise his latest products to an audience of potential customers by exhibiting on Friday evenings.[84]

The archive of the Royal Institution, in addition to this information regarding the active role taken by instrument makers in its weekly meetings, provides much information on the apparatus which belonged to the Institution at the various stages of its development. From such documents, some light is shed on the employment of Newman and his predecessor. A catalogue of "Models, Machines, and Chemical Apparatus" belonging to the Royal Institution, originally dated 1825 but updated to 1852[85] contains about 800 items, so that if Newman had made even a proportion of these, his appointment at the Institution would have been most lucrative. The instruments range in complexity from simple inclined planes with pulleys to electrical machines,[86] and thus represent varied levels of effort and expenditure of time for any maker, but most are items which one would expect a philosophical instrument maker to construct, though there are examples listed of manufactured products such as door locks and conical beer warmers. Another inventory, this one undated, lists the specifically physical and chemical apparatus belonging to the Royal Institution and intended for use along with the

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[84] *Ibid.*

[85] Catalogue of Models Machines and Chemical Apparatus belonging to the Royal Institution 1825, Royal Institution.

[86] *Ibid.*

professorial lectures.[87] Amongst upwards of 100 instruments listed, almost 50 of which were intended to complement the lectures on electricity, very few makers are mentioned so that one may reasonably assume that a significant part of the collection was made by Newman. The only exceptions to this are a large double air-pump, and a large cylinder electrical machine, both made by Nairne, and an electrical machine made by Cuthbertson.[88]

Newman not only made instruments for the Institution, he borrowed them as well; for example in 1856 he applied by letter for the loan of the "Pepys apparatus for freezing mercury",[89] and in 1859 he borrowed a mercurial trough for one month.[90]

From the foregoing it will be apparent that the Royal Institution provided one of the foremost scientific arenas in London for the instrument maker. The maker was able not only to do work for the Institution (though the opportunity for this was limited, given the effective monopoly of John Newman), he was also able to attend lectures along with representatives of "educated London", and to display his work to a large group of potential customers at Friday evening lectures. All this was consistent with the Royal Institution's ideology of making scientific knowledge an available commodity, and of displaying

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[87] Inventory of the Physical and Chemical Apparatus belonging to the Royal Institution (no date), Royal Institution.

[88] *Ibid.* Reference to Cuthbertson's bills is made in Greenaway et al., *op.cit.* (note 16), entry for 27 August 1810.

[89] Note Book F5B, Record of Apparatus Lent 1856-, Royal Institution.

[90] *Ibid.*

the products of science to a large audience. As the Royal Institution's most famous representative, Michael Faraday shared this ideology with regard to the instrument maker - it was right that he should be able to listen to lectures, and even to give them himself if he had expertise to impart to others. It was also proper that he be given the opportunity to display his work, even if it was to some degree with an eye to personal gain. But this regard for the skill and knowledge displayed by the instrument maker should not be confused in Faraday with a desire to accord him a status equal to the philosopher. As we have seen with Clarke, Faraday recognised that when it came to the proper domain of the philosopher, the instrument maker's expertise could be sadly lacking, and when presented with unsatisfactory work, Faraday felt no obligation to tolerate it. William Ladd found this out to his cost, when he constructed a large delicate mercurial thermometer for Faraday,[91] and after some weeks' work with it Faraday wrote:

> The instrument seems very imperfect and badly made. There is the appearance of a crack round the upper end near the extreme. Furthermore, the retreating mercury has left particles sticking in various parts of the tube... The instrument will not do.[92]

Without further ado, Faraday ordered another instrument, but of a different maker, Louis Casella, which proved more satisfactory to him,[93] and Ladd's failure was presumably noted for the future. The strict attitude towards failure here may appear harsh on Faraday's part, but if we recognise that his

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[91] *Diary,* VII, p.355.
[92] *Ibid.,* p.366.
[93] *Ibid.,* pp.367ff.

motive was not primarily to reprimand Ladd, but merely to do his work in the most efficient way possible, which meant trying someone else, Faraday's actions can be justified. Indeed, he was as acutely aware of his own limitations regarding his life's work, as he was those of Ladd:

> ...in all kinds of knowledge I perceive that my views are insufficient, and my judgement imperfect. In experiments I come to conclusions which, if partly right, are sure to be in part wrong; if I correct by other experiments, I advance a step, my old error is in part diminished, but is always left with a tinge of humanity, evidenced by its imperfection.[94]

However, this admission of fallibility did not preclude an implicit assertion of the primacy of the philosopher in the search for truth and scientific knowledge, and a belief in the vital role the philosopher consequently played in the progress of society. In the remainder of this chapter I would like to consider in more depth Faraday's view of the status the philosopher ought to be accorded in society, in accordance with my treatment of the subjects of the earlier case studies.

5. Faraday and the Status and Utility of the Philosopher.

One of the main doctrines of Sandemanianism is that one must be loyal to the crown and thus to the Government (which is really just an extension of the crown).[95] Faraday's involvement in a number of Government-encouraged scientific projects is thus easily explained. The work of the Royal Society Committee on

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[94] Faraday to E.Barnard, 23 July 1826, cited in G.Sarton, "Experiments with Truth by Faraday, Darwin, and Gandhi", *Osiris,* 1954 (11), pp.87-107, on p.101.

[95] This loyalty is noted for example in Williams, *op.cit.* (note 1), p.358.

Optical Glass has been mentioned earlier in this thesis. It was in response to the threat to English manufacturing from abroad, particularly Fraunhofer in Germany, that the Government and the Royal Society took steps to facilitate the improvement of the optical glass made in this country.[96] The Government's action was to remove the prohibitive excise restrictions, and to "bear all the expenses of furnaces, materials, and labour, as long as the investigations offered a reasonable hope of success".[97] The investigations were to be carried out by a committee appointed by the Royal Society, comprising Faraday, John Herschel, and George Dollond, though Faraday and Dollond performed most of the work. The importance of such work to the nation was forcefully stressed by David Brewster, who lamented the fact that the improvements of which he knew had been made by foreigners, and expressed his fear of the consequences for British science: no Englishman could be,

> ...without feelings of the most poignant regret, that England has now lost her supremacy in the manufacture of achromatic telescopes, and the government one of the sources of its revenue. In a few years she will also lose her superiority in the manufacture of the great divided instruments for fixed observatories. When these sources of occupation for scientific talent decline, the scientific character of the country must fall along with them, and the British government will deplore, when it is too late, her total inattention to the scientific establishments of

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[96] M.C.Usselman, "Michael Faraday's Use of Platinum in his Researches on Optical Glass", *Platinum Metals Review,* 1983 (27), pp.175-81, on p.177.

[97] M.Faraday, "On the Manufacture of Glass for Optical Purposes", *Philosophical Transactions of the Royal Society of London,* 1830, pp.1-57, on p.2. The work of the Optical Glass Committee has also been discussed in Chapter 2 of this thesis.

> the empire. When a great nation ceases to triumph in her arts, it is no unreasonable apprehension, that she may cease also to triumph by her arms.[98]

While such a pessimistic picture may not have been perceived by Faraday - after all, Brewster was one of the most ardent voices in the decline of science debate - he realised the value of his work to the nation and the importance that his religion attached to serving the nation to the best of his ability. He studied the scientific part of the investigation for six years, with George Dollond assisting by trying practically the good and bad qualities of the resultant glasses. From 1825-7 the work was carried out at the Falcon Glass Works of Pellatt and Green, but in 1827 it was transferred to a room and furnace specially constructed at the Royal Institution, in order that it be more directly under Faraday's control. By 1829 optical glass of some value had been produced, but it was not perfect, and though work continued in 1830 and 1831, Faraday gradually lost interest, realising that he could not reasonably expect to make further progress, and that he had done his utmost as a philosopher to help his nation in this particular project.[99] In July 1831 he wrote to Roget, the Secretary of the Royal Society, resigning from the committee as he wished to devote time to other pursuits.[100] It was later in this year that he discovered electro-magnetic induction.

Work on behalf of the nation in areas such as the optical

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[98] D.Brewster's editorial postscript to W.Struve, "Further Account of the large Achromatic Refracting Telescope of Fraunhofer in the University of Dorpat", *Edinburgh Journal of Science,* 1826 (5), pp.105-10, on p.110.

[99] Williams, *op.cit.* (note 1), pp.116-20.

[100] Faraday to P.M.Roget, 4 July 1831, *Correspondence,* p.199.

glass researches demonstrates Faraday's ardent belief that science must be utilised for the benefit of mankind, and consequently that the philosopher who had the expertise to develop science was a vital member of society. Though some of his motivation was a simple desire to uncover the truths of God's nature, more important was the fact that he believed such truths to be worth discovering because it was part of God's plan that they be there for the benefit of mankind. His discovery of electro-magnetic induction was a piece of "pure" science which in its future development perfectly vindicated Faraday's approach.

In his own Friday evening discourses for the Royal Institution, whose ideology, it must be remembered, had as a central notion the belief in the utility of science, he always stressed how simple scientific principles could have considerable economic results if well applied,[101] though as might be expected, the comfort of mankind was given precedence over the gratuitous accumulation of wealth, of which Faraday disapproved:

> The development of the applications of physical science in modern times has become so large and so essential to the well-being of man that it may justly be used, as illustrating the true character of pure science, as a department of knowledge, and the claim it may have for consideration by Governments, Universities, and all bodies to whom is confided the fostering care and direction of learning. As a branch of learning, men are beginning to recognise the right of science to its own particular place; - for though flowing in channels utterly different in their course and end to those of literature, it conduces not less, as a means of instruction, to the discipline of the mind; whilst it ministers, more or less, to the

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[101]Berman, *op.cit.* (note 7), p.173.

> wants, comforts, and proper pleasure, both mental and bodily, of every individual of every class in life.[102]

Although Faraday and others stressed the importance of research which did not necessarily have an obvious immediate practical application, to outsiders Britain seemed still very much preoccupied with instantaneous practical pay-off, as Liebig wrote to him in 1844:

> What struck me most in England was the perception that only those works which have a practical tendency awake attention and command respect, while the purely scientific works which possess far greater merit are almost unknown. And yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here in the eyes of scientific men no value or at least but a trifling one is placed on the practical results. The enrichment of Science is alone considered worthy of attention. I do not mean to say that this is better, for both nations the golden medium would certainly be a real good fortune.[103]

Having decided that Faraday did consider the development of scientific knowledge in order that it be used for the benefit of mankind as a morally valuable pursuit, I would like to conclude this chapter by looking in more detail at his view of the position of the man in society who could do this, the philosopher. I have already stressed that Faraday's religious convictions did not preclude a belief that the philosopher occupied a more important place in society than did other groups (such as instrument makers, and those not contributing by

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[102] M.Faraday, "On Wheatstone's Electric Telegraph's Relation to Science", *Proceedings of the Royal Institution,* 1854-8 (2), pp.555-60, on p.555.

[103] J.Liebig to Faraday, 19 December 1844, *Correspondence,* pp.429-30.

productive labour to national wealth), only that status and praise ought not to be *sought* after. In fact he believed that to be a philosopher at all absolutely required that such worldly goals be rejected:

> It puzzles me greatly to know what makes the successful philosopher. Is it industry and perseverance with a moderate proportion of good sense and intelligence? Is not a modest assurance or earnestness a requisite? Do not many fail because they look rather to the renown to be acquired than to the pure acquisition of knowledge, and the delight which the contented mind has in acquiring it for its own sake? I am sure I have seen many who would have been good and successful pursuers of science, and have gained themselves a high name, but that it was the name and the reward they were always looking forward to - the reward of the world's praise. In such there is always a shade of envy or regret over their minds, and I cannot imagine a man making discoveries in science under these feelings. As to Genius and its power, there may be cases, I suppose there are. I have looked long and often for a genius for our Laboratory, but have never found one. But I have seen many who would, I think, if they had submitted themselves to a sound self applied discipline of mind, have become successful experimental philosophers.[104]

There is a difference, then, between seeking praise and status and being privately satisfied that one has performed an important role in society in the advancement of the knowledge of God's creation and the applications of science for man's benefit consequent upon that knowledge.

When others trespassed on matters which were properly within the province of the philosopher, Faraday was ready to act against them. The most notable case of this was during the table-turning controversy, when he showed that the movement of

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[104]Note found after Faraday's death, quoted in Thompson, *op.cit.* (note 1), p.243.

tables, which spiritualists claimed was caused by mental forces, was really caused by physical movements of the table-turners' hands.[105] Although Faraday was publicly generous in his renunciation of the table-turners, his private views were much different:

> What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a ridiculous world ours is, as far as concerns the mind of man. How full of inconsistencies, contradictions, and absurdities it is. I declare that, taking the average of many minds that have recently come before me (and apart from that spirit which God has placed in each), and accepting that average as a standard, I should far prefer the obedience, affections, and instinct of a dog before it. Do not whisper this, however, to anyone. There is One above who worketh in all things, and who governs even in the midst of that misrule to which the tendencies and powers of men are so easily perverted.[106]

Scientific men, then, constituted a special class of society, seeking, in Faraday's view, to advance the knowledge of God's creation for the benefit of the nation and the rest of mankind. As such the State ought to honour this elite, though the men of science themselves should not *seek* the honours as this would make them no better than politicians and capitalist entrepreneurs. It was sufficient to the philosopher to know that his work was appreciated. For a government to bestow honours upon him was of two-fold importance to Faraday; firstly it showed that *his* work had been appreciated (and here he valued the awards he received from foreign governments more highly than

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[105] "Michael Faraday's Researches in Spiritualism", *Scientific Monthly,* 1956 (83), pp.145-50.

[106] Faraday to C.F.Schoenbein, 25 July 1853, cited in Bence Jones, *op.cit.* (note 1), Volume 2, pp.307-8.

his own - refusing a knighthood), but more importantly it showed that *science* was seen as a valuable pursuit. To honour science thus reflected well on the men of science as well as on the representatives of the State responsible for the award. Faraday's opinion of honours is encapsulated in a letter to Lord Wrottesley, which is worth quoting here at some length:

> I feel unfit to give a deliberate opinion on the course it might be advisable for the Government to pursue if it were anxious to improve the position of science and its cultivators in our country. My course of life, and the circumstances which make it a happy one for me, are not those of persons who conform to the usages and habits of society. Through the kindness of all, from my sovereign downwards, I have that which supplies all my need; and in respect of honours, I have, as a scientific man, received from foreign countries and Sovereigns, those which, belonging to very limited and select classes, surpass in my opinion anything that it is in the power of my own to bestow.
> I cannot say that I have not valued such distinctions; on the contrary, I esteem them very highly, but I do not think I have ever worked for or sought after them. Even were such to be now created here, the time is passed when these would possess any attraction for me; and you will see therefore how unfit I am, upon the strength of any personal motive or feeling, to judge of what might be influential upon the minds of others.[107]

Thus Faraday expressed his view of the honours which had been bestowed upon him as a reward for his scientific achievements. He continued:

> Without thinking of the effect it might have upon distinguished men of science, or upon the minds of those who, stimulated to exertion, might become distinguished, I do think that a Government should <u>for its own sake</u> honour the men who do honour and service to the country. I refer now to honours only, not to beneficial rewards; of such honours I think there are none. Knighthoods and baronetcies

[107] Faraday to Lord Wrottesley, 10 March 1854, *Correspondence*, pp.724-5.

> are sometimes conferred with such intentions, but I think them utterly unfit for that purpose. Instead of conferring distinction, they confound the man who is one of twenty, or perhaps fifty, with hundreds of others. They depress, rather than exalt him, for they tend to lower the especial distinction of mind to the commonplaces of society. An intelligent country ought to recognise the scientific men among its people as a class. If honours are conferred upon eminence of any class, as that of the law or the army, they should be in this also. The aristocracy of the class should have other distinctions than those of lowly or high-born, rich and poor, yet they should be such as to be worthy of those whom the Sovereign and the country should delight to honour, and, being rendered very desirable and even enviable in the eyes of the aristocracy by birth, should be unattainable except to that of science. Thus much I think the Government and the country ought to do, for their own sake and the good of science, more than for the sake of the men who might be thought worthy of such distinction. The latter have attained to their fit place, whether the community at large recognise it or not.[108]

We can therefore see how Faraday believed the scientific elite to be a truly distinct component of society, and as such to be worthy of special recognition from the Government, something the group had not hitherto received. He concluded the letter with recommendations to the Government as to how they should proceed in giving to men of science the status they sought:

> But besides that, and as a matter of reward and encouragement to those who have not yet risen to great distinction, I think the Government should, in the very many cases which come before it having a relation to scientific knowledge, employ men who pursue science, provided they are also men of business. This is perhaps now done to some extent, but to nothing like the degree which is practicable with advantage to all parties. The right means cannot have occurred to a Government which has not yet learned to approach and distinguish the class as a whole.[109]

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[108] *Ibid.*
[109] *Ibid.*

Faraday thus emerges as a believer in the importance of men of science to society just as much as did Wheatstone, Babbage, and Airy, in the earlier chapters in this thesis. His self-characterisation as a member of an elite group was of fundamental importance to his ideology, even though at first glance such affirmation of status might have seemed to have been in conflict with the tenets of his religion. Also, as I have shown earlier in the chapter, his attitude to instrument makers and their work was generous, but their work could not recognised as of the same standing as that of the philosopher. In as much as Faraday and his fellow-philosophers strove to press their claims to recognition, the instrument maker was bound to lose ground in terms of his status in society, and to be relegated to the level of a manual worker or tradesman.

Having concentrated in the past four case studies on individual philosophers and the way in which they articulated their claims to knowledge and status, and having considered the loss of status for the artisans as a consequence of this, I would like in the next chapter to study the British Association for the Advancement of Science. I will argue that this Association acted as a forum for the career ambitions of an elite which had emerged as far as the instrument makers were concerned, and as far as its own members were concerned (I have shown this with Wheatstone, Babbage, Airy, and Faraday, and their treatment of groups participating in the scientific enterprise, but outside the elite), but which believed it still required recognition by society.

CHAPTER SEVEN

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The British Association for the Advancement of Science differs from the subjects already discussed in this thesis in two respects. Firstly, unlike the Royal Society and the men of science Wheatstone, Babbage, Airy, and Faraday, it was not based in London, but met annually in towns and cities throughout England, Ireland, Scotland and Wales. The Council of the B.A., which conducted its business *between* meetings, did meet in London, however, and to this extent it can be claimed that the B.A. was a metropolitan, rather than a provincial, body. Secondly, and more importantly for the purposes of this discussion, the B.A., like the Royal Society, was an *institution,* with members from many different backgrounds, who shared the aim of wishing to "advance science". These actors may therefore be expected to possess a diversity of outlooks, and in particular may be expected to differ in their attitudes to instrument makers.

This chapter attempts to analyse the role that instrument makers played in the proceedings of the B.A., by considering the contributions they made to annual meetings and to research between meetings, and also tries to elucidate, by a study of the pronouncements of the Association's leading philosophers, the way in which the B.A. was used to fulfil personal ambitions.[1] The

[1] The most authoritative modern treatment of the Association's history is J.B.Morrell and A.Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science,* (Oxford, 1981), which deals with the years 1831-44. An important component of these authors' analysis is the notion that the B.A. was seen as a useful resource for the advancement of one's career.

perception of the instrument makers' status within the scientific community, arising from the authority of the philosophers, will also be an important concern.

1. The Origins of the British Association.

My aim in this chapter is to study the B.A. as an institutional encapsulation of the motives and ambitions of the emergent scientific elite which have been discussed throughout this thesis. Whereas the previous four chapters have dealt with individual members of the scientific community, this chapter extends the themes developed in the earlier case studies in order to provide a full picture of the scientific elite and the place it saw itself as occupying in society. Also, in studying the role of the instrument maker within this institution, the aim is to provide a reflection of his role in the scientific community as a whole. In order more fully to understand the nature of the institution under discussion, however, I would like to consider its origins, and the state of historical scholarship concerned with it which I would like to question.

Previous historical scholarship regarding the B.A. has dealt mainly with events leading to its formation at York in 1831. Although much of the earlier literature has long been successfully challenged, it is still useful to consider these accounts, as they provide an illustration of the difficulties inherent in approaches to the history of an institution based on the writings of selected individuals. They also show that it is possible to regard the efforts of a number of actors as being motive forces behind its formation, just as different men of science were important in its subsequent activity. Early accounts stressed the position of David Brewster as the Association's "founder". Like Babbage, Brewster, a staunch declinist, attacked the Royal Society for failing to represent science, and those who pursued it, effectively to the Government:

> ...the sciences and the arts of England are in a wretched state of depression, and their decline is mainly owing to the ignorance and supineness of the Government; to the injudicious organisation of our scientific boards and institutions; to the indirect persecution of our scientific and literary men by their exclusion from all the honours of the State; and to the unjust and oppressive tribute which the patent law exacts from inventors.[2]

Brewster's model for bolstering the relationship between science and the public interest was, again like Babbage's, French. Foote, in 1951,[3] attributed the formation of the B.A. to this one cause of agitation over the decline of science, with Brewster as the main instigator, the man who made the suggestion that an Association be formed with its aims to overcome the problems which he listed in the above quotation. The basic cause of the inaccuracies in Foote's account was that he allowed himself to be convinced by Brewster's rhetoric. As Williams pointed out,[4] there were many discrepancies in Foote's analysis, which showed that the Association's genesis could not have been

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[2]D.Brewster, "Review of Reflexions on the Decline of Science in England, and on Some of its Causes, by Charles Babbage", *Quarterly Review,* 1830 (43), pp.305-42, on p.341.

[3]G.A.Foote, "The Place of Science in the British Reform Movement, 1830-50", *Isis,* 1951 (43), pp.192-208.

[4]L.P.Williams, "The Royal Society and the founding of the British Association for the Advancement of Science", *Notes and Records of the Royal Society of London,* 1961 (16), pp.221-33.

the simple result of pressure by the declinists. For example Faraday was strongly opposed to the declinist arguments of Babbage and Brewster, and yet he was a charter member of the B.A..[5] Williams also stressed, rather less helpfully, that the Royal Society had been attacked before without the attack giving rise to a new institution.[6] His own suggestion was that it was the loss by John Herschel to the Duke of Sussex of the Royal Society Presidential Election in 1830 which frustrated the "professional" would-be reformers of the Royal Society and stimulated the formation of a new society, the British Association, in which were embodied the reforms which they had desired for the Royal Society. This account has its own problems, not least of which is that of those "professional" reformers to whom Williams refers, two of the most notable, Herschel and Babbage, were conspicuous by their absence from the York meeting. Still, the crucial aspect of Williams' analysis is that the B.A. derived from a desire held by men of science to become "professional". The word "professional" is the most misleading aspect of this and other articles, and the notion of a "professional scientist" is a modern construct which carries with it the connotation of being paid to prosecute one's research, often in government establishments. Such a situation clearly was not the aim of actors in the 1830s. This chapter attempts to argue that while the philosophers who attended B.A. meetings had no desire to be "professional", they did share a wish to proclaim themselves as an elite group in society, and

[5] *Ibid.*, p.221.
[6] *Ibid.*

the Association came to be used by them to achieve this aim.[7] To this extent only has the "professionalisation" model a certain validity, because other "professions" existing in the nineteenth century could be said to possess similar criteria for membership as did the scientific community. For example the legal and medical professions, it could be argued, restricted their membership according to training and expertise in the same way as did the scientific elite. This discussion, however, seeks to emphasise the sense of identity and unity of purpose of the philosophers without equating these with a desire to professionalise their activity.

Historical accounts subsequent to those discussed above have stressed the role of provincial initiative in the formation of the B.A..[8] While Brewster has been universally acknowledged as the philosopher who originally suggested the idea of an Association of men of science meeting in Britain, it has become clear that the main efforts to bring such a meeting into being were due to William Vernon Harcourt, of the Yorkshire Philosophical Society. Brewster relinquished his own direct control over the course of events when he suggested that the Yorkshire Philosophical Society should prepare a code of laws for

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[7] For analyses of historiography of the British Association, see S.F.Cannon, *Science in Culture,* (New York, 1978), pp.167-200, and J.D.Burchfield, "The BA and its historians", *Historical Studies in the Physical Sciences,* 1982 (13), pp.165-74. See Morrell and Thackray, *op.cit.* (note 1) for an analysis in which the notion of the use of the B.A. as a resource for ambitious philosophers features prominently.

[8] For example A.D.Orange, "The British Association for the Advancement of Science: The provincial background", *Science Studies,* 1971 (1), pp.315-29.

the consideration of the foundation meeting, a task which fell to Harcourt.[9] As James David Forbes recalled some years later at the B.A. meeting in Edinburgh:

> ...an imitation of the foreign meetings having been suggested by some individuals engaged in scientific pursuits, among whom Sir David Brewster was conspicuous... the original idea, and the much more signal merit of bringing that idea to bear, of establishing a permanent society, of which these annual reunions should be simply the meetings... were due to one individual and to one alone... Mr. William Vernon Harcourt.[10]

Harcourt was acutely conscious, in his inaugural address,[11] of the need to avoid aligning himself with any of the declinist doctrines of those such as Brewster. Such doctrines were dangerous politically, and avowal of these opinions would have rendered it difficult for the new organisation to attract the full spectrum of philosophers which it did in the future. Admittedly many stayed away from the first meeting, but for Harcourt and York it was a success. The members of the Yorkshire Philosophical Society, in particular Harcourt, saw in the B.A. a way in which they could achieve the status possessed by the metropolitan philosophers. The elite philosophers of the metropolis would in time come to view the Association in the same way - as a vehicle for career interests, and after the initial meeting the main positions of power within the new organisation

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[9] D.Brewster, Notice in *Edinburgh Journal of Science,* 1831, pp.180-2.

[10] J.D.Forbes, *Address to the British Association at Edinburgh, September 8th 1834,* pp.5-6.

[11] Address by W.V.Harcourt, *B.A.A.S. Report, York, 1831,* pp.17-41, esp.p.22.

would by and large be lost from the provinces. The stated aims of the B.A., however, were undoubtedly attributable to Harcourt:

> ...a British Association for the Advancement of Science, having for its objects, to give a stronger impulse and more systematic direction to the objects of science, and a removal of those disadvantages which impede its progress, and to promote the intercourse of the cultivators of science with one another, and with foreign philosophers.[12]

The idea of the use of the B.A. to further personal career interests will be a prominent theme in this chapter. It will be argued that various individuals were able to use the Association for their own ends. To this extent my analysis agrees with that of Morrell and Thackray. However, I disagree with their emphasis on the liberal Anglican standpoint of most of the protagonists in their discussion. It is not helpful to characterise the scientific elite in the British Association as predominantly liberal Anglican, and it is even less helpful to extend this to the British scientific community as a whole. Babbage, an active member of the B.A., could not be considered as being liberal in his politics. Likewise, Faraday was not an Anglican, and not one of Morrell and Thackray's "Gents", but certainly was a prominent member of the scientific community. The aim in this chapter, and in the thesis as a whole, is to show that membership of a group searching for scientific truths transcended any political or religious affiliations these actors may have had, and it was this that provided them with their class identity, thus setting them apart from those groups, such as instrument makers, that were not seen as advancing scientific

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[12] *Ibid.*, p.22.

knowledge.

During the course of this chapter it will be shown that instrument makers were able to gain publicity and reputation through the B.A., and provincial philosophers and engineers were able to learn from, be directed by, and take part alongside philosophers such as Whewell, Babbage, Herschel, and Airy. However the interests of all were very much subordinate to the needs of that elite group of philosophers of which those named were among the most famous.

2. The Structure of the British Association.

The opening paragraph of the Association's constitution was slightly changed from that expressed by Harcourt in his speech to the York meeting. In their final form, the objects of the Association were stated as:

> To give a stronger impulse and more systematic direction to scientific inquiry; to promote the intercourse of those who cultivate Science in different parts of the British Empire with one another and with foreign philosophers; to obtain more general attention for the objects of Science; and the removal of any disadvantages of a public kind which impede its progress.[13]

Essentially, however, this was no different to the original version. The B.A. was to consist of a Council which met in London between the times of the annual meetings to transact the business of the Association, and a General Committee which met during the week of each meeting, and in whom governing power was effectively vested. Although membership of the Association itself was in principle open to any man interested in the advancement of

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[13] *B.A.A.S. Report, Cambridge, 1833,* p.497.

science, it was felt by those in authority that the General Committee ought to be a more elite body. Thus it was proposed that membership of the General Committee should be limited to those who had contributed at least one paper on a scientific subject to a volume of transactions of a recognised provincial literary and philosophical society, or a metropolitan society. This was a sufficiently objective criterion - no regard was given to the quality, importance, subject, or length of the paper, and yet it was effective in bestowing the necessary status upon the General Committee compared with the ordinary membership.[14]

The scientific work which the Association carried out at its annual meetings can be divided into two classes. Firstly, there were reports of researches undertaken at the request of the B.A., and reports of the state of knowledge in various subject areas. The latter arose from a suggestion made by Whewell that a useful purpose for the Association to perform would be to commission reports to be written by leading philosophers in each subject area, so that it would be easier for others to learn what had and had not been achieved in these areas.[15] Secondly, there were miscellaneous communications to the "sections". These were accepted as being of subordinate importance, and the presentation of papers to the subject sections in a form in which they could have been published in learned journals was discouraged,[16] as the B.A. had as part of its constitution the assurance that it did

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[14]Morrell and Thackray, *op.cit.* (note 1), pp.77, 90-1.
[15]I.Todhunter, *William Whewell. An Account of his Writings with Selections from his Literary and Scientific Correspondence,* (2 volumes, London, 1876), Volume 2, pp.126-30.

[16]*B.A.A.S. Report, Dublin, 1835,* part 2, p.1.

not wish to encroach upon areas which were properly within the province of other societies.[17] Thus communications to the sections were only published in the form of abstracts in the annual reports of the Association.

It seems that in the first few years of the B.A., sections were only created to accommodate those communications which were made, i.e. there was no necessary fixed pattern of sections to which papers could be contributed; the papers contributed *determined* the sections. Thus in 1831 communications were made to "sub-committees", under the General Committee, of which there was one for Mathematics and Physics, one for "Mechanical Arts", with J.H.Abraham, John Robison (*son* of the former Edinburgh Natural Philosophy Professor), and Benjamin Rotch, M.P., all serving,[18] and four others. In 1832, however, there was no mechanical art "committee", suggesting that no communications were made on that subject.[19] By Cambridge in 1833 there were such communications, and they were made to a "section" entitled "Philosophical Instruments and Mechanical Arts", which was listed second, after the mathematics and physics section, and which included contributions by philosophers such as W.H.Miller, future Professor of Mineralogy at Cambridge, and James Cumming, Professor of Chemistry at Cambridge, but also by instrument

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[17] Objects of the Association, *B.A.A.S. Report, York, 1831*, p.ix.

[18] See *B.A.A.S. Report, York, 1831*, pp.56-90, for an account of miscellaneous communications made to the meeting.

[19] See *B.A.A.S. Report, Oxford, 1832*, pp.545-602, for an account of miscellaneous communications made to the meeting.

makers such as John Newman and Edward Dent.[20] The following year, in Edinburgh, the section became "Mathematical Instruments and Mechanical Arts", and included papers by J.D.Forbes, local Professor of Natural Philosophy, and John Dunn, local optician.[21] A consistent pattern of sections appeared by the Bristol meeting in 1836, with Mathematics and Physics as Section A, and "Mechanical Science" as Section G, though in some future years it would be known as "Mechanics".[22] It was apparent, then, that instrument makers and practical projects had a place within the activity of the B.A.. In the following sections I would like to consider the way in which the new institution could be seen to provide the opportunity of a forum within the scientific community for the instrument maker, who as we have seen was in general excluded from that community over the period discussed in this thesis. I will then continue, in the later part of the chapter, to show the role taken by the philosopher in the B.A., and to demonstrate that, though the instrument maker's participation in the affairs of the B.A. was welcomed by him, it was very much a role subordinate to that of the philosopher.

3. The Role of Instrument Makers in the British Association.

From the earliest years of the B.A., then, instrument makers were providing contributions to the sections, on innovations which they had made in their instrument designs. These makers

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[20] See *B.A.A.S. Report, Cambridge, 1833,* pp.v-vii, for the list of contributions to the sections.

[21] See *B.A.A.S. Report, Edinburgh, 1834,* pp.v-ix, for the list of contributions to the sections.

[22] For example, in *B.A.A.S. Report, Plymouth, 1841.*

wished to use the Association for career reasons - a contribution aired at a meeting would make them known to many potential clients among men of science, most importantly, and also to the many local enthusiasts who were present at each of the meetings. Thus it was common for a maker to join for one year in his home town, where he could attend the meeting, perhaps make a contribution to either the mathematics and physics section, or the mechanical science section, and thereby enhance his reputation, certainly locally, and perhaps even nationally. Some of the London makers joined for longer than the single year, regarding the exposure to potential clients which the B.A. afforded as a worthwhile reason for investment.

George Dollond was one such maker. He constructed an instrument for distinguishing precious stones and minerals, which had been designed by David Brewster, and this instrument was displayed to the first meeting of the Association as an example not only of Brewster's expertise in the design, but also as an illustration of Dollond's competence as an instrument maker.[23] In 1833, at Cambridge, Dollond was mentioned favourably in connection with a "beautiful" dipping needle which he had made,[24] and also appeared on the Committee for Mathematics and General Physics.[25] Any potential customer who saw that Dollond was regarded by the leading philosophers of the B.A. as having

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[23] D.Brewster, "An Instrument for distinguishing Precious Stones and Minerals", *B.A.A.S. Report, York, 1831,* pp.72-3.

[24] W.Scoresby, "On a peculiar source of error in Experiments with the Dipping Needle", *B.A.A.S. Report, Cambridge, 1833,* pp.412-13.

[25] *B.A.A.S. Report, Cambridge, 1833,* p.xxxix.

sufficient expertise to warrant placing him on an influential committee could not fail to be impressed. Dollond appears among the life members of the Association listed in the report for 1835; the only other prominent maker to have paid his life subscription by that date was William Simms; both had their full addresses given in the report, presumably lest any readers wished to place orders.[26] Dollond took part in the activity of the meetings to some extent certainly until as late as 1846, when he communicated an account of his atmospheric recorder to the mathematics and physics section.[27] This was a simple extension of ordinary self-registering apparatus which facilitated the recording, on one instrument, of the readings of the barometer, thermometer, electrometer, hygrometer, pluviometer, evaporator, force board, and anemometer.

John Newman, though not of the same pedigree as Dollond, was able to make his name known, and to enhance his reputation, by taking part in the early meetings of the B.A., though he did not make the investment of becoming a life member. He was unable to describe himself, at Cambridge in 1833, as a Fellow of the Royal Society, as Dollond could. Instead he merely styled himself "Mathematical Instrument Maker",[28] though presumably he could have referred to his status as official maker to the Royal Institution. The communication which he made there to the section

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[26] List of members, *B.A.A.S. Report, Dublin, 1835,* part 3, pp.1-9.

[27] G.Dollond, "An Account of an Atmospheric Recorder", *B.A.A.S. Report, Southampton, 1846,* part 2, p.17.

[28] J.Newman, "On a new method of constructing a Portable Barometer", *B.A.A.S. Report, Cambridge, 1833,* pp.417-18.

on "Philosophical Instruments and Mechanical Arts" was concerned with a new method of constructing a portable barometer.[29] The object which he had in mind in the design of this new instrument was a firmly practical one - to make the barometer portable without the use of a leather bag, which had apparently always been a defective part of such instruments. Clearly exposure to the B.A. members at Cambridge would have been useful to Newman commercially, and although his activity in the Association itself may not have been the cause (as he was renowned for his work for the Royal Institution since the early years of the century), his reputation some years later was such that he was one of the most widely employed makers of instruments, particularly meteorological ones, for B.A. researches and for Kew Observatory.

It was not only makers of philosophical instruments who were able to use the B.A. as an aid to their career ambitions. Chronometer makers also played an active role. William James Frodsham, although he had just become a Fellow of the Royal Society, aged 61, attended the Birmingham meeting in 1839.[30] Frodsham was the last chronometer maker to be elected to a Fellowship of the Royal Society, and the only instrument makers of any kind subsequently elected were William Simms, and Thomas and Howard Grubb. Frodsham's communication to the Mathematics and

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[29] *Ibid.*

[30] E.G.R.Taylor, *The Mathematical Practitioners of Hanoverian England 1714-1840*, (Cambridge, 1966), p.338; V.Mercer, *The Frodshams*, (London, 1981), pp.37ff.

Physics section was on a comparative pendulum.[31] In order to compensate the pendulum for different temperatures, Frodsham slipped a zinc tube over the steel rod of a common pendulum, and by making the tube a little too short and "applying small rings cut from the same tube as connecting pieces until the proper length is found", the problem, from an accuracy point of view, of the irregularity of expansion of different specimens of the same metal, was obviated. Improvements in the mode of suspending the pendulum were also overcome. Although in the eyes of the leading philosophers of Section A, this method would undoubtedly have been regarded as somewhat "rule of thumb", and indeed typical of the practice of an artisan, it would nonetheless have been seen as ingenious, and Frodsham's status would by no means have suffered as a result of the communication. He does not seem to have made any further communications, though in 1842 Bessel contributed a paper on the Astronomical clock,[32] in which he gave Frodsham, a celebrated "artist", credit for the development of what he called the "isochronal piece", which is related to the maker's own above development.

In Bessel's 1842 paper, favourable reference was also made to Edward Dent (1770-1853)[33] who, perhaps more than any other instrument maker, was able to make use of the B.A. for career purposes. He had been employed by Vulliamy and Son, and Barraud and Son, chronometer makers, before forming a famous

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[31] W.J.Frodsham, "Notice of a Comparative Pendulum", *B.A.A.S. Report, Birmingham, 1839,* part 2, p.24.

[32] F.W.Bessel, "On the Astronomical Clock", *B.A.A.S. Report, Manchester, 1842,* part 2, pp.1-2; Mercer, *op.cit.* (note 30).
[33] *Ibid.,* p.2.

partnership with J.R.Arnold, which lasted from 1829 until 1840.[34] Dent's first dealings with the B.A. were in 1833, when the Committee on Arts at the Cambridge meeting recommended that he be requested to communicate to the Edinburgh meeting of the Association a statement of the performance of his chronometer with a glass balance spring.[35] This request was a response to a paper of Dent's, contributed to the section on "Philosophical Instruments and Mechanical Arts" of 1833, on the novel application of a glass balance spring to a chronometer.[36] In this communication, Dent had described the various difficulties caused by the use of metallic balance springs - of gold, or soft or hardened steel, and had explained the advantages which might be expected to accrue if a substance of greater and more regular elasticity could be employed. Glass appeared to be a suitable choice for such a substance, "and when formed into a cylindrical spring, it promised, from the trials that had been made, to be both accurate and durable".[37] A working instrument with the glass spring was exhibited to the assembly at Cambridge. The following year, at Edinburgh, Dent was able, as requested, to give an account of the rate of his instrument, which had been kept at the Royal Observatory, Greenwich, since the Cambridge meeting. According to the abstract, he:

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[34] E.G.R.Taylor, *op.cit.* (note 30), pp.388-9; V.Mercer, *The Life and Letters of Edward John Dent, Chronometer Maker, and Some Account of his Successors,* (London, 1977).

[35] Recommendations of the Committee on Arts, *B.A.A.S. Report, Cambridge, 1833,* p.483.

[36] E.J.Dent, "On the application of a glass balance spring to chronometers", *B.A.A.S. Report, Cambridge, 1833,* p.421.

[37] *Ibid.*

> showed a chronometer in motion, with a pure palladium balance spring; and produced a table of the variations of gold, steel, palladium and glass, from 32^0 to 100^0 Fahr., and another table of the quantities respectively due to direct expansion, and to loss of elasticity, in steel and palladium.[38]

The attention which was paid to him by the B.A. members would have convinced him that here was an important vehicle for his career ambitions, but at least initially he chose not to become a life member, preferring instead to pay his subscriptions annually,[39] and although he made no communications in the years subsequent to the Edinburgh meeting, he did exhibit a portable mercurial pendulum among the mechanical inventions at Newcastle in 1838.[40] In 1839, however, he made two contributions to the Mathematics and Physics section. The first, concerning the rate of the transit clock at the Radcliffe Observatory in Oxford,[41] was of more limited interest than the other communication, which dealt with the important question, in terms of the use of science to facilitate transatlantic relations, of the determination, by means of chronometers, of the difference in longitude between

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[38] E.J.Dent, "On a Chronometer with a Glass Balance Spring", *B.A.A.S. Report, Edinburgh, 1834,* p.595.

[39] List of members, *B.A.A.S. Report, Dublin, 1835,* part 3, p.18.

[40] "Catalogue of the Philosophical Instruments, Models of Inventions, Products of National Industry, contained in the First Exhibition of the British Association for the Advancement of Science" *B.A.A.S. Report, Newcastle, 1838,* part 3, pp.1-26.

[41] E.J.Dent, "Note accompanying a Table of the Rate of the Transit Clock in the Radclyffe Observatory, Oxford", *B.A.A.S. Report, Birmingham, 1839,* part 2, pp.28-9.

Greenwich and New York.[42] The results with each of the Dent chronometers employed agreed to within half a mile of longitude. Dent pointed out that his chronometers were the first to be employed in this determination of longitudes between Britain and America, and also demonstrated, by the close agreement of the results that, at least for his chronometers, there was no difficulty or loss of accuracy through travelling in steam vessels. The importance of this investigation, as far as Dent was concerned, was that it ought to have convinced the members of the B.A. that they should order any chronometers which they required, whether for personal or institutional use, from him, as only his had so far proved capable of finding the longitude between Britain and America.

A similar project involved the determination of the longitudes of the principal observatories in the British Isles. A committee had been appointed at the Edinburgh meeting in 1834 to achieve this end, and Thomas Romney Robinson reported on behalf of the committee in 1839 that:

> The chronometric part of the process has however been most effectually performed by one of our members, Mr.Dent, who, in the first instance, sent twelve of his chronometers from Greenwich to Edinburgh and Makerstown...[43]

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[42] E.J.Dent, "Account of a recent successful experiment to determine, by means of chronometers, the difference of longitude between Greenwich and New York", *B.A.A.S. Report, Birmingham, 1839,* part 2, pp.27-8.

[43] T.R.Robinson, "Notice of Determination of the Arc of Longitude between the Observatories of Armagh and Dublin", *B.A.A.S. Report, Birmingham, 1839,* part 1, pp.19-22, on p.19.

The results with these chronometers had been reported at the previous meeting. For the more recent determination of the longitudes of the Irish observatories, however,

> Mr.Dent not merely placed these chronometers at our assistance, with three additional, but bestowed what was even more precious, his personal attendance, and assisted us in the comparisons; an advantage which could not have been purchased, but which I notice as an instance of the aids which these meetings afford to Science.[44]

Dent further enhanced his reputation by contributions made to the Mathematics and Physics section in following years. For example in 1841 he described his method of giving the steel balance springs of chronometers a galvanic coating of gold, as a protection from the ill-effects, such as spray from salt-water, which could occur on board ship.[45] This innovation was praised at the meeting of the following year by Bessel.[46] In this year, 1842, at Manchester, Dent made no fewer than four contributions to the section, one of which was concerned with a demonstration that the rates of his chronometers were not affected by the gold covering on the steel spring,[47] and another mentioning the invention by Airy, with whom he worked closely, of a new escapement.[48] By 1844, Dent, described in the report as "Fellow of the Royal Astronomical Society, 82 Strand, London", had become

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[44] *Ibid.*

[45] E.J.Dent, "On the application of a Coating of Gold by the Electro-metallurgic process to the Steel Balance-springs of Chronometers", *B.A.A.S. Report, Plymouth, 1841,* part 2, p.41.

[46] Bessel, *op.cit.* (note 32).

[47] E.J.Dent, "On the rate of protected Chronometer Springs", *B.A.A.S. Report, Manchester, 1842,* part 2, p.9.

[48] E.J.Dent, "On the rate of a Patent Compensating Pendulum", *B.A.A.S. Report, Manchester, 1842,* part 2, p.10.

a life member of the B.A.,[49] and he contributed two more papers that year, one on clocks[50] and one rather more of a diversion from the usual subject of his communications - on a steering and azimuth compass,[51] used for taking bearings of stars etc. He carried on with the subject of compasses in 1845 at Cambridge, again displaying, to the members of the B.A., his superiority to the other makers. In the abstract of this 1845 paper it was stated that Dent's compass was found "to be extremely sensitive... while the other compasses with which it was compared were always in error".[52] Dent's last significant dealing in B.A. affairs was the following year, when he exhibited a portable azimuth compass to the Southampton meeting of the Association.[53]

Although, unlike Frodsham, Dent never became a Fellow of the Royal Society, the active role which he took in the affairs of the B.A. was enough to make him the most celebrated chronometer maker of his day. He communicated researches to the Mathematics and Physics section from Edinburgh in the north to Plymouth and Southampton in the south, and became known in all areas by those attending meetings as a reliable and ingenious maker. A small investment of time and money in the Association thus paid Dent

[49] List of members, *B.A.A.S. Report, York, 1844,* part 3, p.7.

[50] E.J.Dent, "On the Shape of the Teeth of the Wheels of the Clock in the New Royal Exchange", *B.A.A.S. Report, York, 1844,* part 2, p.8.

[51] E.J.Dent, "On a new Steering and Azimuth Compass", *B.A.A.S. Report, York, 1844,* part 2, p.12.

[52] E.J.Dent, "On a Method of Suspending a Ship's Compass", *B.A.A.S. Report, Cambridge, 1845,* part 2, p.16.

[53] E.J.Dent, "On a New Portable Azimuth Compass", *B.A.A.S. Report, Southampton, 1846,* part 2, p.25.

and his business handsome dividends. The Association, meanwhile, derived from Dent, as we have seen, useful help and advice, notably in researches concerned with establishing longitude; projects which the B.A. could claim to have initiated, with results that were vital to the State and Empire.

Of other instrument makers who were prominent in the B.A., one of the more surprising may be Andrew Pritchard. Although he had written *A Practical Treatise on Optical Instruments* in the 1820s,[54] he specialised mainly in microscopes, for which the prestigious commissions that one could then obtain for making telescopes were not available (being a small instrument, there was no equivalent of the large observatory commission) . However, although the microscope maker could not hope to obtain an order from the B.A. to equip a research project, such as for example a thermometer maker might, by attending a meeting he could hope to impress *individuals* with his workmanship. Pritchard made the trip to Dublin in 1835, where he gave an account to the section of "Mechanical Sciences Applied to the Arts" of his microscopes and polariscopes.[55] He exhibited examples of the latter class of instruments, which he had constructed to illustrate the polarisation of light, and also his improved achromatic microscope. This microscope was "constructed on the principles recently published by Dr.Goring and Mr.Pritchard in their works

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[54] A.Pritchard, *A Practical Treatise on Optical Instruments*, (London, 1832).

[55] A.Pritchard, "Mr.A.Pritchard on Microscopes, Polariscopes etc.", *B.A.A.S. Report, Dublin, 1835*, part 2, p.112.

on the microscope".[56] From a commercial standpoint, the exhibition of these instruments must have been a success; Pritchard was unequivocal in his description:

> Mr.Pritchard stated expressly of this instrument, that it was the simplest that had yet been constructed that would accomplish all the work that might be required of a microscope, either for general examination, dissection, or minute investigation.[57]

Pritchard was unusual among makers who used the B.A., outside of the top few London makers, in that he made the effort to travel to, in this case, Dublin, in order to make his name known in circles of philosophers who attended the meeting. The typical path for an aspiring instrument maker to pursue was to wait until the Association visited his town or neighbourhood, and then join the B.A. for one year only, hoping that the exposure which he achieved at the meeting would make him known to a large number of locals present, whence he might derive the greater part of his business, but also perhaps hoping that if he could display sufficient skill and knowledge, that he would be noticed and acclaimed by the leading philosophers, metropolitan or otherwise, at the meeting. At Dublin in 1835 for example, besides the London maker Pritchard, the local maker of astronomical instruments, Thomas Grubb, was present,[58] although he did not make any contribution to the sections. The previous year, at

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[56] *Ibid.* C.R.Goring and Pritchard published several works with the aim of popularising microscopy, for example C.R.Goring and A.Pritchard, *Microscopic Illustrations,* (London, 1830); C.R.Goring and A.Pritchard, *Micrographia,* (London, 1837). For further details of Pritchard's career, see R.H.Nuttall, "Andrew Pritchard, Optician and Microscope Maker", *The Microscope,* 1977 (25), pp.65-81.

[57] Pritchard, *op.cit.* (note 55).

[58] Morrell and Thackray, *op.cit.* (note 1), p.259.

Edinburgh, John Dunn, a local optician, had communicated to the section on "Mathematical Instruments and Mechanical Arts" a paper on a "klinometer" and portable surveying instrument.[59] At the Manchester meeting of 1842 it is no surprise to find local maker John Dancer listed among the annual subscribers.[60] Dancer was later to achieve fame as one of the leading makers employed by James Prescott Joule.[61]

Artisans were given an opportunity to display their products to assembled philosophers and others at Newcastle in 1838, where there was an exhibition of models of mechanical contrivances, philosophical instruments, and products of manufacturing industry. Babbage had seen the desirability of such an exhibition as far back as 1831:

> It is extremely desirable that every member should be urged to bring with him such portable instruments as he may employ in experimenting... Might it not be possible to have an exhibition of manufactures at each meeting??[62]

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[59] J.Dunn, "On a new Klinometer and Portable Surveying Instrument", *B.A.A.S. Report, Edinburgh, 1834,* pp.594-5. Further information on Dunn and his brother Thomas is provided in T.N.Clarke, A.D.Morrison-Low, and A.D.C.Simpson, *Brass and Glass. Scientific Instrument Making Workshops in Scotland as illustrated by instruments from the Arthur Frank Collection at the Royal Museum of Scotland,* (Edinburgh, 1989), pp.89-95.

[60] List of members, *B.A.A.S. Report, Manchester, 1842.*

[61] R.H.Nuttall, "Microscopes for Manchester", *Chemistry in Britain,* 1980 (16), pp.132-5. See also D.S.L.Cardwell, *James Joule. A biography,* (Manchester, 1989), pp.63, 206.

[62] C.Babbage to W.V.Harcourt, 31 August 1831, Harcourt Papers, xiii, pp.239-42, cited in J.B.Morrell and A.Thackray (eds.), *Gentlemen of Science: Early Correspondence of the British Association for the Advancement of Science,* (London, 1984), pp.49-51.

Such an exhibition had been planned for the Liverpool meeting in 1837, but had failed to take place as nobody had been put in charge.[63] A committee was therefore appointed at Liverpool to superintend the projected exhibition at the following year's meeting. Brewster, Babbage, Wheatstone, Professor Willis, and Professor Baden Powell were on the committee, and Professor Johnston (one of the local secretaries for the Newcastle meeting) was appointed secretary. The composition of this committee (all philosophers and no artisans, despite the nature of the exhibition), was indicative of the power base within the B.A., and the way in which authority was in general denied to those outside the philosophical elite. As the committee membership was widely scattered, Johnston did most of the work, producing a draft circular to send to potential exhibitors by February 1838.[64] A final printed circular, approved by Brewster and Babbage, was sent to inventors, engineers, and manufacturers, in addition to the *Literary Gazette* and *Athenaeum* by late July.[65] Unfortunately, Johnston did not give himself enough time to organise the exhibits, so that the printed catalogue of the exhibition was only available towards the close of the meeting.[66]

The exhibition consisted of two parts: specimens connected with the arts and the development of national industry, and a section entitled "mechanical and philosophical" which included sub-sections containing machines, and philosophical instruments. Within the latter area the London instrument makers were

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[63] Morrell and Thackray, *op.cit.* (note 1), p.195.
[64] *Ibid.,* p.196.
[65] *Ibid.*
[66] *Ibid.*

conspicuous by their absence, with only prominent Association member Edward Dent contributing an exhibit.[67] Among those few philosophical instruments which were displayed were two by local maker John Brown, of Grey Street, Newcastle. He exhibited a new self-registering thermometer, and an "instrument for crushing stone in the bladder",[68] showing the diversity of his enterprise to a local and national audience of potential customers.

A small number of elite foreign makers were attracted to the B.A. in its early years. These men would have seen the meetings as an opportunity to take part in one of the main events in the British scientific calendar, more so than using them as a means of advertising their products, which would of course have found a greater market in their own countries (importing a scientific instrument was much more expensive than procuring it at home). Philadelphian Joseph Saxton's famous magneto-electric machine was exhibited to the Cambridge meeting of 1833, before it moved to the Gallery of Practical Science in Adelaide Street that August.[69] At Edinburgh Saxton contributed a paper on an instrument which could measure variations in temperature in metal rods, thus displaying his versatility.[70] George Chilton, a chemist and scientific instrument maker from New York, was

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[67] Catalogue of Philosophical Instruments etc., *op.cit.* (note 40).

[68] *Ibid.,* p.15.

[69] J.Saxton, "Mr.J.Saxton on his Magneto-electrical Machine", *Philosophical Magazine,* 1836, pp.360-5.

[70] J.Saxton, "On an Instrument for Measuring Minute Variations of Temperature in Metal Rods etc.", *B.A.A.S. Report, Edinburgh, 1834,* p.xlvii. This paper was mentioned only by title in the report; no abstract of its contents was given.

present at this same meeting in 1834.[71] The Newcastle meeting of 1838 attracted two of the most famous European makers of the period, Traugott Lebrecht Ertel, from Munich, and Henri Prudence Gambey, from Paris, though they do not seem to have taken an active part in the proceedings of the meeting.[72]

Overall, few instrument makers took any active part in the B.A.. Of the London makers we can perhaps only list Dent, Newman, Dollond, and Pritchard. Simms and Watkins were members but did not contribute to any business of the Association.[73] Simms' partner, Edward Troughton, was the only instrument maker who received a copy of the circular advertising the proposed meeting at York in 1831,[74] but he was by then an old man and took no part in the affairs of the B.A.. J.J.Lister, although not an instrument maker by trade, rather a distinguished amateur skilled in the design and construction of instruments, became a life member early on.[75] The only other notable makers were those locals who joined for the year in which the Association visited their town. However, those makers who did join the B.A. were able to use it in order to further their own careers, by making their names and activities known throughout the area of the Association's influence. To this extent, it can be said that the B.A. was a favourable institution for instrument makers. It is significant, however, that no instrument maker, with the

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[71] Morrell and Thackray, *op.cit.* (note 1), p.414.

[72] J.Phillips to W.V.Harcourt, 10 August 1838, Harcourt Papers, cited in Morrell and Thackray, *op.cit.* (note 62), pp.272-4.

[73] Watkins appears in the list of members in, for example, *B.A.A.S. Report, Oxford, 1832,* p.609.

[74] Morrell and Thackray, *op.cit.* (note 1), p.545.

[75] List of members, *B.A.A.S. Report, Oxford, 1832,* p.617.

exception of George Dollond in 1833, when the Association was still in its infancy, ever served in a position of authority in the B.A.. These positions were reserved for philosophers. In the remainder of this chapter I would like to demonstrate the way in which the scientific elite were able to use the B.A. to further *their* ambitions and status, and the manner in which they subordinated the activities of other groups to theirs. I would like to start by considering their work in the area of instruments, where the makers of the instruments tended to be excluded from authority on their design and testing, which were seen as tasks for the man of science.

4. Philosophers and Instrumentation.

It would be wrong to imagine that the majority of communications made to the B.A. on the subject of innovations in instrument design were made by the makers of the instruments. In fact, in keeping with the general situation at the time regarding instrument design, the innovations and the written descriptions usually came from the men of science. Thus at the very first meeting in 1831, papers were contributed by philosophers such as Daubeny, who exhibited an instrument of his design which was intended to illustrate the effects of capillary action,[76] and Robison, who described a linseed oil barometer he had developed.[77] More interesting is the case of Richard Potter, a young Manchester philosopher who attended the early meetings and contributed several papers on his favourite subject area of

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[76] *B.A.A.S. Report, York, 1831,* p.85.
[77] *B.A.A.S. Report, York, 1831,* p.86.

optical instrumentation. In 1831 he read a description of his new design of Newton's reflecting microscope, and exhibited the instrument,[78] and the following year he described an instrument concerned with photometry.[79] The attention which Potter gained at these meetings convinced him that he was capable of pursuing an academic career, and he subsequently became a Fellow of Queens' College, Cambridge, from 1839-1841, and Professor of Natural Philosophy at University College, London, from 1841-3, and 1844-65.[80]

One of the main areas of instrumentation with which the B.A. concerned itself was anemometry. Follett Osler and Snow Harris were at the forefront of these developments and attempted to design an instrument that would record the direction and speed of the wind and its changes with time. Whewell was also interested in the field and presented an unfinished self-registering anemometer to Section A at Dublin in 1835,[81] an updated version the following year at Bristol,[82] and by 1837 had come up with an

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[78] R.Potter, "Description of a new construction of Sir Isaac Newton's reflecting microscope", *B.A.A.S. Report, York, 1831,* pp.71-2.

[79] R.Potter, "On an Instrument for Photometry by comparison, and on some applications of it to optical phenomena", *B.A.A.S. Report, Oxford, 1832,* pp.554-5.

[80] Morrell and Thackray, *op.cit.* (note 1), p.407. For Potter's professorship in London, see G.J.N.Gooday, *Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain,* (Ph.D. Thesis, University of Kent, 1989), pp.4/13-4/16.

[81] W.Whewell, "On a New Anemometer", *B.A.A.S. Report, Dublin, 1835,* part 2, p.29.

[82] W.Whewell, "Further account of the Anemometer", *B.A.A.S. Report, Bristol, 1836,* part 2, pp.39-40.

improved model which could measure the velocity of the wind and the time for which that velocity was maintained.[83] Harris was commissioned by the B.A. to compare the performance of Whewell's and Osler's anemometers, finding the former less satisfactory because of friction in its mechanical parts.[84] Whewell was unable to devote the time to the anemometric field that he would have liked, though Osler certainly made use of the B.A. to propagate his views on this subject and to establish himself as a leading savant in the area of anemometry.

Anemometry was but one aspect of meteorological instrumentation in general, which was one of the main areas of interest of the B.A., and this may be seen as a manifestation of the desire among those interested in physical science at the time to collect large quantities of data on all manner of terrestrial phenomena - meteorological, magnetical, data on tides, and so on, with the view to establishing laws. The key point is that the philosophers saw themselves as the most reliable source of knowledge on the subject of instruments, and regarded the men who made the instruments as playing a subordinate role. Thus, for example, Forbes, in his report on meteorology for the second meeting of the B.A., mentions with respect developments made by some of the more prominent instrument makers:

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[83] W.Whewell, "On the Principle of Mr.Whewell's Anemometer", *B.A.A.S. Report, Liverpool, 1837,* part 2, pp.32-3.

[84] W.Snow Harris, "Report on Professor Whewell's Anemometer, now in Operation at Plymouth", *B.A.A.S. Report, Glasgow, 1840,* part 1, pp.157-62.

> Among other ingenious devices for diminishing the risk of breakage, one has been proposed by Mr.Jones of Charing Cross, by constructing the tube wholly of iron... Mr.Robinson... has lately constructed a barometer in which the tube consists of two parts, capable of being screwed together at the moment of observation...[85]

However, these developments may be regarded as being on a *practical* level, and those innovations of *principle* with which Forbes concerns himself, are considered to have been made by philosophers. For example, in the same volume, John Phillips, one of the secretaries of the B.A., and a geologist of some repute, contributed a description of a new self-registering thermometer which he had designed in which he made it clear that he was providing an innovation in the *principle* of construction of the instrument:

> The advantage of this invention is stated to be, the acquisition of an instrument, capable of exactly the same delicacy and exactness as the best mercurial thermometer, possessing the same durability as that instrument, and applicable to measure the extremes of heat in a variety of positions - objects to which the ordinary maximum thermometer is, from the principles of its construction, entirely inadequate...[86]

In a supplementary report on meteorology, made at the Glasgow meeting in 1840, Forbes discussed the instruments used in meteorology at rather more length, and when referring to thermometers, came to the conclusion that each maker, lacking the insight of the philosophical mind, had failed to realise that there was a zero error in their warranted standard

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[85] J.D.Forbes, "Report upon the Recent Progress and Present State of Meteorology", *B.A.A.S. Report, Oxford, 1832,* pp.196-258, on p.226.

[86] J.Phillips, "Description of a new self-registering Maximum Thermometer", *B.A.A.S. Report, Oxford, 1832,* pp.574-5.

thermometers. Forbes ascribed this to molecular expansion of flint glass bulbs - even in the top Troughton and Simms thermometer Forbes found a zero error of 0.33^{0}F due to this cause.[87] While accepting that the instrument makers engaged on meteorological apparatus lacked something of the philosophical expertise of the man of science such as himself, Forbes, in descriptions of instruments of his design which he communicated to the B.A., as a rule mentioned the maker who executed his designs for him, presumably feeling that the mechanical skill of a tradesman should be made known to the Association. Although the philosopher was seen as doing the important work, then, the maker could also benefit from his role in the project. Such a mention by the philosopher was therefore not only courteous and gentlemanly, but could be of financial advantage to the maker. Thus, at the 1832 meeting, Forbes exhibited and explained the construction of an improved portable barometer that he had designed, and he stated that it had been executed for him by "Mr.Robinson of Devonshire Street".[88] Among his favourite makers seem to have been Alexander Adie and his son John; Edinburgh men like Forbes. He particularly liked additions which Alexander Adie had made to a type of hygrometer.[89]

While a mention in a paper by an established man of science such as Forbes meant valuable publicity for a maker, and

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[87] J.D.Forbes, "Supplementary Report on Meteorology", *B.A.A.S. Report, Glasgow, 1840,* part 1, pp.37-156, on p.46.

[88] *B.A.A.S. Report, Oxford, 1832,* pp.575-6.

[89] Forbes, *op.cit.* (note 85), p.241. For an extensive discussion of the Adie business, see Clarke, Morrison-Low, and Simpson, *op.cit.* (note 59), pp.25ff.

taking part in Association meetings could be valuable in career terms, as we have seen for example in the cases of Dent, Pritchard, Newman, and others, the most direct benefit which an instrument maker could hope to derive from the B.A. was to be commissioned to provide apparatus for a research project initiated by the philosophers in control in the Association. In the early years, this could mean supply of apparatus for magnetical observations (the B.A. spent £247 12s.6d. on this in 1840 and 1841)[90] or perhaps supply of meteorological instruments such as thermometers for observations to be made at some particular station. However by 1842 the B.A. had gained control of Kew Observatory, and in order to equip that site with up to date instruments it was necessary that a number of makers receive valuable commissions from the B.A.. As was the case with commissions from Greenwich Observatory, securing such an order was, for the maker, not only rewarding in direct financial terms, it was also advantageous to his reputation. Kew was important therefore not only to those philosophers who wished to promote the cultivation of the subjects which came within the province of Section A, as the proper recipients of the Association's resources, but also for those who derived income by building instruments for it.

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[90] O.J.R.Howarth, *The British Association for the Advancement of Science: A Retrospect,* (2nd edition, London, 1931), p.267.

5. Kew Observatory as a Vehicle of Career Interests.

Kew Observatory was acquired on behalf of the B.A. in 1842. The previous year the Government had decided that they no longer wished to keep up the Observatory and Museum at Kew which had been established by George III. The contents were distributed to the British Museum, King's College London, the College of Surgeons, Armagh Observatory, and some members of the Royal Family. The vacant building was then applied for by the Royal Society on the recommendation of their Committee of Physics and Meteorology, and the application was granted.[91] However the Society's Council applied to the aforementioned Committee again, asking it to report:

> to what scientific purposes it would be desirable to appropriate the building formerly occupied by the Observatory at Kew... and what would be the probable annual expense of applying it to such purposes.[92]

The Committee reported that:

> ...they do not consider that any regular and systematic course of physical observations at present devisable could be therein advantageously made by the Society, or by any observer under their immediate appointment and direction.[93]

They did indicate, however, several other scientific purposes to which it might be put. The expenses estimated for the running of the establishment by the committee, which included Herschel,

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[91] C.Wheatstone, "Historical Remarks by Sir Charles Wheatstone", Minutes of Council, 11 December, 1869, cited in R.H.Scott, "The History of the Kew Observatory", *Proceedings of the Royal Society of London,* 1885, pp.37-86, on pp.47-52.

[92] Resolution of Royal Society Council, 11 November 1841, cited in Wheatstone, *op.cit.* (note 91).

[93] Report of Royal Society Committee of Physics and Meteorology, 10 February, 1842, cited in Wheatstone, *op.cit.* (note 91).

Wheatstone, and Sabine, were £27 p.a. for the wages of a resident, £5 p.a. or less for repairs, plus taxes, rates, and the interest on the money initially expended.[94] This estimate by the committee was sufficient to convince the Council that it did not seem to be "expedient" to occupy the Observatory at Kew.[95]

Several Fellows of the Royal Society, and members of the B.A. were still keen that Kew should be secured for the purposes of science, and they urged that an application be made for Kew in the name of the B.A.. In order to convince those in authority in the B.A. that this was a desirable course to take, several uses to which Kew might be put were prepared. These were based in part upon the list drawn up by the earlier Royal Society committee:

> 1. It will be a repository for, and place for occasional observation and comparison of the various instruments which the recent discoveries in physical science have suggested for improving our knowledge of meteorology etc...
> 2. A repository and station for trial of new instruments...Among instruments which have been proposed, and which will probably not be constructed and brought into use without the assistance which an Institution like this alone can afford, may be mentioned: a universal meteorograph, which will accurately record half-hourly indications of various meteorological instruments... an apparatus for recording the direction and intensity of the wind simultaneously at different heights...an apparatus for telegraphing the indications of meteorological instruments carried up in balloons...
> 3. As a station to which persons...may bring their instruments for the purpose of comparison with the standard instruments there deposited...
> 4. As a depository of a complete set of the magnetic instruments at present in use...in order

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[94] *Ibid.*

[95] Report of the Royal Society Council, 10 March 1842, cited in Wheatstone, *op.cit.* (note 91).

> that any person desirous of so doing may understand their construction and acquire their use. The only magnetic observatory in England is at Greenwich, and the instruments, being in constant use, cannot be employed for the purposes here mentioned.
> 5. A complete series of apparatus for experiments on atmospheric electricity...
> 6. One of the rooms to be fitted up as an optical chamber with a Heliostat, Fraunhofer's prismatic telescope, photometers etc., principally for the purposes of optical astronomy, a subject at present totally neglected.
> 7. As complete a collection as possible of the measuring instruments employed in the various branches of physical science, for the purpose of obtaining accurate quantitative results...[96]

By May 1842 the B.A. was in control of Kew, and would remain so until 1872, when it was handed over to the Royal Society. During this period the Association spent £12300 on Kew[97]: in the early years of its existence it had a reasonable share of the Association's research expenditure, e.g. £200 out of £1877 at Cork in 1843,[98] and £180 out of £1421 at York in 1844,[99] but by the following decade Kew was receiving most of the money which the B.A. granted for scientific purposes, e.g. the Observatory received £300 out of a total of £448 at Edinburgh in 1850.[100] Upon the B.A. taking over the establishment, Charles Wheatstone superintended its equipping with (i) an ordinary meteorological record with standard instruments (ii) a meteorological record

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[96] Application to the Government from Herschel et al., cited in Wheatstone, *op.cit.* (note 91).

[97] Howarth, *op.cit.* (note 90), p.158.

[98] Synopsis of Grants of Money appropriated to Scientific Objects, *B.A.A.S. Report, Cork, 1843,* pp.xxiv-xxv.

[99] Synopsis of Grants of Money appropriated to Scientific Objects, *B.A.A.S. Report, York, 1844,* pp.xxv-xxvi.

[100] Synopsis of Grants of Money appropriated to Scientific Objects, *B.A.A.S. Report, Edinburgh, 1850,* p.xxiv.

with self-registering instruments of his own design (iii) a record of the electrical state of the atmosphere.[101] However, after only three years, the Council of the B.A. was asked by the General Committee to consider a proposal to relinquish control of the Observatory.[102] From early in the life of the B.A. it had been accepted that the permanent maintenance of any research work or institution by the Association should be avoided. The running of Kew clearly was contrary to this principle. However, as shown by Morrell and Thackray, the cultivators of Section A sciences were very much in power in the B.A.,[103] and the Committee appointed to consider the expediency of discontinuing the Observatory - Herschel, Peacock, Airy, Wheatstone, Sabine, and Professor Graham, prepared a report on 7 May 1846 which showed that they recommended unanimously that Kew continue to be maintained by the Association. The reasons they gave were as follows:

> 1. Because it affords a local habitation to the Association, and a repository for its books, manuscripts, and apparatus.
> 2. Because it has afforded to Members the means of prosecuting many physical inquiries which otherwise would not have been entered upon.
> 3. Because the establishment has already become a point of interest to scientific foreigners...
> 4. Because the grant of the occupancy of the building by Her Majesty... is an instance of Her Majesty's interst in, and approval of, the objects of the Association.
> 5. Because if the Association... relinquish the establishment, it will probably never again be

[101] Report of the Council to the General Committee, *B.A.A.S. Report, Cork, 1843,* p.xxxviii.

[102] Report of a Committee to the Council, 7 May 1846, *B.A.A.S. Report, Southampton, 1846,* pp.xvii-xviii.

[103] Morrell and Thackray, *op.cit.* (note 1), pp.267-75.

> available for the purposes of science.
> 6. [Because atmospheric electricity work is going well].
> 7. Because other inquiries into self-registering apparatus...are in actual progress...
> 8. Because the access to the Observatory from London... will shortly be greatly improved by railroads...[104]

All these reasons were of some validity. As far as the members of the Committee were concerned, however, implicitly the most important was reason 2 - Kew had been useful to them in their own researches.

Notwithstanding this convincing report, which at least secured the temporary continuance of Kew, proposals were made at succeeding meetings that the B.A. could not hope to sustain the Observatory for much longer. Thus at Swansea in 1848, a "Kew Committee" reported that in view of the state of funds of the Association it would be impracticable to continue Kew on a permanent basis, and that it should be discontinued as early as "practicable", i.e. as early as it could be usefully handed over to the Government.[105] The General Committee concurred in this wish of a Committee of the Council, and they asked the Council to consider what steps might be necessary in order to relinquish control of the Observatory.[106] Despite this resolution, it was deemed expedient to establish a fund to defray the cost of pursuing investigations then in progress, and another sum for the reduction of electrical observations which had been carried out between August 1843 and August 1848.[107] However, by

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[104] Report of Committee, *op.cit.* (note 102).
[105] Report of the Kew Observatory Committee, *B.A.A.S. Report, Swansea, 1848*, pp.xix-xxi.

[106] *Ibid.*
[107] *Ibid.*

1850 the expense of Kew as a research establishment had been partially sustained by a grant from the Royal Society.[108] The Kew Committee of that year reported that in future no consistent and regular observation would take place at the Observatory, such as had been the case with atmospheric electricity, but that Kew would be strictly an *experimental* observatory, devoted to opening out new physical inquiries, but only in very selected instances to preserve continuous records of phenomena.[109] With the grant of £100 from the Royal Society, whose funding came from the Government, and further such grants in succeeding years (e.g. the grant from Government sources exceeded that from the Association's own funds in 1851) Kew was not in danger of running into debt, and it was considered that such financial arrangements worked well so that there was no further hesitation in maintaining Kew for the use of the B.A..[110]

Francis Ronalds acted as honorary superintendent of Kew Observatory from 1842-1851, and gave frequent reports to B.A. meetings during that period as to the state of the instruments and researches there.[111] In his early years in charge Ronalds was much concerned with atmospheric electricity, but he also spent considerable time on the perfection of the photographic processes

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[108] Report of the Kew Committee, *B.A.A.S. Report, Edinburgh, 1850,* pp.xx-xxi.

[109] *Ibid.*

[110] Report of the Proceedings of the Council, *B.A.A.S. Report, Ipswich, 1851,* p.xxviii.

[111] Ronalds did not write the first year's report: Report of the Committee appointed by the Council to superintend the establishment of Meteorological Observations at Kew Observatory, *B.A.A.S. Report, Cork, 1843,* pp.xxxix-xl.

for the registration of meteorology and terrestrial magnetism.[112] The name of Kew has been permanently associated with these types of researches. In his report for 1844, which dealt with the activity of the Observatory between 1 August 1843 and 31 July 1844, Ronalds discussed at some length the instruments with which Kew had become equipped.[113] John Newman seems to have received the greater part of the commissions to build new instruments - for example he supplied standard, maximum, and minimum thermometers, and also built the principal conductor, along with its appendages and instruments in the electrical observatory, items which, according to Ronalds, "do him very great credit".[114] Newman had also constructed a mountain barometer, which had been lent to the Observatory by Sabine, until it could afford a standard instrument.[115] A certain type of anemometer, Lind's anemometer, constructed by Watkins and Hill, was found not to be sensitive enough for Kew's purposes, so Ronalds invented his own cruder but more efficient balance anemometer.[116] The Observatory also contained some instruments by foreign makers, usually instruments which might not so readily be obtained from London makers; for example an atmospheric galvanometer with 2400 coils by Gourjon of Paris, and a Saussure's hygrometer by Richer of Paris.[117]

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[112] Scott, *op.cit.* (note 91), p.56.

[113] F.Ronalds, "Report concerning the Observatory of the British Association at Kew, from August the 1st, 1843, to July the 31st, 1844", *B.A.A.S. Report, York, 1844,* part 1, pp.120-42.

[114] *Ibid.,* p.125.

[115] *Ibid.,* p.127.

[116] *Ibid.,* p.129.

[117] *Ibid.,* pp.124, 128.

Newman's success in making the complete electrical apparatus for Kew paid off when in 1847 he was commissioned, by the East India Company, to make an identical set for Bombay Observatory.[118] Ronalds' report for 1849 for the first time contained an account of the state of the various instruments - those made by Newman seem to have been performing well and were in good condition. An electrometer by W.T.Henley was rather less efficient than in 1843, but Ronalds attached no real blame to the maker, as friction of the pivots, the problem here, was apparently always bad in electrometers.[119]

With the new grant of £100 for instruments having been made by the Royal Society, Newman received orders from the B.A. to supply assorted magnetographs, of which the optical parts were to be furnished by Andrew Ross,[120] who specialised in microscopes, and apparently gained some of his theoretical knowledge from J.J.Lister, who showed him how an aplanatic lens could be made.[121] Newman made a declination magnetograph and a horizontal force magnetograph for Kew, and a vertical force magnetograph for Toronto Observatory, which was complete towards the end of 1849.[122] The vertical force magnetograph for Kew itself was made

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[118] Report of the Kew Observatory Committee, *B.A.A.S. Report, Swansea, 1848,* pp.xvii-xxi, on p.xviii.

[119] F.Ronalds, "Report concerning the Observatory of the British Association at Kew, from Aug.9, 1848, to Sept.12, 1849", *B.A.A.S. Report, Birmingham, 1849,* part 1, pp.80-7, on p.81.

[120] *Ibid.*

[121] Taylor, *op.cit.* (note 30), p.459. See also Chapter 8.

[122] F.Ronalds, "Report concerning the Observatory of the British Association at Kew, September 12, 1849, to July 31, 1850", *B.A.A.S. Report, Edinburgh, 1850,* part 1, pp.176-93, on p.181.

by Ross and Henry Barrow, Thomas Robinson's successor.

In 1852, the Council directed that the Kew Committee be authorised to present standard mercurial thermometers to certain of the philosophical instrument makers.[123] In practice this meant those makers who had a high reputation in the construction of thermometers, and from whom thermometers might in the future be ordered by the Association or by prominent members within it. In order that the best possible instruments be obtained in the future, then, it was only natural that the B.A. should give whatever aid was in their power to achieve this end. The makers to whom thermometers were sent were an elite of only six in number: Adie, Barrow, Newman, Watkins and Hill, Negretti and Zambra, and Troughton and Simms.[124]

In 1854, the Kew Committee reported that a model of a thermometer to be used by ships at sea had been sent to each of the philosophical instrument makers, and they had been asked to give estimates of the cost at which they could undertake to construct such an instrument.[125] Clearly the list of instrument makers was longer than the list of those to whom the standard thermometer had earlier been sent, for one of the most favourable estimates came from Casella and Company. Negretti and Zambra, and

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[123] Report of the Proceedings of the Council, *B.A.A.S. Report, Belfast, 1852,* p.xxviii.

[124] *Ibid.* The "Adie" referred to in this list was Patrick Adie, son of Alexander and brother of John, his more famous Edinburgh relations, who operated his instrument making business in London. For details of his career, see Clarke, Morrison-Low, and Simpson, *op.cit.* (note 59), pp.75ff.

[125] Report of the Kew Committee, *B.A.A.S. Report, Liverpool, 1854,* pp.xxvii-xxxvi, on p.xxvii.

Casella, both English firms of Italian origin, gained the commission.[126] In addition, the United States Navy was persuaded to use the same companies, and they ordered 500 thermometers from each.[127] In the same year, Kew became equipped with *standards* of various instruments - a standard barometer constructed by Negretti and Zambra, standard weights made by Oertling (a Council Medal winner at the Great Exhibition of 1851) under the direction of Professor W.H.Miller, and a standard scale by Troughton and Simms.[128] In addition, a maximum thermometer by Negretti and Zambra, and a minimum thermometer by Patrick Adie, of 395 Strand, were on display.[129] The Committee was particularly impressed with Negretti and Zambra's thermometer:

> ..the very ingenious instrument of Messrs.Negretti and Zambra has one quality which, as regards durability, places it above every other form of maximum thermometer, for once well constructed, it can never get out of order; it is somewhat difficult in construction, and consequently more costly...[130]

The Committee pointed out that Phillips' thermometer was "most valuable for its extreme simplicity"[131] and capable of greater delicacy than Negretti and Zambra's owing to the *principles* of its construction. However the philosopher's design was lacking in the *practical* quality of durability.

The thermometers for use at sea were reported on at the Glasgow meeting of 1855 by John Welsh, by then superintendent at

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[126] *Ibid.*
[127] *Ibid.*
[128] *Ibid.*, pp.xxxi-xxxii.
[129] *Ibid.*, p.xxxii.
[130] *Ibid.*, p.xxxiv.
[131] *Ibid.*, p.xxxiv. See also Phillips, *op.cit.* (note 86).

Kew.[132] Welsh was satisfied with the accuracy of graduation of the thermometers by both Casella, and Negretti and Zambra. He was not so well pleased with the uniformity of the instruments from a mechanical point of view:

> ...the diameter of the bulbs has been too irregular, and in many cases considerably more than is desirable - the range of the graduation has differed in many instances excessively from that prescribed in the instructions of the Kew Committee, and even the dimensions of the mere *material* have been too little attended to, at least in some of the instruments more recently made by Negretti and Zambra.[133]

In other words this work provides another example of the philosopher emphasising his authority and control over the instrument maker, whose work was here regarded as inadequate. Hygrometers, meanwhile, had been made for the use of the Admiralty not only by the two makers who received the thermometer commission, but also by Patrick Adie. Welsh opined that those by Casella were by far the best both in terms of accuracy and suitability for practical work (those of the other makers were far too fragile).[134] Such publicity would not have been welcomed by Negretti and Zambra; however it was also implied that there were few firms who could provide instruments to the standard required. For example, in discussing the proposed penalty to the maker on lack of punctual delivery - that the Admiralty or Board of Trade should purchase the instruments elsewhere, the maker defraying the extra cost, Welsh pointed out that:

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[132] Report of the Kew Committee, *B.A.A.S. Report, Glasgow, 1855,* pp.xxx-xlv, which includes J.Welsh to J.P.Gassiot, 27 August, 1855, on pp.xxxviii-xxxix.

[133] *Ibid.,* p.xxxviii.

[134] *Ibid.,* pp.xxxviii-xxxix.

terms. This was the case with Newman, Casella, Negretti and Zambra, and others. Government bodies such as the Admiralty and the Board of Trade received a service which enabled instruments required for practical use (as distinct from research) to be compared with standards. Indeed between 1854 and 1869, over 11000 thermometers, nearly 2500 barometers, and nearly 2500 hydrometers were verified at Kew.[138] However, this rating was performed by philosophers. It was not seen as a task which the makers themselves could perform, and in this way the constructors of the instruments were assigned a role subordinate to those who used them and pronounced on their quality. The business of Kew was effectively controlled by a group of philosophers, cultivators of the physical sciences granted pride of place in the B.A.. They saw it as an effective base for researching those subjects, such as meteorology and terrestrial magnetism, whose study could be legitimated in terms of their importance to the nation. Research in such subjects was in turn seen by these men as a means of demonstrating their personal status and importance to the State, and was thus a way of enhancing their own careers. The B.A. Presidential Address by the Duke of Argyll at Glasgow in 1855 showed the high esteem in which the work of the philosophers on the Kew Committee was held:

> The thanks of the commercial, as well as of the scientific world are due to Colonel Sabine and the other members of the Kew Committee, whose

[138]Howarth, *op.cit.* (note 90), p.161.

> assistance is now highly appreciated by practical men, and eagerly sought for by the best instrument makers..[139]

In the following sections I would like to develop this theme of the uses philosophers could make of the B.A., having discussed the specific areas in which they were able to emphasise their authority over instrument makers, such as in the work of Kew Observatory. This analysis also follows on from that in earlier chapters concerned with individual members of the scientific elite.

6. Philosophers and the Acquisition of Elite Status.

The first half of the nineteenth century was characterised by a number of different groups of actors *claiming knowledge* - for example Tractarians and Unitarians, to name but two such groups.[140] Knights has argued that a number of significant writers and social thinkers in the nineteenth century believed in the importance to social well-being of those possessing knowledge - of the literary and philosophic class.[141] This should not be confused with the advocacy of a hierarchical social system, as in Plato, with philosopher-rulers at the head: indeed the social thinkers discussed felt politics to be of subordinate importance. Rather, the *clerisy,* as those possessing the knowledge were termed, notably by Samuel Taylor Coleridge, "were to work on

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[139] Address by the Duke of Argyll, *B.A.A.S. Report, Glasgow, 1855,* PP.lxxiii-lxxxvi, on pp.lxxix-lxxx.

[140] See Morrell and Thackray, *op.cit.* (note 1), pp.17-21, for a discussion of aspirants to the title of "men of knowledge".

[141] B.Knights, *The Idea of the Clerisy in the Nineteenth Century,* (Cambridge, 1978).

that substance of national life upon which its political institutions were based - its opinions, its language, and its concepts of ethical action".[142] As Knights continues:

> The clerisy was to do for society something that society, unaided, could not do for itself. It was to render susceptible of comprehension the raw matter of experience under the heading of transcendent principles. Thus the clerisy was to act as the active mind of society.[143]

Writers such as Coleridge and John Stuart Mill emphasised the power of an elite whose skill in speculative philosophy was essential to the cultivation, even, as Knights goes so far as to say, to the existence of the nation.[144] Mill, however, believed that the clerisy had not hitherto been aware of its own importance:

> It has often struck me that one of the many causes which prevent those who cultivate moral and political truth from occupying the place and possessing the influence which properly belong to them as the instructors and leaders of mankind, is that they never consider themselves as other labourers do, to constitute a *guild* or fraternity, combining their exertions for certain common ends, and freely communicating to each other everything they possess which can be used to promote these ends.[145]

It should be apparent that this notion of the clerisy is equally applicable to those who I have been claiming saw themselves as part of the emergent scientific elite, and whose increasing sense of self-consciousness had been manifested in the formation of the British Association. In particular in recent

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[142] *Ibid.*, p.6.
[143] *Ibid.*, p.8.
[144] *Ibid.*, p.63.
[145] J.S.Mill to J.P.Nichol, 16 January 1833, in F.E.Mineka (ed.), *The Earlier Letters of John Stuart Mill 1812-1848,* (2 volumes, Toronto, 1963), p.136.

historiography of the B.A., another of the groups seen as claiming knowledge and regarding themselves as influencing the state of the nation in this way was a set of liberal Anglicans who advocated moderate reform and who were to some extent centred around Trinity College, Cambridge, where Whewell and Sedgwick were fellows. Cannon terms this group the "Cambridge Network"[146]; it also gave rise to many of the main office-holders in the early years of the B.A. - Morrell and Thackray's "Gentlemen of Science".[147] As the latter authors declare of these men: "In their avowals of science, of religion, and of objective truth, they took and skilfully defended high intellectual ground".[148] Although Morrell and Thackray's Gentlemen of Science were by no means exclusively centred on Trinity College, Cambridge, including for example Scots such as Brewster and Forbes, Irishmen such as William Rowan Hamilton, and provincials such as John Phillips and Vernon Harcourt, and although their orientation was by no means exclusively liberal Anglican (Dalton, a Quaker, was among them), the leaders of the B.A. had in common their attitudes to the *status* which cultivators of science should enjoy. Some, for example Brewster, were staunch declinists, and advocated government support for men of science.[149] Others, such as Whewell, were hostile to the doctrines of the declinists, and believed science should be left to individual initiative.[150] *All* the Gentlemen of Science however believed that those who pursued

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146 Cannon, *op.cit.* (note 7), pp.29-72.
147 Morrell and Thackray, *op.cit.* (note 1).
148 *Ibid.,* p.22.
149 Brewster, *op.cit.* (note 2).
150 W.Whewell to W.V.Harcourt, 22 September 1831, Whewell Papers, 0.15.47[97], Trinity College, Cambridge.

science ought to command a high social status as a consequence of their expertise in this area, and they saw the B.A. as one vehicle by which they could achieve that status. Morrell and Thackray go so far as to say that the Gentlemen of Science not only saw themselves as having expertise in an area of national importance, nor only as spokesmen for cultivators of science in general, but as "the anointed interpreters of God's truth about the natural, and hence the moral, world".[151]

Mill's writings, meanwhile, though mainly concerned with those seeking moral and political truth, extended the Coleridgean notion of the clerisy to give those engaged in the pursuit of scientific truth a place within it, and advocated their recognition by society:

> The cultivation of speculative knowledge, though one of the most useful of all employments, is a service rendered to a community collectively, not individually, and one consequently for which it is, *prima facie,* reasonable that the community collectively should pay...It is highly desirable.. that there should be a mode of insuring to the public the services of scientific discoverers, and perhaps of some other classes of savants, by affording them the means of support consistently with devoting a sufficient portion of time to their peculiar pursuits. The fellowships of the Universities are an institution excellently adapted for such a purpose; but are hardly ever applied to it, being bestowed, at the best, as a reward for past proficiency, in committing to memory what has been done by others, and not as the salary of future labours in the advancement of knowledge.[152]

Mill's writings considered *savant* as a category divorced from any political and religious affiliations, and my thesis

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[151] Morrell and Thackray, *op.cit.* (note 1), p.29.
[152] J.S.Mill, *Principles of Political Economy,* (London, 1848). New edition, edited by W.J.Ashley, (London, 1909), pp.976-7.

extends upon that of Morrell and Thackray in as much as, where they consider their "Gents" to be significantly homogeneous in political and religious terms, I argue that such connections are secondary to their sense of class-consciousness as pursuers of scientific truth. It is this that sets them apart from the Tractarians, Unitarians, and instrument makers, and not their liberal Anglicanism.

As has been touched upon earlier in this chapter, much of the literature on the position of men of science in this period uses the vocabulary of "professionalisation". A number of articles assume that around the 1830s there was a growing desire among men of science to become professionals. Only the most naive of historians would maintain that what is meant by this is that men of science were pressing to become employed in government institutions, which is perhaps what one's definition of a modern scientific professional would include. Declinists such as Brewster and Babbage advocated financial rewards for men of science from the Government, but even they cannot be said to have striven to "professionalise" science. A more useful vocabulary to employ in this situation, as I have been advocating, is that involving the notion of *elite status.* The controversy over the Royal Society election in 1830 was therefore not one between professionals and amateurs, but rather between those who wished to construct an elite, containing themselves, of men who had devoted their lives to the cultivation of science, and those who were indifferent to the construction of such a group, perhaps because they could not legitimately claim to be part of it, having in many cases only a passing connection with scientific pursuits.

In the early part of the century, then, science was becoming an activity by which one who spent most of his time and effort in this area might maintain or improve his social status - an activity to which a gentleman might devote his life without also having to be a member of one of the traditional professions of doctor, lawyer, or cleric. The successful gentlemanly cultivator of science would be able to justify the time he devoted to scientific pursuits by the reputation he gained among his peers, and by the benefits to society which accrued as a result of his work. This ideology was shared, as I have shown, by Wheatstone, Babbage, Airy, and Faraday, without their considering themselves professionals. Cannon's definition of a "professional scientist" may here be usefully quoted if we remember that what she is really defining are the characteristics of a member of the elite group engaged in scientific pursuits for whom the B.A. was such an important forum for their aspirations:

> A professional scientist was characterised by the facts that he was involved in an endeavour of recognised social status, and that he recognised its own standards of merit (he did not appeal to a general "public"; it was a "self-reviewing circle"), but he was distinguished from the gentleman, the amateur, the patron, the dilettante, the interested cleric, and the retired naval officer, by his long-term attention to science (not necessarily *a* science) as his major activity, by his technical expertise, and by the quality and number of his accomplishments.[153]

Besides justifying their activity in terms of the social status which it conferred upon them in the eyes of their peers, it was important for the men of science to legitimate scientific

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[153] Cannon, *op.cit.* (note 7), p.150.

pursuits by demonstrating that the wealth of the new industrial Britain was closely dependent upon the researches which they, as philosophers, were carrying out. The would-be member of the new scientific elite could justify his position in society as being a vital one in the creation of national wealth. In particular, his was a more important role than that played by the artisan in the industrial society. This quest for status by cultivators of science, however, had as a necessary corollary that certain other groups would *not* achieve such status. In the eighteenth century the instrument makers enjoyed a privileged position in the scientific community, and displayed the expertise they possessed by extensive contributions to learned journals, and by development of new instruments. In the early nineteenth century, this quest for status on the part of philosophers necessarily excluded those such as instrument makers, who could be seen as doing little more than manual work, and could not be regarded as "God's anointed interpreters" like the men of science. The earlier chapters in this thesis have shown how this affected the makers' status in the Royal Society, and how their treatment by savants such as Wheatstone, Babbage, Airy, and Faraday determined their standing in the scientific community as a whole. This chapter has shown how they were also relegated to a peripheral role in the affairs of the B.A., which I have considered as the institutional manifestation of the motives of the men of science as individuals, and it has been argued that the main advantage the makers could derive from the B.A. was publicity in scientific circles and consequent employment by philosophers.

Men of science such as Whewell and Forbes, then, were able to use the B.A. for what they saw as a far more important purpose than mere money - they could use it to demonstrate their expertise and so elevate their reputation. In general, for those active in the B.A., scientific knowledge was not something on which they wanted to put a price. The Gentlemen of Science committed themselves to science as a career "not for a livelihood but for a lifetime".[154] Their interest was not with science as a means of earning a living, but rather as a "vocation or personal calling",[155] as in many cases they were already financially secure. So it was the intellectual *status* given by pursuit of science that was their goal, a desirable acquisition in this period in which individual status seems to have been of importance to a number of widely disparate groups of actors. Clearly not all the members of the B.A. would be able to ignore financial commitments so emphatically as, for example, Herschel, but the motive of achieving status was held even by those such as Airy, Babbage, and Brewster, who were not so financially secure. Airy, indeed, held out for increases in his salary on more than one occasion, after being put in charge at Cambridge and Greenwich Observatories, and was each time successful.[156] Babbage and Brewster had to depend on institutional support to fund

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[154] Morrell and Thackray, *op.cit.* (note 1), p.424.
[155] *Ibid.*, p.33.
[156] See G.B.Airy, *Autobiography of Sir George Biddell Airy, edited by Wilfrid Airy*, (Cambridge, 1896), pp.79-80, and A.J.Meadows, *Greenwich Observatory. Volume 2: Recent History (1836-1975)*, (London, 1975), p.1.

certain of their projects.[157] But these men were not motivated by greed, rather by a desire to receive the recognition which they felt they merited. Nonetheless, they saw the B.A. primarily as an organisation through which their career interests could be promoted (in terms other than financial); the leading members of the Association were all of the opinion that the men of science deserved a high social status.

Individuals belonging to the B.A. differed, however, as to the form which the acquisition of this status should take. Whewell saw rational knowledge as the preserve of the "leading cultivators of science".[158] His analysis confirmed that the historical development of science through successive "inductive epochs" showed that an *elite* had always been "at the head of the queue when the need was for a consilience of inductions". It was also an elite which made bold deductive assertions which in time proved correct.[159] This the group did in a mysterious way, which bodies such as the B.A. could not directly encourage. For Whewell, the man of science who was a member of this elite had the quality of genius; a *je ne sais quoi.* By implication, of course, Whewell was a member of the current generation of this group. It was his opinion, based on his analysis of scientific advance in historical and philosophical terms, that control and

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[157] See Chapter 4 of this thesis, and also A.Hyman, *Charles Babbage: Pioneer of the Computer,* (Oxford, 1982).

[158] W.Whewell, *The Philosophy of the Inductive Sciences, founded upon their History,* (2 volumes, London, 1840), Volume 2, p.373.

[159] W.Whewell, *Astronomy and Physics considered with Reference to Natural Theology,* (London, 1833), pp.263-94.

authority had to be vested in a narrow elite of philosophers, or "scientists", as he was calling them by 1835.[160] Whewell argued that the leading role within the B.A. had to be taken by experienced men of science, and in fact he wished to reduce the number of members who had only a peripheral interest in science - otherwise it would be no better than the Royal Society with its crowd of lay members.[161]

So for Whewell, the qualities possessed by the philosopher, or "scientist", were genius, and inventiveness. All cultivated men could aspire to the status of the philosophical elite, but Whewell's definition excluded mere artisans such as instrument makers, who ought not to possess authority or be able to exert control, whether in the B.A. or elsewhere. The Association could be used to stimulate the men of science, but science itself was not seen as a collaborative enterprise, rather as one which was advanced by heroic individuals.[162] In particular, men of science were not to be supported by the Government:

> I believe that, in England at least, men of science as a body will secure their dignity and utility best by abstaining from any systematic connection or relation with the government of the country, and depending on their own exertions.[163]

While this opinion was strongly opposed to that of a declinist such as Brewster who advocated government support, these two men of quite different backgrounds shared the view

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[160] M.Shortland, "A Mind for the Facts: Some Antinomies of Scientific Culture in Nineteenth-Century England", *Archives Internationales d'Histoire des Sciences,* 1986 (36), pp.294-324, on p.321.

[161] *Ibid.*

[162] *Ibid.,* pp.323-4.

[163] Whewell to Harcourt, *op.cit.* (note 150).

that the man of science was something special, and both maintained that *democracy* in science ought to be resisted,[164] for those men who did the groundwork for men of science i.e. observers (or instrument makers, for that matter), had not the claim to status that philosophers such as Whewell and Brewster had.

While professing that science depended on individual exertion and not on institutional support, Whewell accepted that an organisation such as the B.A. could be of use in stimulating the efforts of individual philosophers. John Herschel was rather more sceptical of its usefulness. He concurred with Whewell in as much as he supported his view that societies should exist apart from the Government, that they should not try to influence the Government, but should endeavour to gain such a reputation that those in power would naturally turn to them.[165] However, he did not believe, at least in the early years of the B.A., when it was an open question whether or not he would attend, that a large organisation could be effective in stimulating scientific activity simply because, on account of its size, it could not hope to be sufficiently exclusive; "it could not be a group all of whose members had similar expertise".[166] This was a problem

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[164] D.Brewster, "M.Comte's *Cours* of Positive Philosophy", *Edinburgh Review,* 1838 (68), pp.271-308, on p.272.

[165] Cannon, *op.cit.* (note 7), p.192, which is based on J.F.W.Herschel to W.V.Harcourt, 5 September 1831, Harcourt Papers, xiii, pp.244-8, cited in Morrell and Thackray, *op.cit.* (note 62), pp.57-9.

[166] Cannon, *op.cit.* (note 7), p.192. For a fuller treatment of this question, see W.J.Ashworth, *John Herschel and the Ideology of Science,* (M.A. Thesis, University of Kent, 1989).

which applied not only to the B.A., but to all societies which hoped to deal with the entire range of scientific subjects.

Unlike Whewell and Herschel, Babbage felt that the B.A. could be most effective by keeping its membership qualifications relaxed[167] (as it did), but as has been shown elsewhere in this thesis, despite any rhetoric which may imply the contrary, Babbage was as elitist, in regard to the status of men of science, as were the other two. It is also interesting to note that Babbage would not take time off from work on his calculating machine to attend the first meeting of the B.A. at York in 1831, though in future years he took an active part in the proceedings of the meetings, and was for several years one of the Permanent Trustees of the Association, until a quarrel with Murchison in 1838 caused him to resign from this post.[168]

Morrell and Thackray show that Forbes was one of the Gentlemen of Science for whom the B.A. was helpful in career terms.[169] Forbes made a distinction of which both Whewell and Brewster would have approved (though Brewster was a bitter enemy of Forbes after the election for the Edinburgh Natural Philosophy chair which Forbes won in 1833), between the lay membership of the B.A. whose task was to gather up raw materials, and the "projectors of science" - men such as himself, Whewell, and Brewster, whose task it was to mould those raw materials into

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[167] Babbage to Harcourt, *op.cit.* (note 62).

[168] For correspondence pertaining to this issue, see Morrell and Thackray, *op.cit.* (note 62), pp.265-86.

[169] Morrell and Thackray, *op.cit.* (note 1), pp.430-4.

useful scientific theories.[170] As he put it at the 1834 meeting in his home town, "a division of labour is as practicable in intellectual as in mechanical science".[171] Such a statement was unequivocal in its reference to a hierarchy of participants in the scientific enterprise, with the Association's leading philosophers occupying the top positions. Cannon cites a letter from Herschel to Beaufort which illustrates the prevalence of these elitist attitudes in the period, although it is not directly related to B.A. affairs. Herschel was advising Beaufort on a suitable candidate for the post of Astronomer at the Cape of Good Hope:

> You need a man who both knows enough about astronomy as a science to set up and guide his own operations, and also has a practical knowledge of instruments and observing. Your choice is very limited, because the really satisfactory men would not take the post: George Airy (the best), Dr.Robinson, or Richard Sheepshanks...At the other extreme, the multitude of "men who buy fine instruments and even use them to tolerable purpose" almost never learn the principles of astronomy as a science... A group worth considering is that of officers of the armed services who have done surveying, who know practical work and some theoretical principles, and who have enough ability to learn more. There are several of these... A final group is the assistants at Greenwich Observatory. None of these men really have the advanced science needed...[172]

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[170] Shortland, *op.cit.* (note 160), p.321, and J.D.Forbes, "The History of Science, and some of its Objects", *Fraser's Magazine,* 1858 (57), pp.283-94.

[171] Address by J.D.Forbes, *B.A.A.S. Report, Edinburgh, 1834,* pp.xii-xxii, on p.xxii.

[172] J.F.W.Herschel to F.Beaufort, 19 September 1831, Herschel Papers, Royal Society, cited in Cannon, *op.cit.* (note 7 , pp.153-4.

The men of science, in other words, had an inherent self-consciousness which the B.A. served to promote, irrespective of the differing scientific politics of its members.

7. The Hierarchy of Sciences.

While those Gentlemen of Science who have been discussed were in accord on the subordinate place that ought to be held in the B.A. (and even in society in general) by those who were not philosophers, they had the further common ground that their scientific interests lay very much within the domain of the physical sciences. Thus they believed that those lesser mortals who chose natural history, botany, zoology, or medicine as their subject of study should be content to play subordinate roles in any Association dedicated to the advancement of science.[173] A hierarchy of sciences was effectively proclaimed, with Newtonian astronomy at the head, a hierarchy that was explicitly endorsed by giving letters to the sections, the Mathematics and Physics section having pride of place as Section A. As early as 1833, at the Cambridge meeting of the B.A., Whewell dispelled any doubts among the assembled friends of science as to which was the most prestigious science there represented:

> Astronomy, which stands first on the list [of reports on the state of various sciences], is not only the queen of the sciences, but, in a stricter sense of the term, the only *perfect* science;- the only branch of human knowledge in which particulars are completely subjugated to generals, effects to causes... its claims are so fully acknowledged, that the public wealth of every nation pretending to civilisation, the most consummate productions of labour and skill, and the loftiest and most powerful intellects

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[173]Morrell and Thackray, *op.cit.* (note 1), p.28.

> which appear among men are gladly and emulously assigned to the task of adding to its completeness...[174]

As a philosopher interested in astronomy, Whewell's enthusiasm is predictable even if he does get somewhat carried away by his own rhetoric in referring to the process of "adding to its completeness". We would expect the enthusiasm for astronomy, and its mathematical base, of Cambridge-educated mathematicians such as Babbage and Airy to be somewhat greater in this context than that of primarily experimental philosophers such as Wheatstone and Faraday. Charles Daubeny, however, was a chemist rather than an astronomer, and yet he too was explicit in assigning the place at the head of the hierarchy of the sciences to astronomy, in his address at Bristol some three years later:

> All the physical sciences aspire in time to become mathematical; the summit of their ambition, and the ultimate aim of the efforts of their votaries, is to obtain their recognition as the worthy sisters of the noblest of these sciences - Physical Astronomy. But their reception into this privileged order is not a point to be lightly conceded, nor are the speculations of modern times to be admitted into this august circle, merely because their admirers have chosen to cast over them a garb, often ill-fitting and inappropriate, of mathematical symbols...[175]

Daubeny's address went on to discuss the steps that should be taken to stimulate researches in an area below physical astronomy in the order of precedence, but still very much within the province of Section A and her philosophers:

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[174] Address by Whewell, *B.A.A.S. Report, Cambridge, 1833,* pp.xi-xxvi, on p.xiii.

[175] Address by Professor Daubeny, *B.A.A.S. Report, Bristol, 1836,* pp.xxi-xxxvi, on p.xxiii.

> With regard... to Magnetism...much still remains to be done, before the mathematician can flatter himself that a secure foundation for his calculations has been established... there is perhaps no scientific undertaking for which the co-operation of public bodies, and even governments, is more imperiously demanded, and the Association has, in consequence, both engaged its members in the prosecution of these researches, and has promised to obtain for them the national assistance...[176]

However, while he may have been forced to concede that his science, chemistry, did not occupy pride of place in the hierarchy of sciences cultivated by the B.A., Daubeny did indicate in his address that he shared the motives of the Section A philosophers in their ambition for intellectual status, and respect from their peers. According to Daubeny, the presence of distinguished individuals at a B.A. meeting such as that at Bristol, showed:

> ...that other roads to distinction, besides that of mere wealth, are opened to us through the instrumentality of the Sciences, for although, thanks to the spirit of the age, which in this respect at least stands advantageously distinguished from those preceding it, the discoverers of important truths are not, as heretofore, allowed to languish in absolute poverty, yet the debt which society owes to them would be but inadequately paid, were it not for the tribute of respect and admiration which is felt to be their due.[177]

In a manufacturing centre, albeit not a major industrial one, such as Bristol, Daubeny felt the need to stress the importance of philosophical knowledge as being that on which depended the wealth of the new industrial towns. Inventions, technological progress, and the mechanical and chemical arts were to be given a place in the B.A.. However, the Association's

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[176] *Ibid.*, pp.xxiii-xxiv.
[177] *Ibid.*, p.xxxv.

position was that practical and applied subjects were to be honoured, but, as Daubeny had suggested at Bristol, they provided "a derivative illustration of the progress of man and the march of the intellect".[178] The work of engineers, industrialists, and, more implicitly, instrument makers, was thus acknowledged, and such groups were welcomed by the Association. However, they were seen as subordinate participants in the affairs of the B.A., and as such they were liable to be manipulated and patronised by the leading philosophers striving for their own ambitions. From a social point of view, it was desirable to show that representatives of the large engineering concerns on whom the wealth of the manufacturing towns depended were given their place in the Association. Thus Marc Isambard Brunel and George Stephenson were among engineers who served on Committees of Section G, the mechanical science section. Instrument makers were not as "useful", in a social sense, to the B.A., as those engineers with their high public profiles, and with the single exception of George Dollond in 1833, none ever served in a position of authority in the Association. The top posts in Section G were usually held by academics, rather than practical engineers. Thus, in 1837, the President of Section G was Thomas Romney Robinson, with Dionysius Lardner, Wheatstone, and Professor Willis, all academics, as the Vice-Presidents.[179] The following year, although two engineers, Bryan Donkin and George

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[178] Morrell and Thackray, *op.cit.* (note 1), p.261.
[179] Officers of Sectional Committees at the Liverpool Meeting, *B.A.A.S. Report, Liverpool, 1837,* p.x.

Stephenson, were Vice-Presidents, the leadership of the section belonged to Babbage.[180] It was 1861 before an engineer, William Fairbairn, became President of the B.A..[181]

As Morrell and Thackray point out, despite the stress placed on the relationship between theory and practice, and the demonstration by technological achievement of the importance of a study of the principles of abstract science, the consolidation of the B.A. coincided with the *neglect* of the mechanical arts.[182] The practical engineer could not feel comfortable at a meeting at an academic centre such as Oxford in 1832. Even though a mechanical art sub-committee was appointed, it failed to report to the meeting, partly because Babbage, its most eminent member, refused to serve.[183] In the next few years the mechanical arts did not thrive within the B.A., but by the meetings in Dublin in 1835 and Bristol in 1836, a number of men interested in engineering joined the Association, and the mechanical science section, Section G, was firmly established. It is significant, however, that it came to be termed mechanical "science" rather than mechanical "art". This distinction would have been apparent both to practical men, who wished to gain credibility by showing that their subject had a scientific base, and philosophers, who

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[180] Officers of Sectional Committees at the Liverpool Meeting, *B.A.A.S. Report, Newcastle, 1838,* p.xiii.

[181] See *B.A.A.S. Report, Manchester, 1861,* pp.li-lxvii, for Fairbairn's Presidential Address.

[182] Morrell and Thackray, *op.cit.* (note 1), p.257.

[183] *Ibid.* See also C.Babbage to W.V.Harcourt, 12 October 1831, Harcourt Papers, cited in Morrell and Thackray, *op.cit.* (note 62), p.86.

wanted to emphasise that it was the principles elucidated by them which were the main cause of practical advance. Mechanical science was thus a subject "transmissible by symbolic language",[184] whereas the mechanical arts involved techniques transmitted by personal example, and the former came to be preferred as a suitable subject for inclusion by the B.A.. However, there could be no doubt that mechanical science occupied a subordinate place, and was much farther down the hierarchy of sciences from the physical sciences represented in Section A. By the same token, its practitioners could not expect to enjoy the same status as the philosophers, either within the B.A., or in society at large.

Nonetheless, manufacturers and self-made capitalist engineers were ready to accept their station, provided they were able to identify themselves to some extent with the "resources of intellect and power"[185] increasingly represented in the B.A., and other groups such as precision instrument makers were able to use the Association in a similar way for their career purposes. W.J.M.Rankine summed up the relationship between Section G, the practical section, and Section A, the elite theoretical section, by saying that G was to A what the classes of engineering and mechanics in a university were to those of physics and mathematics, i.e. Section G was that which considered "the practical application of those branches of science to whose theoretical advancement Section A... was

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[184] Morrell and Thackray, *op.cit.* (note 1), p.260.
[185] *Ibid.*

devoted".[186]

Thus the leading role in practical advancement, and in the creation of wealth, was ascribed to those who pursued the sciences of Section A. To a lesser extent Section B sciences, i.e. chemistry, were of benefit as well in terms of this creation of wealth. This was the notion that all the leading philosophers of the B.A. had in common - they were the main agents of material advancement for the State, and therefore the positions of power within the B.A. were rightfully concentrated in their hands. The way in which individual philosophers differed in their views of the role the B.A. could take in advancing science should not blind us to the fact that they shared the important concern of constructing a philosophical elite, and they saw the Association as the main arena in establishing themselves in that position.

8. The British Association and Government.

Differences of opinion, however, characterised the Gentlemen of Science on such important matters as the relationship of science with the Government. Whewell had written to Harcourt in 1831 that he did not wish to share in the activity of the B.A. if it was a body which explicitly attempted to influence the Government in the name of science.[187] At the other extreme, Brewster and Babbage at this same time wished to use the B.A. to strengthen the relationship which they saw as much needed between science and the State. Different views again were held by Murchison, who, like Whewell, did not agree with the doctrines

[186] W.J.M.Rankine, *A Manual of Applied Mechanics*, (London, 1858), p.4.

[187] Whewell to Harcourt, *op.cit.* (note 150).

concerning the decline of science. However Murchison felt that the fact that science had laboured "solely on individual and isolated efforts" had been a great impediment to its progress, and hence the efforts of a *body* of men interested and experienced in science were bound to have a considerable effect on its advance.[188] While Whewell and Murchison may have differed in the emphasis which they placed upon the value of individual efforts to scientific progress, they were in accord in their opinion of the necessary steps to be taken on behalf of science for the "removal of any disadvantages of a public kind which impede its progress". This demanded dealings with the Government.

Of successful B.A. lobbies of Government, the most relevant here is one launched in 1836 advocating the remission of duty on imported scientific instruments. The lobby was successful by the Spring of 1837. Murchison and Babbage, who had prepared a memorial, met the President of the Board of Trade, Charles Poulett Thomson, and the Chancellor of the Exchequer, Thomas Spring Rice.[189] The latter was so impressed with the memorial that he quickly asked the advice of Richard Betenson Dean, Chairman of the Board of Customs, on the matter. Dean had some doubts as to the details of the particular instruments on which the B.A. wished to see the duty repealed, but when a more explicit definition of what was meant by a "philosophical instrument" was given, there was little hesitation, in view of

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[188]Address by Murchison, *B.A.A.S. Report, Newcastle, 1838*, pp.xxxi-xliv, on p.xxxii.

[189]Morrell and Thackray, *op.cit.* (note 1), p.335.

the importance for the development of scientific ideas (and thus wealth) that such instruments afforded, in repealing the duty.[190]

A similar problem caused by Government legislation was alluded to at the Association's very first meeting at York in 1831 by Harcourt, and by the President, Milton. This concerned the duty paid to the Government on the price of glass. Milton lamented the high price of glass and the obstacle which this presented to the science of optics.[191] Harcourt was primarily concerned with the expense of glass which had to be used for display cabinets in the Yorkshire Museum:

> There is nothing more indispensable to the utility of such an Institution than a complete display of the specimens which it contains; and for that purpose, where the specimens are numerous, extensive glazing is required. Now there is a most serious impediment to this in the high price of glass, and of that price we find that two thirds consist in the duty paid to Government.[192]

So this tax was detrimental to the subject of natural history, although:

> ..the regulations of the Excise oppose an obstacle to the improvement of astronomical instruments still more to be regretted.[193]

Philosophers such as Whewell and Harcourt could be seen here to be advocating a kind of *laissez faire*, with regard to scientific progress. They saw removal of impediments as the best

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[190] *Ibid.*, and R.B.Dean to T.Spring Rice, 17 April 1837, British Library Add. Ms. 37190 ff.108-9.

[191] Address by Lord Milton, *B.A.A.S. Report, York, 1831*, pp.15-17, on p.16.

[192] Address by Harcourt, *B.A.A.S. Report, York, 1831*, pp.17-41, on p.32.

[193] *Ibid.*, pp.32-3.

stimulus to advancement, in science as in trade, and tended to discourage active intervention by the Government. All philosophers would have agreed with Whewell on the expediency of the removal of impediments to science (this was part of the objectives of the B.A.), but, on matters on which it was not so clear that men of science ought to appeal to the Government, differences of opinion persisted, a common standpoint being that such appeals lowered the dignity of a philosopher.[194] A glance at the Presidential Addresses of the first 20 years of the B.A. shows the disparate views maintained by holders of the highest office in the Association. Nowhere is this more apparent than in the years 1849-51; 1849, at Birmingham, with Thomas Romney Robinson in the chair, 1850, in Edinburgh, with David Brewster holding office, and 1851, in Ipswich, Astronomer Royal George Airy being President. Robinson drew attention to the dependence of astronomy on factors such as instrument making:

> Astronomy must not only track the unseen with Adams and Leverrier, or fathom the abysses of the sky with Herschel and Rosse; it must also visit the workshops of the machinist with Airy and Struve.[195]

However, he went on to show that the men of science who employed the instrument makers (and thus were of a higher status) were not well rewarded in terms of social recognition:

> Nowhere in the civilised world is less honour paid by a nation to science, though nowhere is national prosperity more connected with its progress, nowhere are heavier penalties paid for its neglect.[196]

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[194] Whewell to Harcourt, *op.cit.* (note 150).
[195] Address by the Rev.Thomas Romney Robinson, *B.A.A.S. Report, Birmingham, 1849,* pp.xxix-xlvi, on p.xxxiv.

[196] *Ibid.,* p.xl.

Although Robinson denied that the philosopher desired honours in themselves from the State, he pointed out that it was important for him to know that those in a position to *bestow* the honours held the philosopher precious.[197] He also divorced himself from the notion that the *only* use for science was if it directly produced wealth by its applications:

> Whatever tends to raise man above low and sensual pursuits - whatever to lead him from the partial and present to the general and the future - whatever to exalt in his mind the dimension of order and the supremacy of truth - that must be useful to the individual, useful to the nation... [however] there is not a single element of our commercial prosperity in which the vivifying power of science might not be felt...[198]

Brewster aligned himself with Robinson's position on the necessity of bestowing honours on men of science but was more forceful in his idea of the explicit relation that should exist between science and the State. As in his writings on the decline of science in the early 1830s, France was his model; there scientific institutions were fostered by the State, and Britain suffered through lack of this recognition:

> In a great nation like ours...it is a singular anomaly that the intellectual interests of the country should, in a great measure, be left to voluntary support and individual zeal - an anomaly that could have arisen only from the ignorance or supineness of ever changing administrations, and from the intelligence and liberality of a

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[197] *Ibid.*, p.xli. As we saw in the previous chapter, Michael Faraday's opinion of honours was similar to this: M.Faraday to Lord Wrottesley, 10 March 1854, in L.P.Williams (ed.), *The Selected Correspondence of Michael Faraday*, (2 volumes, London, 1971), pp.724-5.

[198] Robinson, *op.cit.* (note 195), p.xli.

> commercial people - an anomaly too that could have been continued only by the excellence of the institutions they had founded.[199]

Airy's address of 1851, meanwhile, directly contradicted Brewster's view of the role which the Government should take in stimulating scientific endeavour. He acknowledged that in some instances help had been given by Government, and that it was occasionally good that the administration should bestow personal rewards for discoveries of national importance. However, Airy expressed the hope that scientific initiatives would be left to individuals and independent associations,[200] and declared:

> ...the absence of Government science harmonises well with the peculiarities of our social institutions. In science, as well as in almost everything else, our national genius inclines us to prefer voluntary associations of private persons to organisations of any kind dependent on the State.[201]

The sentiments expressed by Robinson, Brewster, and Airy from the Presidential Chair were thus very different and preclude attempts by the historian to establish any general position of B.A. members on an important issue such as the relationship of science with Government. But as this chapter has shown, and as the Presidential Address by Sir Robert Harry Inglis at Oxford in 1847 implied, the members of the Association were linked by a belief in the value of science to the nation, and of the B.A. as a means of enhancing their own reputation:

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[199] Address by Sir David Brewster, *B.A.A.S. Report, Edinburgh, 1850,* pp.xxxi-xliv, on p.xlii.

[200] Address by George Biddell Airy, *B.A.A.S. Report, Ipswich, 1851,* pp.xxxix-liii, on p.li.

[201] *Ibid.*

> None can doubt that the reputation of our country depends far more on its intellectual strength than on its military glory. Without for a moment undervaluing those to whom in past ages as in the present England is - humanly - indebted not merely for her empire but for preservation also, I cannot doubt that the European reputation of England is owing far more to Newton than to Marlborough. I believe that every new discovery of science which England is permitted to make, while it adds perhaps directly to her wealth, or indirectly to the development of her resources, adds also to her influence in the scale of nations. Our government has exercised a prudent and sagacious liberality in adopting thus far suggestions of this Association for the advancement of science: and it may be well assured that such suggestions, made cautiously and disinterestedly by this Association, will continue to advance the public interests, as well as the mere incidental honour of the body from which they proceed, and which, from past experience, may justly claim the confidence of the State.[202]

9. Conclusion.

The British Association for the Advancement of Science was undoubtedly seen by the scientific community as having been successful in its aim of giving a "stronger impulse and more systematic direction" to scientific inquiry. Without even touching on the work which the Association initiated in, for example, geology and the life sciences, many researches in meteorology, terrestrial magnetism, and other subjects within the province of Section A would not have been possible on the scale on which they took place without the stimulus of a body such as the B.A.. For instance, in the area of terrestrial magnetism, the first occurrence of a survey being undertaken for the express purpose of determining the values and positions of the magnetic

[202] Address by Sir Robert Harry Inglis, *B.A.A.S. Report, Oxford, 1847,* pp.xxxix-xlvi, on p.xlv.

lines of declination and force corresponding to a given period over an entire country, was the magnetic survey of the British Isles. This was performed between 1834 and 1838 by a committee of members of the B.A., acting on a suggestion made at the Cambridge meeting of 1833.[203] In later years, as we have seen particularly in Section 5, the Association's work at Kew Observatory was acknowledged to be at the forefront of developments in meteorological and magnetic science and instrumentation on a global level.

Apart from the advantage which such work could be demonstrated to have presented for science, as important was the value which participation in such endeavours could be to a philosopher's career. The B.A. thus provided a vehicle for career ambitions and the quest for status which philosophers shared during the period. Lesser mortals were accorded respect by the philosophers who controlled the B.A., but such respect was not to be confused with the usurpation of the natural order by those of lower station such as artisans. As Sedgwick declared:

> Do not suppose for a moment that I am holding any levelling doctrines. Far from it. I seek but to consolidate the best interests of society...[204]

However, he was aware of the social importance of paying respect to the artisans and engineers who were of importance to the generation of the nation's wealth:

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[203] J.Cawood, "The Magnetic Crusade: Science and politics in early Victorian Britain", *Isis,* 1979 (70), pp.493-518.

[204] J.W.Clark and T.M.Hughes, *The Life and Letters of the Reverend Adam Sedgwick,* (2 volumes, Cambridge, 1890), Volume 2, p.47.

> ...but I do wish that the barriers between man and man, between rank and rank, should not be harsh, and high, and thorny; but rather, that they should be a kind of sunk fence, sufficient to draw lines of demarcation between one and another.[205]

Such a statement was proper in its granting of credit to classes such as engineers and instrument makers, who were enabled to use the B.A. for the furtherance of their own careers, but it left no doubt that the positions of power and authority in the B.A., and elsewhere, belonged to the philosopher.

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[205] *Ibid.* This quotation, cited in Morrell and Thackray, *op.cit.* (note 1), p.32, is taken from a lecture Sedgwick delivered to a large crowd of locals assembled just outside Newcastle during the week of the meeting there in 1838, and it should be realised that the fact that he was not addressing an audience of philosophers may have had a bearing on the content of the speech.

CHAPTER EIGHT

THE INSTRUMENT MAKER AND INDIVIDUAL INITIATIVE: AVENUES OF ADVANCEMENT FOR THE ARTISAN

The analysis in the previous chapters has been concerned primarily with the philosophers' tactics in articulating the idea of their importance to society, and with the consequences of this for the perceived standing of the instrument maker in society, though especially within the scientific community. However, particularly in Chapter 7, on the British Association for the Advancement of Science, it was shown that the instrument maker could find various sources of career advantage in the institutions of the scientific world. Most obviously, its institutions and individuals paid money to the makers to have instruments built, but by participating in the meetings of the British Association, for example, it has been shown that the successful maker could advance and promote himself and his business as well. This chapter will substantially be concerned with this idea of the instrument maker's self-promotion as a market-related motive. His interests in the B.A.A.S., however, as in all the other institutions discussed, remained very much subordinate to those philosophers who wielded power in them, whether it was Airy at the Royal Observatory, Wheatstone at King's College, Faraday at the Royal Institution, or the President and Council in the Royal Society.

In this chapter will be considered the other avenues by which the ambitious maker could attempt to gain standing in the scientific community, and to participate in scientific life. Three main areas will be discussed here. Firstly, the makers' involvement in scientific societies other than the Royal Society and the British Association will be analysed. This discussion will mainly be concerned with the specialist societies, many of which had their origins in the early part of the nineteenth century, though the makers' part in those societies whose science depended heavily on precision instruments, such as the Astronomical Society, was, as might be expected, more prominent than that in, for example, the Linnean Society or Geological Society, which owed little if anything to precision instruments and thus fall outside the scope of this thesis.[1] Secondly, the makers' use of publication media such as independent journals to give notice of their work will be discussed. This point overlaps to some extent with the first in that the instrument makers who took part in the affairs of the specialist societies were thereby enabled to have more ready access to publish in the journals of those societies. Finally, the relatively small number of cases of publication of scientific and technical books by instrument makers in the period will be considered.

The discussion of these three areas of involvement will enable a full picture of the instrument makers' tactics in marketing himself in circles of clients to be built up, extending the analysis in the earlier chapters. This account will then be extended by a further analysis of the trade itself, especially of the new business pressures which the nineteenth-century maker had to face, and in this way I will show the interdependence of those factors which, at the start of this thesis, were suggested as causing the decline in status of the

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[1] The Linnean Society was formed in 1788, and the Geological Society in 1807. The Astronomical Society was founded in 1820.

instrument maker in the scientific community, and caused him to be seen as a craftsman, rather than as a partner in contributing to the increase of scientific knowledge.

1. The Instrument Maker as a Member of Metropolitan Societies: The Astronomical Society.

The Royal Society and the British Association for the Advancement of Science, it has been argued, provided an opportunity for the ambitious instrument maker to make known his name in scientific circles. In Chapter 2 of this thesis it was shown that Fellowship of the Royal Society could be a significant career advantage to a maker. However, it was also shown that he was denied any positions of *power* within the Society, and indeed the number of makers who became Fellows in the nineteenth century was very small. Thus, apart from these few, and the small number of others who were commissioned to provide instruments for Royal Society projects, its usefulness to makers was very limited. The British Association, on the other hand, kept its membership policy open, so that instrument makers were in principle as free as anyone else to attend meetings. Many of the prominent London makers attended on a regular basis, as I have shown, and many provincial makers were present when the Association reached their home area. But here also positions of power were the preserve of the philosopher, and the scientific instrument maker had little to say in an institution devoted to the whole of science.

The emergence of the specialist societies in the early part of the century had provided a different set of circumstances to the Royal Society for enthusiasts in different subjects.

Those interested in Natural History, with the Linnean Society, or in Geology, now had more specific, indeed sympathetic, arenas in which to put forward their work. The idea of a society devoted to astronomy therefore had clear antecedents. A proposal that such an organisation be formed was put forward initially by William Pearson in 1812, and appealed to Patrick Kelly, a teacher of mathematics, and to Edward Troughton.[2] Pearson was a devoted astronomer who had two observatories built for his own use, in which some Troughton instruments were contained.[3] Troughton's support of Pearson's idea was significant; it showed not only his interest in astronomy (after all, his source of livelihood), but also a desire to participate in the affairs of a society devoted to his favourite science, involvement which was being denied to him in the Royal Society under Banks, owing to his artisan status.

The Astronomical Society did not come into being until 1820, but Troughton and Pearson were among its original members, and indeed Troughton later served for a period as Vice-President.[4] The interest in instrumentation used in astronomy was apparent from very early in the life of the Society. In December 1820 Troughton began a lengthy paper on the repeating circle and

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[2] G.J.Whitrow, "Some Prominent Personalities and Events in the Early History of the Royal Astronomical Society", *Quarterly Journal of the Royal Astronomical Society,* 1970 (11), pp.89-104, on p.101.

[3] *Ibid.,* pp.101-2.

[4] Troughton served as Vice-President in 1830-1. See A.W.Skempton and J.Brown, "John and Edward Troughton, Mathematical Instrument Makers", *Notes and Records of the Royal Society of London,* 1973 (27), pp.233-62, on pp.247-8.

altazimuth instrument,[5] continued with it in January 1821, and also for the whole of the March meeting. George Dollond discoursed on the repeating circle as well,[6] in April 1821, so that four meetings in close succession had been devoted to instrumentation, the one intervening meeting having been the annual one, at which no scientific papers were read.[7]

The opportunity to read and publish papers was clearly one to be welcomed by those astronomical instrument makers who took part in the Society's meetings upon becoming members. However, the lengthy paper on the repeating circle and altazimuth was Troughton's only published contribution, and Dollond too published very little thereafter.[8] Of the other work by makers which occupied the Society, only William Simms seems to have been at all prolific, in the period after Troughton's death in 1835 (the Society had become the Royal Astronomical Society in 1831). In his case the issue is somewhat confused by the fact that his son and nephew, both called William Simms, also participated in the life of the Society. The son, William Henry, who lived for a

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[5] E.Troughton, "An Account of the Repeating Circle, and of the Altitude and Azimuth Instrument", *Memoirs of the Astronomical Society,* 1822 (1), pp.33-54. This was also published in *Philosophical Magazine,* 1822, pp.8-18, 102-13. Subsequent page references are to this latter version.

[6] G.Dollond, "The Description of a Repeating Instrument upon a new construction", *Memoirs of the Astronomical Society,* 1822 (1), pp.55-8.

[7] H.H.Turner, "1820-30", in J.L.E.Dreyer and H.H.Turner (eds.), *History of the Royal Astronomical Society 1820-1920,* (London, 1923), pp.1-49, on p.13.

[8] The only other paper Dollond published in the Society's journal was G.Dollond, "A short account of a new Instrument for measuring Vertical and Horizontal Angles", *Memoirs of the Astronomical Society,* 1826 (2), pp.125-8.

time in Ceylon, had been educated at Cambridge and his contributions, as a result, were similar in mathematical sophistication to those of other astronomers educated there in the Mathematical Tripos.[9] William Henry took no part in the instrument maker's business of his father. William junior, the nephew, worked under his uncle along with another of Simms' sons, James,[10] and devoted most of his time to that business, only contributing one paper to the Royal Astronomical Society,[11] having become a Fellow in 1852 upon the recommendation of Richard Sheepshanks, long time friend of Troughton and Simms.[12] The *Royal Society Catalogue of Scientific Papers* conflates these three individuals under "Simms, W.", so that under closer inspection Simms only contributed about half as many papers as would at first glance appear to have been the case.

The proportion of contributions to the Society by instrument makers, compared to that by philosophers with astronomical interests, was thus relatively small. It was certainly higher than the corresponding proportion of papers contributed to the

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[9] Most notable in this respect are W.H.Simms, "Formulae for deducing the Latitude of an Observatory, from Observations of Stars with a Transit-Instrument placed in the Prime Vertical", *Memoirs of the Royal Astronomical Society,* 1858 (26), pp.1-8, and W.H.Simms, "On the Corrections to be applied to Observations made with the Sextant", *Memoirs of the Royal Astronomical Society,* 1858 (26), pp.19-44.

[10] For biographical details, see E.J.Mennim, *Reid's Heirs. A Biography of James Simms Wilson (1893-1976), Optical Instrument Maker,* (Braunton, 1990), pp.23ff.

[11] W.Simms junior, "On a mounting for a large reflecting telescope", *Monthly Notices of the Royal Astronomical Society,* 1852-3 (13), p.187.

[12] Mennim, *op.cit.* (note 10), p.24.

Royal Society, but that was only to be expected in a Society devoted to a science of which its instrumentation was such an integral part. The nature of the makers' written work also differed from that of most of the philosophers involved in the Society. Troughton's paper, while lengthy, contained no notifications of great new designs. Rather, it represented an attempt to compare the respective constructions and merits of two instruments, of which he preferred the altitude and azimuth instrument, which had hitherto mainly been made in Britain, the repeating circle being very much a continental instrument.[13] The paper therefore can be seen as a description given for the benefit of those amateur adherents of the science of astronomy who were desirous of an opinion on the relative merits of two similar instruments:

> Of all astronomical instruments, those fixed in national observatories must be considered of the first importance to science; and in a commercial country, like our own, perhaps those subservient to nautical astronomy ought to be regarded as the next point in of utility. Those which I would call the third class are numerous; they are such as are used in the small observatories of the amateur, to which they are in general equally adapted, as to the service of the gentleman who may travel to foreign parts. Of those, the two I have named in the title, are the most approved of for these purposes...[14]

In starting the paper in this way Troughton could be seen to be emphasising his authority on matters connected with instrument making, in particular by stressing the paramount importance to science of the instruments fixed in national observatories which were his speciality. By writing a paper on smaller instruments

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[13] Troughton, *op.cit.* (note 5), p.9.
[14] *Ibid.,* pp.8-9.

suitable for employment by the amateur, however, he emphasised authority to a larger potential market.

It would be wrong to think that such commercial aspirations were Troughton's only concern, but financial considerations did form a substantial part of his motivations, as with other makers who published work which was intended for a readership of potential clients. George Dollond, in his two short papers published in the *Memoirs*,[15] was keen to emphasise the novelties which he had introduced in the constructions of the instruments he described, claiming in the case of his repeating instrument that: "The whole of the instrument is differently framed from any that has previously been made...".[16] However, we have seen in previous chapters that the subjects of such claims were viewed by philosophers as little more than refinements in practical design, and not as great innovations in theoretical principle.

In other words, the Astronomical Society represented a forum which was very similar to the Royal Society with respect to the part played by instrument makers, in that they were enabled to give the benefit of their practical expertise to an interested audience, without making the contributions to research that would enable them to reach positions of influence in such an institution. It is significant, then, that the only makers who devoted a substantial amount of time to the Astronomical Society's affairs, in the early years of its existence, were also Fellows of the Royal Society - Troughton and Dollond. Later, Simms contributed his papers to the Astronomical rather than the

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[15] Dollond, *op.cit.* (note 6), and Dollond, *op.cit.* (note 8).
[16] Dollond, *op.cit.* (note 6), p.58.

Royal Society,[17] but these too were infrequent, and with one significant exception which will be discussed later, concerning his self-acting dividing engine,[18] relatively insubstantial. Many more makers became Fellows of the Royal Astronomical Society than became Fellows of the Royal Society, and it could be a great career advantage to a maker such as Louis Casella,[19] John Benjamin Dancer,[20] or Troughton's former workman Andrew Yeates[21] to be able to call themselves F.R.A.S. on their trade cards. Members of the famous dynasties of Troughton and Simms, and Dollond also could be seen as almost automatic Fellowship material. James Simms and William Simms junior, son and nephew respectively of William Simms (and his successors in ownership of Troughton and Simms on his death in 1860), were both Fellows, as was George Dollond's nephew (also called George Dollond).

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[17] Simms' longer papers included W.Simms, "On the Optical Glass prepared by the late Dr.Ritchie", *Memoirs of the Royal Astronomical Society,* 1840 (11), pp.165-70, and W.Simms, "On a New Arrangement of a Vertical Collimator to the Altitude and Azimuth Instrument", *Memoirs of the Royal Astronomical Society,* 1846 (15), pp.19-22.

[18] W.Simms, "On a Self-acting Circular Dividing Engine", *Memoirs of the Royal Astronomical Society,* 1846 (15), pp.83-90.

[19] Casella did make one published contribution to the Society: L.P.Casella, "On a Micrometric Diaphragm", *Monthly Notices of the Royal Astronomical Society,* 1861 (21), pp.178-80.

[20] Dancer was elected in 1855. See M.Hallett, "John Benjamin Dancer 1812-1887: A perspective", *History of Photography,* 1986 (10), pp.237-56.

[21] For details on Yeates' career, see A.D.Morrison-Low, "The Trade in Scientific Instruments in Dublin, 1830-1921", in J.E.Burnett and A.D.Morrison-Low, *Vulgar and Mechanick. The Scientific Instrument Trade in Ireland 1650-1921,* (Dublin, 1989), pp.39-70, on p.42. This also refers to Yeates' obituary in *Monthly Notices of the Royal Astronomical Society,* 1877 (37), pp.159-60.

While these men did attend meetings, they published very little, so that the Astronomical Society's main advantage to most of those instrument makers who became Fellows was as an arena in which they could move socially with potential clients among those philosophers who contributed research papers and who held positions of power within the Society. In this respect the analogy with the Royal Society was particularly striking, for many of those in power in the Astronomical Society throughout this period regularly appeared on the Council of the Royal Society,[22] and although Troughton was appointed Vice-President, no maker was ever to become President, Treasurer, or Secretary of the Astronomical Society.

In the next section will be considered another of the specialist societies relevant to the development of instrumentation, in which the access to positions of power for the maker might have been expected to be more readily available than was the case with the Royal Society or the British Association. This institution is also a particularly significant choice as it was the first scientific society to be devoted exclusively to an *instrument* - the Microscopical Society of London, later to become the Royal Microscopical Society. In this section the emphasis will, as with the Royal Astronomical Society, be on the maker's motives of self-promotion through activity in scientific institutions.

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[22] Francis Baily, for example, who served as President of the Astronomical Society on four separate occassions, was elected to the Council of the Royal Society 15 times. See Whitrow, *op.cit.* (note 2) for further details of individuals who held office in both institutions.

2. The Microscopical Society of London.

The Microscopical Society had its origins in a meeting of seventeen gentlemen at the house of E.J.Quekett on 3 September 1839. The purpose of this assembly of the devotees of microscopy was:

> To take into consideration the propriety of forming a Society for the promotion of microscopical investigation, and for the introduction and improvement of the microscope as a scientific instrument.[23]

By 20 December 1839, it was finally resolved that such a Society should indeed be formed.[24] The idea of the improvement of the microscope itself was therefore foremost in the thoughts of the original proposers of the Society, and thus, by implication, those who made such improvements could expect to play a paramount role. However, the Bye-Laws adopted by the Society give a more accurate impression of the class of individuals for which the Society mainly was to exist:

> For some years past several of the metropolitan microscopical observers have been in the habit of occasionally meeting in each others' houses for the purpose of comparing the powers and other merits of different microscopes - of testing the merits of each others' observations of minute objects and structures - and of submitting doubtful and obscure microscopical phenomena to instruments of different constructions. But while the benefit and pleasure arising out of these casual associations were acknowledged by all who participated in them, the inconvenience of having

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[23] Excerpt from the history, constitution and laws of the Microscopical Society of London, cited in B.Bracegirdle, "Famous Microscopists: Joseph Jackson Lister, 1786-1869", *Proceedings of the Royal Microscopical Society,* 1987 (22), pp.273-97, on p.281.

[24] *Ibid.* See also G.L'E. Turner, "The origins of the Royal Microscopical Society", *Journal of Microscopy,* 1989 (155), pp.235-48.

> no fixed or central place of meeting, and the inadequacy of most private residences to accommodate the increasing numbers of the lovers of the microscope, desirous of joining such an association, began to be severely felt; and thus the design of instituting a Society for the advancement of the science of the microscope originated as the legitimate consequence of the exigencies of the scientific investigator...[25]

It was thus the gentleman interested in the microscopical investigation itself who seemed to be the *raison d'etre* of the new society. By the end of the first year of its existence there were 177 members of whom over 20 were Fellows of the Royal Society,[26] so it can be seen to have possessed an interest even for those perceived as being part of the higher echelons of the scientific community. At this stage in its development, however, the microscope was not in use as a professional or research tool in medicine, so that those who purchased microscopes were contiguous with those who were interested in using them on an amateur basis, and who attended the meetings of the Microscopical Society. In other words, though the Society existed for the gentlemanly philosopher interested in microscopical investigation, it also existed indirectly for the microscope maker who wished to display his products and his competence to a captive audience.

The three leading British microscope makers, Andrew Ross,

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[25] Excerpt from the bye-laws of the Microscopical Society of London, cited in G.L'E.Turner, *The Great Age of the Microscope. The Collection of the Royal Microscopical Society through 150 years,* (Bristol, 1989), pp.3-4.

[26] *Ibid.,* p.4.

James Smith, and Hugh Powell,[27] took a prominent role in the Society virtually from its beginnings, acutely aware of the considerable opportunity for career development with which it presented them. Powell (1799-1883) apparently made microscopes for the philosophical instrument trade for some time before he set up under his own name, beginning to sign his microscopes "Hugh Powell" in 1840.[28] He was elected a Member of the Microscopical Society on 29 January 1840, effectively meaning that he was a founder member. He took his brother-in-law, Peter Henry Lealand, as partner in 1842, and Lealand was elected to the Microscopical Society that year.[29] Andrew Ross (1798-1859) had made objectives to the 1830 design of Joseph Jackson Lister, and a partnership between the two men signified by "+ Co." after Ross' name existed from 1837 to 1841.[30] Ross, like Powell, was a founder member of the Microscopical Society. James Smith had also done work for Lister, was set up in business with Lister's help, and took Lister's nephews Richard and Joseph Beck into partnership, Richard (having served him as an apprentice) in 1847, and Joseph (having been apprenticed to Troughton and Simms) in 1851.[31] James Smith was elected a Member of the Microscopical Society on 19 February 1840, three weeks after the foundation

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[27] G.L'E.Turner, "Hugh Powell, James Smith, and Andrew Ross: Makers of Microscopes", in J.D.North (ed.), *Mid-Nineteenth Century Scientists,* (Oxford, 1969), pp.104-38. Details on these makers are also provided in Turner, *op.cit.* (note 25), pp.114-15, 154, 171, and in R.H.Nuttall, "The Achromatic Microscope in the History of Nineteenth Century Science", *Philosophical Journal,* 1974 (11), pp.71-88.

[28] Turner, *op.cit.* (note 25), p.114.
[29] *Ibid.*
[30] *Ibid.,* p.154.
[31] *Ibid.,* p.171.

elections.[32]

Bennett shows that the activities of the new Society encouraged makers who could take a prominent part in it, such as the above, to produce lavish microscopes with large apertures, high resolutions and high magnifications, which were effectively aimed at resolving ever more exacting test plates.[33] The makers were enabled to gain prestige by construction of microscopes which were not aimed at general useful applications but at the interests of the gentlemanly philosophers wishing to perform "virtuoso feats of resolution".[34]

The microscope, then, came to be seen by these men very much as a scientific instrument rather than as a potential professional tool. They felt no need to justify their enterprise in terms of political economy, and so they represent a different aspect of the scientific community to those I have discussed elsewhere in this thesis. However, like the rest of the scientific world they provided the *market* for the artisan, and even though Bennett shows that the practical section of the market grew with the expansion of science education and medicine later in the century, with stress being placed on cheapness and practical utility in a microscope,[35] it is undeniable that it was the forum provided by the gentlemanly philosophers in the Microscopical Society which encouraged the leading British makers to undertake those developments which would contribute to their

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[32] *Ibid.*
[33] J.A.Bennett, "The Social History of the Microscope", ***Journal** of Microscopy,* 1989 (155), pp.267-80, on p.276.

[34] *Ibid.*
[35] *Ibid.,* p.277.

being seen as the best in the world.

It seems, however, that the emphasis for the maker was very much on the practical display of his products to the enthusiasts. Few written contributions were made to the journals of the Society by microscope makers, though in fact the publication history of those journals associated with the Society was rather erratic. *The Microscopic Journal and Structural Record* was first compiled by Daniel Cooper in 1841, and was a 200-page collection of technical and general articles, together with reports of the transactions of the Microscopical Society.[36] On Cooper's death in 1842 the second volume was completed by George Busk.[37] Then the journal was discontinued.

The first volume had contained a short paper by Ross on how to prevent tremor in microscopes,[38] and also some references to a new instrument by Powell and Lealand,[39] and the second volume had a description of a new microscope by James Smith,[40] but other than these, references to the men who actually made the instruments for the improvement of which the Society was supposed to exist were rare. As Cooper's had been a commercial venture,

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[36] W.H.Brock, "Patronage and publishing: journals of microscopy 1839-1989", *Journal of Microscopy,* 1989 (155), pp.249-66, on p.249.

[37] *Ibid.,* pp.249-50.

[38] A.Ross, "On the Means of Preventing Tremor in Microscopes", *Microscopic Journal and Structural Record,* 1841 (1), pp.23-4.

[39] [D.Cooper], "Powell and Lealand's description of a newly constructed microscope", *Microscopic Journal and Structural Record,* 1841 (1), pp.177-81.

[40] [D.Cooper], "Description of Mr.James Smith's Newly Constructed Achromatic Microscope", *Microscopic Journal and Structural Record,* 1842 (2), pp.1-6.

publications concerned with the Microscopical Society ceased after the appearance of his 1842 volume, and it was September 1844 before the Council decided that it was desirable to publish the *Transactions of the Microscopical Society.*[41] From 1853 this became incorporated into a *Quarterly Journal of Microscopical Science.*[42]

The situation in all these microscopy periodicals with respect to the publication of papers by instrument makers was exactly analogous to that in the journals of other societies such as the Royal Society and the Astronomical Society: the maker was not forbidden from publishing, but the notion of what constituted *real* research, in the opinion of those in power, excluded the technical refinements noted by instrument makers. With the microscope, it was Lister's work on objective lens systems which was seen as at the base of all modern microscope design, and not the latest design of a stand, or a means of preventing tremor, that a maker might have developed.

Before Lister's work, the problem of correcting the spherical aberration in achromatic double and triple lens systems had been solved in a trial and error way,[43] but his paper provided a mathematical basis for combining lenses so as to

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[41] Brock, *op.cit.* (note 36), p.250; Turner, *op.cit.* (note 24), pp.244-5.

[42] Brock, *op.cit.* (note 36), p.250.

[43] Turner, *op.cit.* (note 24), p.237. See also Bracegirdle, *op.cit.* (note 23).

eliminate spherical and chromatic aberration in the microscope.[44] The first effective achromatic object glass in England had been made in 1824 by William Tulley, working to the empirical suggestions of C.R.Goring,[45] but the development which such a trial and error process for removing the spherical aberration allowed was argued to be limited, and the general consensus among microscopists became that Lister's work gave the future course of microscope design a scientific basis.[46] Ross was instructed by Lister himself as to how to execute the designs of successful lens systems,[47] and had a full appreciation of the importance of the new developments, though his rhetoric in pressing the central importance of the microscope to society was as one might expect from someone who derived his income from making the instruments:

> The last fifteen years have sufficed to elevate the compound microscope from the condition we have described to that of being the most important instrument ever bestowed by art upon the investigator of nature. It now holds a very high rank among philosophical implements, while the transcendent beauties of form, colour and organisation which it reveals to us in the minute works of nature, render it subservient to the most delightful and instructive pursuits. To these claims on our attention it appears likely to add a third of still higher importance. The microscopic examination of the blood and other human organic

[44] *Ibid.* The paper referred to is J.J.Lister, "On some properties in achromatic object glasses applicable to the improvement of the microscope", *Philosophical Transactions of the Royal Society of London,* 1830, pp.187-200.

[45] Nuttall, *op.cit.* (note 27), pp.80-2; A.Hughes, "Studies in the History of Microscopy. The Influence of Achromatism", *Journal of the Royal Microscopical Society,* 1955 (75), pp.1-23, esp.pp.6-8.

[46] Nuttall, *op.cit.* (note 27), pp.76-83; S.Bradbury, *The Evolution of the Microscope,* (Oxford, 1967), p.191.

[47] Turner, *op.cit.* (note 27), pp.131-5.

> matter will in all probability afford more and conclusive evidence regarding the nature and seat of disease than any hitherto applied...[48]

Of the other top London makers, James Smith was aided by Lister in the setting up of his business, and used the latter's theories in the design of his lens systems.[49] It seems, however, that Hugh Powell was seen by some as having continued to employ the sort of empirical methods which had enabled Tulley to design the successful achromatic lens system for Goring in 1824. According to Mayall:

> The English optician who did more than all the others together to improve objectives was the late Hugh Powell, and he repeatedly affirmed to me that every advance he himself had made... had been arrived at by sheer experiment, without a single hint of any value from any theorist whatsoever. It is also certain that Oberhauser, Hartnack, Nachet senior, knew nothing of Lister's investigations, and yet as late as 1863, objectives made by them were only slightly inferior to Powell's, and were quite on a par with objectives of any other English maker. Whence I infer that Lister's influence has been much exaggerated...[50]

If indeed Powell did not use Lister's theories, then it shows that their supposedly all-pervasive influence may have been a rhetorical device of those who wished to stress the importance of theoretical principles and those who developed them. Such rhetoric ensured that a mere empiric like Powell would not gain scientific status, but did not mean that he was unable to develop

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[48] A.Ross, "The Microscope", *Penny Cyclopaedia of the Society for the Diffusion of Useful Knowledge,* 1839 (15), pp.177-88, on p.182.

[49] Turner, *op.cit.* (note 27), p.171; Bracegirdle, *op.cit.* (note 23), p.282.

[50] J.Mayall, *Cantor Lectures on the Microscope,* (London, 1886), pp.94-5.

the microscope by dint of his experience and manual skill.

One of the other English makers in this period was notable for his interest in scientific affairs, as we have seen in the previous chapter - Andrew Pritchard. Pritchard had first come to the notice of the scientific establishment when he was commissioned by Goring in 1824 to make a diamond lens,[51] on the basis of an earlier hint by David Brewster, that improvements in lenses might result from use of a substance which combined a high refractive power with a low power of dispersion.[52] Jewel lenses were effectively eliminated by Lister's discoveries, as well as by the difficulties involved in removing flaws in such substances as diamond and ruby,[53] but Pritchard's work in this area as well as his association with Goring, which resulted in a number of books popularising microscopy,[54] spread his name in scientific circles. Like Powell, Ross, and Smith, however, his interest in these areas was directed mainly towards his business, which he established at the end of his apprenticeship to his uncle Cornelius Varley in the mid-1820s.[55]

Pritchard made no secret of the fact that he employed other trade opticians to make microscopes for him, as well as making

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[51] G.L'E.Turner, "The Rise and Fall of the Jewel Microscope 1824-1837", *Microscopy: Journal of the Quekett Microscopical Club,* 1968 (31), pp.85-94, on pp.85-6; R.H.Nuttall, "Andrew Pritchard, Optician and Microscope Maker", *The Microscope,* 1977 (25), pp.65-81, on pp.68-9.

[52] Turner, *op.cit.* (note 51), p.85.

[53] *Ibid.,* p.89.

[54] For example, C.R.Goring and A.Pritchard, *Microscopic Illustrations,* (London, 1830); C.R.Goring and A.Pritchard, *Micrographia,* (London, 1837).

[55] Nuttall, *op.cit.* (note 51), pp.67-8.

his own,[56] and indeed Powell, Ross and Smith all made microscopes to be sold by Pritchard. This presumably provided Pritchard with some time to write his books on microscopy, thus promoting his business, and also to pursue his other career of patent agent.[57] Nuttall argues that as Pritchard's business developed, there became a tendency to employ his own workmen, rather than trade opticians,[58] to provide him with microscopes, though he was unable to compete with the three leading makers when the Microscopical Society ordered microscopes for use at its meetings, and he received little praise for his display at the Great Exhibition of 1851.[59] It seems that he had retired from microscope making by the mid-1850s, probably having made sufficient money by his various enterprises to live in comfort.[60]

While Pritchard may have had aspirations to have been considered a philosopher, the argument here maintains that his primary motivations were geared towards his business, and as with the other makers who diversified less into areas other than instrument making - Powell, Ross and Smith - the Microscopical Society was useful in career terms, but its positions of power were out of reach. The makers entered into the life of the Society as contributing partners in its work, though not as central figures of power. Indeed it is significant that very few

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[56] *Ibid.*, p.70.

[57] In this capacity he published lists of patents: A.Pritchard, *A List of all the Patents for Inventions*, (London, 1841), dealt with all patents issued from 1800 onwards, and there were at least three further volumes produced by Pritchard, dealing with the years 1841, 1842, and 1843.

[58] Nuttall, *op.cit.* (note 51), pp.74-5.

[59] *Ibid.*, pp.76-7.

[60] *Ibid.*, p.77.

of the research papers in this period by microscopical investigators contain even the name of the maker who provided them with the tools of their investigation.

Like the other societies which have been discussed, then, the Microscopical Society, even though it was unique in being devoted to an instrument, provided the instrument maker only with a forum to display his competence and promote his business, not with a forum in which he could increase his scientific status. The philosophical community had ensured, by making it a matter of scientific ideology, that one had to make improvements in *principle* (as Lister had), in order to gain status, so that even if an artisan made innovations, his mode of doing so remained empirical, rather than truly *scientific.* Thus, as the makers were unable to make the type of improvements required, they remained very much *artisans,* not *philosophers* like Lister.

3. The Scientific Community in Scotland and Ireland.

Although the higher echelons of the metropolitan scientific community were closed to the maker, he could play a more significant part in the relatively more open strata of provincial scientific communities in this period. It has been shown in Chapter 7 that the British Association meetings provided a valuable opportunity for provincial instrument makers to demonstrate their skill and knowledge to a local and national audience of potential clients, when the Association reached their area. Recent work has shown that in the cases of these makers, contributions made to B.A.A.S. meetings were not the only ones which they were able to make to scientific life in its local context. Indeed Clarke, Morrison-Low and Simpson (on Scotland),[61] and Burnett and Morrison-Low (on Ireland)[62] show that the elite makers in these regions had substantial parts to play in their local scientific community. The apparent tension between these accounts and my claim, that the instrument makers in London were denied such a role, can be resolved if it is realised that the sense of class identity and belief in importance to the State amongst the philosophers was much more fully developed at this time in England than it was in the provinces. Whereas the English man of science, as I have shown, felt a pressure to assert the primacy of his own enterprise, and to emphasise his importance to the State above other groups, and formed institutions to perpetuate this ideology, in Scotland and Ireland the scientific community and its institutions had not developed this elitism so fully, and therefore they were more open to participants in the scientific enterprise such as instrument makers.

In Scotland, the most prominent of the instrument making firms was that of Alexander Adie (1775-1858) and his sons, of whom John (1805-57) was most closely involved with the business, based in Edinburgh.[63] Alexander Adie's sympiesometer, or "new air barometer", varied from the standard mercurial barometer in that oil replaced mercury as the hydrostatic fluid, and a column of gas was used. Adie pointed out the advantages of the instrument over other barometers used for marine purposes, in several papers

[61] T.N.Clarke, A.D.Morrison-Low and A.D.C.Simpson, *Brass and Glass. Scientific Instrument Making Workshops in Scotland as illustrated by instruments from the Arthur Frank Collection at the Royal Museum of Scotland,* (Edinburgh, 1989).

[62] Burnett and Morrison-Low, *op.cit.* (note 21).
[63] Clarke, Morrison-Low and Simpson, *op.cit.* (note 61), pp.25ff.

submitted to local journals,[64] quoting as authorities naval officers who had used the instrument (in this case a Lieutenant Robertson):

> The Sympiesometer is a most excellent instrument, and shews the weather far better than the Marine Barometer. In short, the barometer is of no use compared to it. If it has any fault, it is that of being too sensible of small changes, which might frighten a reef in when there was no occasion for it; but, take it altogether, in my opinion it surpasses the mercurial barometer as much as the barometer is superior to having none at all.[65]

References such as this must have increased his sales potential, and indeed Adie authorised Thomas Jones to make sympiesometers in London, so there was a demand there as well.[66] In the month following his patent for the sympiesometer (and also a new type of hygrometer[67]) granted on 23 December 1818,[68] Alexander Adie was elected to the Royal Society of Edinburgh, the first instrument maker so honoured, one of his three sponsors for the election being the Society's secretary, David Brewster, for whom Adie had constructed optical instruments.[69] In 1821 he became involved in the formation by Brewster of the Society for the Encouragement of the Useful Arts in Scotland, by sitting on the first meeting of the Society's

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[64] A.Adie, "Description of two New Philosophical Instruments", *Memoirs of the Wernerian Natural History Society, Edinburgh,* 1817-20 (3), pp.483-94; A.Adie, "Description of the Patent Sympiesometer or New Air Barometer", *Edinburgh Philosophical Journal,* 1819 (1), pp.54-60.

[65] *Ibid.,* p.59.

[66] Clarke, Morrison-Low and Simpson, *op.cit.* (note 61), pp.35-7.

[67] A.Adie, "Description of a New Hygrometer", *Edinburgh Philosophical Journal,* 1819 (1), pp.32-3.

[68] Clarke, Morrison-Low and Simpson, *op.cit.* (note 61), p.37.

[69] *Ibid.*

Council to consider how to assess technical communications.[70] Throughout the 1820s he was an active member of the Royal Society of Edinburgh, The Society of Arts, and also the Wernerian Natural History Society, and Clarke, Morrison-Low and Simpson show that his time outside of instrument making was also spent in keeping abreast of new developments in physical and chemical science.[71]

The business traded as Adie and Son from 1835,[72] the son being John, who also became a Fellow of the Royal Society of Edinburgh, and who published several articles in local journals, notably on meteorological instruments[73] and on telescopes.[74] More prolific than either his father or his brother John, though less involved in the business until he took control of it in the late 1850s, was Richard Adie,[75] who published some 27 papers between 1837 and 1868, mostly on meteorology and its associated instrumentation.[76] This type of activity in scientific circles, albeit not in positions of power, was very unusual for a nineteenth-century maker, and such an output of research papers

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[70] *Ibid.*
[71] *Ibid.*, p.40.
[72] *Ibid.*, p.41.
[73] For example, J.Adie, "Comparative experiments on different Dew-point Instruments", *Edinburgh Journal of Science,* 1829 (1), pp.60-5; J.Adie, "Account of a new Cistern for Barometers", *Edinburgh Journal of Science,* 1829 (1), pp.338-40.

[74] For example, J.Adie, "Description of the Marine Telescope", *Edinburgh New Philosophical Journal,* 1850 (49), pp.117-22.

[75] Clarke, Morrison-Low and Simpson, *op.cit.* (note 61), p.50.
[76] For example, R.Adie, "Description of a new Anemometer", *Edinburgh New Philosophical Journal,* 1837 (22), pp.309-13; R.Adie, "On ground ice found in the beds of running streams", *Philosophical Magazine,* 1853, pp.340-5; R.Adie, "On the Generation of Electrical Currents, *Edinburgh New Philosophical Journal,* 1854 (57), pp.84-8.

by an artisan was unheard of in England. Another brother, Patrick Adie, moved to London and set up business[77] but the more fully developed structure of the scientific community there confined his activity to making instruments for supply to men of science (notably to the British Association), and he did not take the part in scientific life that his Edinburgh relatives had.

In Ireland, the equivalent of the Adie business was that of the Grubbs, Thomas and later his son Howard, whose main area of expertise and business was in large astronomical instruments.[78] Thomas (1800-78) started in life as a precision mechanic around 1830, and designed machinery for engraving, printing and numbering banknotes,[79] which was used by the Bank of Ireland for whom he was the official engineer from 1840 to his death.[80] In the mid-1830s he had become involved in the construction of large telescopes.[81] After constructing an observatory and a 9-inch telescope of his own, the first large instrument he made was a 15-inch equatorial reflector supplied to Armagh Observatory.[82] From his engineering and instrument making base, Thomas became involved in Dublin scientific life, being a Member of the Royal Irish Academy and publishing varied work in Dublin scientific

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[77] Clarke, Morrison-Low and Simpson, *op.cit.* (note 61), pp.75-84.

[78] J.E.Burnett, "Thomas and Howard Grubb", in Burnett and Morrison-Low, *op.cit.* (note 21), pp.89-117.

[79] *Ibid.*, pp.94, 101.

[80] *Ibid.* See also T.Grubb, "On decimal systems of money", *Journal of the Royal Dublin Society*, 1856-7 (1), pp.21-32.

[81] Burnett, *op.cit.* (note 78), p.94.

[82] *Ibid.*

journals.[83]

Howard Grubb (1844-1931) followed in his father's business after a university education at Trinity College, Dublin,[84] and was even more active in the Irish scientific community, making as many as 33 contributions to the publications of the Royal Dublin Society[85] and holding its office of Vice-President for some years.[86] As has been mentioned earlier,Thomas and Howard Grubb were also the last instrument makers to become Fellows of the Royal Society of London.

Other makers besides the Grubbs took part in Dublin scientific life - for example the Yeates family, who can be seen as typical of the more traditional instrument maker (i.e they devoted themselves to a wide range of instruments, rather than concentrating on large observatory instruments).[87] Of the two most eminent sons of instrument maker Samuel Yeates, Andrew, the younger, moved to London to set up on his own account after a period of work under Edward Troughton.[88] He became a Fellow of the Royal Microscopical Society and also of the Royal

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[83] For example, T.Grubb, "On a new Table Microscope", *Journal of the Royal Dublin Society,* 1860-2 (3), pp.85-8; T.Grubb, "A new method of determining, approximately, the spherical aberration of a combination of lenses for microscopic purposes", *Proceedings of the Royal Irish Academy,* 1853-4 (6), pp.59-62.

[84] Burnett, *op.cit.* (note 78), p.106.

[85] For example, H.Grubb, "The Great Melbourne Telescope", *Journal of the Royal Dublin Society,* 1870 (5), pp.460-74.

[86] Burnett, *op.cit.* (note 78), p.106.

[87] Morrison-Low, *op.cit.* (note 21), pp.41-6.

[88] *Ibid.,* p.42. Yeates' notebook of this time survives in General Company Records of Troughton and Simms, Vickers Collectiom AJB050, Borthwick Institute of Historical Research, York.

Astronomical Society, to which he contributed a short paper.[89] His elder brother George was able to take a fuller role in Dublin scientific life than Andrew was in London, while still having a business to run. George published an annual meteorological journal for the Royal Irish Academy,[90] as well as providing notices of meteorological phenomena,[91] and he introduced his son Stephen to the instrument making business and to Dublin scientific life.[92] Morrison-Low claims that Stephen Yeates was a member of the selective Dublin Microscopical Club, whose numbers were limited by its constitution to twelve.[93]

The activity and prominence of Scottish and Irish instrument makers in their respective local scientific communities contrasts sharply, then, with the parts which instrument makers were able to play in the metropolitan scientific community. In the general and specialist societies of London, as well as in the British Association whose positions of power were held by the members of the philosophical elite I have characterised in this thesis, the instrument maker's role was limited to one of making his name known to potential clients and demonstrating his work. The proportion of papers contributed to specialist societies by those

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[89] A.Yeates, "On the notches, Y's, or bearings for the pivots of transit instruments", *Monthly Notices of the Royal Astronomical Society,* 1865 (25), pp.214-15.

[90] Morrison-Low, *op.cit.* (note 21), p.46, citing G.Yeates, "Meteorological journal, from 1st January to 31st December 1843", *Proceedings of the Royal Irish Academy,* 1846 (2), appendix.

[91] G.Yeates, "On a Meteor seen near Dublin", *Proceedings of the Royal Irish Academy,* 1850 (4), pp.37-8.

[92] Morrison-Low, *op.cit.* (note 21), p.46.
[93] *Ibid.*

who made instruments was extremely small, and as I have indicated, very much lacking in original additions to knowledge, generally being little more than notifications of the latest small design refinement. The next section of this chapter discusses more fully the nature of the papers which instrument makers contributed to scientific journals (both those attached to a society and independent journals) in the nineteenth century. This will provide a more complete picture of the business-related motives of the instrument maker in aspiring to scientific respectability, which will be developed further in succeeding sections.

4. Publication by Instrument Makers in Scientific Journals.

Of the independent scientific journals which existed in the early part of the nineteenth century as alternative publication media for scientific work to the *Philosophical Transactions of the Royal Society,* the two most famous were the *Philosophical Magazine* (under various authorships from 1798), and William Nicholson's *Journal of Natural Philosophy, Chemistry and the Arts* which first appeared in 1797 but ceased publication in 1813. Many papers appeared in both journals,[94] giving makers a double chance to advertise their expertise, but the only instrument maker who made substantial use of Nicholson's *Journal* was John Cuthbertson, who communicated to it much of his experimental

[94] For example J.Allan, "Description of Improvements in a Mathematical Dividing Engine", *Philosophical Magazine,* 1811, pp.57-60, and *Journal of Natural Philosophy, Chemistry and the Arts,* 1812, pp.5-9; J.Allan, "Description of an improvement on La Borda's Reflecting Circle", *Philosophical Magazine,* 1812, pp.249-53, and *Journal of Natural Philosophy, Chemistry and the Arts,* 1812, pp.112-17.

electrical research.[95]

Cuthbertson, born in 1743, had moved to Amsterdam in 1768 and remained there until the mid-1790s, performing his electrical experiments with many of the best known Dutch philosophers and giving public lectures (in the same way as Benjamin Martin had in England), as well as making instruments.[96] Although he did not venture into theoretical speculation, accepting Franklin's one-fluid theory of electricity with little question,[97] his experimental work placed him firmly in the tradition of eighteenth-century instrument makers which I have characterised in Chapter 1 - as a contributor to scientific advance and knowledge rather than a mere constructor of apparatus. He provided modifications to the Leyden jar and battery, invented a new type of electrometer, and brought the plate electrical machine to "a high level of sophistication and efficiency".[98] Cuthbertson was also involved with the development of methods by which the efficiency of different types of electrical machine

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[95] Among the more lengthy papers of the 10 or so he published were J.Cuthbertson, "An examination of Sig.Volta's experiments which he calls fundamental", *Journal of Natural Philosophy, Chemistry and the Arts,* 1802, pp.281-9; J.Cuthbertson, "A Series of Experiments upon Metals with an Electrical Battery", *Journal of Natural Philosophy, Chemistry and the Arts,* 1802, pp.136-47.

[96] W.D.Hackmann, *John and Jonathan Cuthbertson. The Invention and Development of the Eighteenth Century Plate Electrical Machine,* (Leyden, 1973), pp.14-15.

[97] *Ibid.,* p.20.

[98] *Ibid.*

could be compared.[99]

When Cuthbertson returned to England he continued these researches, and Nicholson's *Journal* was found to be a convenient communication channel. Not having become a Fellow of the Royal Society, however, he was denied the opportunity to display his results to the higher echelons of the scientific community; this situation was in contrast to that in Holland where he seems to have performed experiments with most of the leading representatives of the Dutch scientific community.

Cuthbertson made his living by the sale of instruments, and thus any improvements which he made in electrical apparatus were likely to be an advantage to him in trade. Even so, the fact that he used the profits from the sale of his products to fund his research[100] demonstrated that his interest in electrical work was at least in part motivated by a gentleman's desire to contribute to scientific advance, rather than simply to promote his business. His work in Nicholson's *Journal* showed that his ambitions were more closely linked to those of the typical makers of the eighteenth century (perhaps especially the other electrical enthusiast, Edward Nairne) than to those of the nineteenth.

The other communications made to the *Journal* in the early part of the nineteenth century were short notices of the latest

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[99] *Ibid.* See also J.Cuthbertson, "On some improvements in the Electrical Machine", *Journal of Natural Philosophy, Chemistry and the Arts,* 1810, pp.9-13; J.Cuthbertson and G.J.Singer, "Experiments on the comparative Powers of Cylinder and Plate electrical Machines", *Journal of Natural Philosophy, Chemistry and the Arts,* 1810, pp.218-25.

[100] Hackmann, *op.cit.* (note 96), p.39.

small refinement in instrument design: for example, Thomas Jones gave a description of an invention which he employed[101] and Robert Brettell Bate responded to an earlier attack on the Camera Lucida by listing its advantages,[102] undoubtedly so that sales of the instrument would not be adversely affected. Edward Troughton also made one of his rare written additions to scientific literature in the *Journal*.[103] Overall, however, its influence as a down-market alternative to the *Philosophical Transactions* was limited, especially as it ceased publication after 1813.

Of the elite makers, Troughton published very little in any journal, Simms managed to publish all of his work, as we have seen, in the *Memoirs of the Royal Astronomical Society*,[104] and George Dollond contributed short papers to the Royal Society and the Astronomical Society.[105] There was undoubtedly a sense in which these makers were so well known by the 1820s that they had less to gain in business terms by spending time contributing

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[101] T.Jones, "On Mr.Woolf's invention for equalising the action of a Crank", *Journal of Natural Philosophy, Chemistry and the Arts*, 1804, pp.133-4.

[102] R.B.Bate, "On the Camera Lucida", *Journal of Natural Philosophy, Chemistry and the Arts*, 1809, pp.146-50.

[103] E.Troughton, "Description of a Tubular Pendulum", *Journal of Natural Philosophy, Chemistry and the Arts*, 1804, pp.225-30.

[104] Simms, *op.cit.* (notes 17 and 18).

[105] Dollond, *op.cit.* (notes 6 and 8). Also G.Dollond, "An Account of a Micrometer made of Rock Crystal", *Philosophical Transactions of the Royal Society of London*, 1821, pp.101-4; G.Dollond, "An Account of a Concave Achromatic Glass Lens, as adapted to the Wired Micrometer when applied to a Telescope, which has the property of increasing the magnifying power of a Telescope without increasing the diameter of the Micrometer Wires", *Philosophical Transactions of the Royal Society of London*, 1834, pp.199-203.

papers to societies and journals. For less well known makers, such as Thomas Jones, publication in journals constituted an important part of advertising oneself to the scientific community. Thus it is not surprising to find several papers by Jones in the *Philosophical Magazine*,[106] in the period prior to his achievement of the social status of Fellow of the Royal Society. As he had been apprentice to Jesse Ramsden, Jones took the advantage of mentioning his late master in several of these papers, to increase his credibility. For example on the optigraph (an instrument to aid drawing in perspective) he stated:

> The late most ingenious Mr.Ramsden, so well known for his inventions and improvements in various instruments, considered the present subject an object worthy of his attention, and invented the instrument I am about to describe...[107]

Jones continued to give the reader an idea of where his originality and expertise lay in this instrument:

> Mr.Ramsden left this instrument without the means of enabling the operator to enlarge or diminish his drawing, an inconvenience which I have obviated, while at the same time I have added some other trifling improvements. This instrument is certainly superior to any hitherto constructed for the same purpose...[108]

At the end of the paper, as might be expected, Jones gave the price of the instrument lest any impressed reader wish to buy one.

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[106] T.Jones, "Description and use of an instrument called "The Sectograph"", *Philosophical Magazine*, 1813, pp.401-7; T.Jones, "Description of a new Reflecting Compass", *Philosophical Magazine*, 1815, pp.7-9; T.Jones, "Description of an Improved Optigraph", *Philosophical Magazine*, 1807, pp.66-9.

[107] *Ibid.*, p.67.
[108] *Ibid.*

As we have seen, some makers were able to make extensive contributions to the scientific literature in their local context, an opportunity grasped by Alexander, John and Richard Adie in Edinburgh, and Thomas and Howard Grubb in Dublin, but for the most part instrument makers were denied such close ties with those in control of publication media. There were exceptions, as with John Newman, who in his capacity as maker to the Royal Institution, gained more ready access to publish various notes of his latest instruments in the Institution's *Quarterly Journal of Science, Literature, and the Arts.*[109] These were of course no different in nature to those of Jones in the *Philosophical Magazine* - short accounts which effectively advertised his business and expertise to a readership of potential clients.

In his two papers on the blow-pipe, Newman not only was keen to emphasise the fact that he sold the item he had been discussing, he was also courteous enough to mention that it was not his own design, but that of Professor Cumming, of Cambridge.[110] He was also careful to warn of any potential hazard to a purchaser of the instrument:

> Whilst the blow-pipe is in the state described, I believe it to be perfectly safe, for I can imagine no possibility of the flame passing into the interior of the box. Experience alone, however, can prove its safety, and... it is possible that

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[109] J.Newman, "Description of a new Machine to measure a Ship's Way by the Log-line", *Quarterly Journal of Science, Literature and the Arts,* 1817, pp.90-1; J.Newman, "On a Mountain Barometer constructed with an Iron Cistern", *Quarterly Journal of Science, Literature and the Arts,* 1823, pp.277-9.

[110] J.Newman, "Account of a new Blow-pipe", *Journal of Science and the Arts,* 1816, pp.65-6; J.Newman, "An Account of an improved Blow-pipe", *Quarterly Journal of Science, Literature and the Arts,* 1817, pp.379-82, on p.380.

> some unknown circumstance, or set of circumstances, may occur, which are not here provided for...[111]

The main channel by which the ambitious instrument maker could publicise his work, however, especially after the demise of Nicholson's *Journal,* was undoubtedly the *Philosophical Magazine,* which was of course not attached to any institution. Although it could be regarded as an alternative philosophical journal to the *Philosophical Transactions,* many men of science regarded it as a less rigorous publication medium than the Royal Society's periodical. Thus, although it did encourage contributions from all classes, its audience mainly comprised the "gentlemanly devotee",[112] so that papers by gentlemanly philosophers were preferred. Thus elite philosophers often contributed to it papers which they may have deemed, for whatever reason, unsuitable for submission to the Royal Society.[113] This certainly increased the credibility of the journal, but also caused a lesser role to be taken in its pages by instrument makers than would otherwise have been the case.

Even so, considerable advantage was taken of the opportunity it presented by Thomas Jones, as we have seen, and the fierce controversy between Edward Montague Clarke and Joseph Saxton on

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[111] *Ibid.,* p.382.

[112] D.S.L.Cardwell, *James Joule. A biography,* (Manchester, 1989), p.17.

[113] Faraday in particular published extensively in its pages. His first contribution was in 1820: M.Faraday and J.Stodart, "Experiments on the alloys of steel, made with a view to its improvement", *Philosophical Magazine,* 1820, pp.26-35. Up until 1832, however, he published mainly in the Royal Institution's own periodical. When the *Quarterly Journal* was discontinued, he again published in the *Philosophical Magazine,* and contributed around 40 papers to it in total.

the subject of electrical machines, discussed in Chapter 3, was fought out on its pages. Francis Watkins published several papers in the 1830s on electro-magnetism[114] and thermo-electricity[115] in the *Philosophical Magazine,* during the period in which Watkins and Hill were the premier firm for the manufacture of electrical and magnetic apparatus. Watkins' publications in this area helped reinforce this status and give authority to the business.

Although other instrument makers made isolated contributions to scientific journals in this period,[116] and individuals on the periphery of the philosophical instrument making community such as Alexander Bain[117] and Edward Dent[118] were keen to provide evidence of their expertise in their fields of work, the proportion of written papers by instrument makers remained small. The controlling position of those who saw themselves as members of the philosophical elite in these various journals ensured that particular types of individuals and research tended to be preferred. For the most part, this meant research seen as representing a contribution to scientific knowledge, i.e. work by

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114 F.Watkins, "On Magneto-electric Induction", *Philosophical Magazine,* 1835, pp.107-13; F.Watkins, "On Electro-magnetic Motive Machines", *Philosophical Magazine,* 1838, pp.190-6.

115 F.Watkins, "On Thermo-electricity", *Philosophical Magazine,* 1837, pp.304-7; F.Watkins, "On the Evolution of Heat by Thermo-Electricity", *Philosophical Magazine,* 1839, pp.82-3.

116 For example James Allan: Allan, *op.cit.* (note 94).

117 A.Bain, "On the Earth, as a Conductor and Permanent Generator of Voltaic Electricity", *The Electrical Magazine,* 1845 (1), pp.50-2.

118 E.J.Dent, "On the Errors of Chronometers, and explanation of a new construction of the Compensation-balance", *American Journal of Science and the Arts,* 1843 (45), pp.83-93.

a man of science who devoted his time to philosophical pursuits, and not by an artisan who devoted himself to a trade. The instrument maker had advantages to gain by publishing in scientific journals in the period, but these were related to economic and market factors, not to his philosophical standing.

<u>5. Publication of Books by Instrument Makers.</u>

In this section I would like to develop the theme of publication as a means of career advancement by considering the work which was produced by instrument makers in book and pamphlet form, other than makers' catalogues, which will be discussed in a later section. Research on this topic is made difficult by the fact that such work was often printed in small quantities (usually at the makers' own expense), and so catalogues in libraries like the British Library may not be exhaustive of this material. Even so, copies of the more lengthy books written by instrument makers survive, though the proportion of shorter pamphlets (which are perhaps better classed as ephemera) of which copies remain is unclear. The motivation of makers for publishing material in book form, it will be argued, was to advertise their expertise in scientific matters, and their business. It was not an attempt to contribute to scientific knowledge, except in the sense of its diffusion.

John Cuthbertson, as we have seen, published extensively on his electrical research in the early part of the century, and produced a lengthy textbook, *Practical Electricity and*

Galvanism,[119] which brought together his own work with contemporary electrical researches. This book was sufficiently successful to go through a second expanded edition in 1821, the year of his death.[120] However, as has been argued earlier in this chapter, Cuthbertson should be regarded as an instrument maker of the eighteenth century, and as such his motivations were able to be different from those of a nineteenth-century maker.

Francis Watkins, the next instrument maker to produce a book on an electrical subject,[121] made clear, as we saw in Chapter 6, that his book was not intended as an original contribution to knowledge, but as a popularisation of the subject of electro-magnetism.[122] As such, it made sense for him to spread the knowledge of a subject for the investigation of which he made expensive instruments. Indeed, a catalogue of the instruments made by Watkins and Hill which one could use to perform the experiments described in the book, was subjoined.

This notion, that one could use the publication of popularising textbooks to enhance the market for one's products, was further illustrated in the works of Andrew Pritchard. Pritchard published several books on microscopy jointly with

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[119] J.Cuthbertson, *Practical Electricity and Galvanism,* (London, 1807).

[120] J.Cuthbertson, *Practical Electricity and Galvanism,* (2nd edition, London, 1821).

[121] F.Watkins, *A Popular Sketch of Electro-magnetism, or Electrodynamics,* (London, 1828).

[122] *Ibid.,* pp.1, 67.

C.R.Goring,[123] as well as several on his own account.[124] Whilst Pritchard did not make all the instruments he sold, and while he was not devoting all his time to his instrument making business (also working as a patent agent),[125] these works were by no means wholly prompted by a love of microscopy. Pritchard realised that providing information of what one could do with, and see through, one of his microscopes would enhance considerably the market for them among wealthy gentlemen (and even ladies, unusually for the philosophical instrument market at this time). The books contained, as appendices, notices of the instruments which Pritchard could supply to the reader. His *Practical Treatise on Optical Instruments,*[126] first published as part of the Society for the Diffusion of Useful Knowledge's *Library of Useful Knowledge,* presents another example of the desire to promote a market for one's products, as well as to popularise science itself: a very large portion of the book is given over to microscopes, Pritchard's main source of livelihood.[127] He was generous, however, in ascribing the credit for the invention and improvement of the optical instruments he described to those who provided the market for them, rather than those who constructed them:

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[123] Goring and Pritchard, *op.cit.* (note 54).
[124] A.Pritchard, *A History of Infusoria, living and fossil,* (London, 1842); A.Pritchard, *A History of Infusorial Animalcules,* (London, 1852).

[125] Nuttall, *op.cit.* (note 51).
[126] A.Pritchard, *A Practical Treatise on Optical Instruments,* (London, 1832).

[127] *Ibid.,* pp.33-52.

> The construction of optical instruments has, in almost every instance, originated with eminent philosophers and mathematicians. Their gradual perfection has been a natural result of the difficulties which were presented to the progress of discovery, by the inefficient and inaccurate means which science possessed; and thus, the same great minds that have struck out and pursued vast and splendid ideas in their investigations of nature, have only been enabled to follow up their own conceptions by applying themselves to the practical improvement of the instruments with which they had commenced their discoveries...[128]

This acknowledgement of their expertise would of course have been welcomed by those philosophers who read the book. However, Pritchard's analysis also begs the question as to whether he considered his standing in the scientific community to be more akin to the philosophers to whom he referred than to the artisan. Certainly, in the 1850 edition of the work, he claimed that he cultivated the science of optics as an amateur,[129] suggesting that he wished to align himself with the gentleman-philosopher, rather than the artisan. Even so, in the 1832 edition, his primary intentions were certainly to promote the market for optical instruments and therefore it represents a typical instrument maker's work of the period.

William Simms produced a work on the achromatic telescope in 1852[130] (just after his involvement with Airy's transit circle for the Royal Observatory), the motivation for the writing of which he expressed in its preface:

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[128] *Ibid.*, p.1.

[129] A.Pritchard, *A Practical Treatise on Optical Instruments*, (New edition, London, 1850), p.61.

[130] W.Simms, *The Achromatic Telescope and its Various Mountings, especially the Equatorial*, (London, 1852).

> Purchasers of Achromatic Telescopes, mounted equatorially or otherwise, having frequently requested me to furnish them with a concise statement of the leading principles upon which the construction and application of such instruments depend, it occurred to me that I should best consult the convenience of such applicants, by preparing for the press a brief summary of the subject...[131]

Simms pointed out that the work did not contain any discussion of principles, i.e. it was devoted to a *practical* description of the working of the instruments.[132] Though it was claimed to be written for those who had purchased telescopes from Simms already, there was undoubtedly a sense in which the work was intended to diffuse knowledge of telescopes, and to provide an expansion of the market for them, something which would benefit the firm of Troughton and Simms. As if to emphasise this, a catalogue of instruments made by the firm was appended.

Simms' son, William Henry, though not involved in the business, produced a work on the sextant[133] which might have been designed partly to promote his father's activity, but it was very mathematical in nature, and seems to have been produced with intentions (such as a wish to enhance his scientific status) other than to promote his father's business.

After these few books published by instrument makers, we can find little else in the British Library catalogue but a small number of pamphlets produced by makers specifically to advertise given instruments which they had constructed - for example Thomas

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[131] *Ibid.*, p.iii.
[132] *Ibid.*
[133] W.H.Simms, *The Sextant and its Applications,* (London, 1858).

Jones,[134] Peter and George Dollond,[135] and John Newman[136] all produced such pamphlets. There is one other anomaly in the catalogue - a book of poems by George Dollond.[137] The latter was the nephew of the George Dollond with whom this thesis has dealt, and his successor in the instrument making business.

Chronometer makers also produced work in book form to demonstrate their expertise and knowledge, and to promote a demand for their products. Edward Dent, whom we have already seen using the British Association for career purposes, was notably active in this area. Not only did he produce a short book on the construction of chronometers, watches and clocks,[138] aimed at anyone from naval officers to astronomers to private gentlemen, he also produced a treatise on the aneroid barometer,[139] which he helped to develop (it had been invented by a Frenchman, Vidi).[140] The work on the aneroid included an advertisement for it at the back, along with an advertisement for Dent's Patent Dipleidoscope, an instrument used for telling the time by

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[134] T.Jones, *Description and Use of "The Sectograph"*, (London, 1814); T.Jones, *A Companion to the Mountain Barometer*, (London, 1817).

[135] P.and G.Dollond, *A Description of the Eirometer*, (London, 1811); P.and G.Dollond, *The Description of a Binnacle Compass, Illuminated by Prismatic Reflection*, (London, 1812).

[136] J.Newman, *Instructions Necessary to be attended to, in using Newman's Standard, or Portable Mountain Barometer*, (London, 1841).

[137] G.Dollond the younger, *Irene, a Love Story, and Other Poems*, (London, 1854).

[138] E.J.Dent, *On the Construction and Management of Chronometers, Watches and Clocks*, (London, 1846).

[139] E.J.Dent, *A Treatise on the Aneroid, a Newly Invented Portable Barometer*, (London, 1849).

[140] *Ibid.*, p.13.

observing the transit of the sun by day, or the stars by night, over the meridian. Dent had collaborated with J.M.Bloxam on the design of this instrument, and produced a treatise on it as well.[141]

The work published in book form in the nineteenth century, whether by philosophical instrument makers or by chronometer makers, however, had a number of key features in common. Firstly, all such books were relatively short, some being little more than pamphlets. Only Cuthbertson's book on electricity[142] was a more substantial piece of work. Secondly, the works addressed scientific principles only to a very minor extent, concentrating on the practical aspects of the use of the instruments being discussed. Thirdly, and related to the second point, there was little of any scientific novelty in these works. Watkins[143] and Simms[144] claimed as much in their respective introductions. Finally, the point must be emphasised that the motivation to the instrument maker for publishing such work was that it gave him an opportunity to promote his business by creating a market for his products. It is significant that the publications were produced almost exclusively at the expense of the instrument makers themselves.

Philosophers, meanwhile, published books in this period not for financial reasons, but from a desire to increase their standing in the scientific community and in society. Hence a book by one who considered himself a philosopher would in general be

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[141] E.J.Dent, *A Description of the Dipleidoscope,* (London, 1843).
[142] Cuthbertson, *op.cit.* (note 119).
[143] Watkins, *op.cit.* (note 121), p.1.
[144] Simms, *op.cit.* (note 130), p.iii.

more substantial in extent, and would contain some claims to originality, even if these had first appeared in journal form. The instrument maker, on the other hand, as I have argued, was urged by business pressures to publish in books, in journals, and to take part in societies. In the rest of this thesis I have ascribed the decline in instrument maker's status in the scientific community mainly to the increasing sense in which ambitious philosophers tended to refine the criteria for membership of that community, but it has also been clear that more was being expected of the instrument maker in terms of his business than had been the case in the eighteenth century, and in this chapter I have linked the activity of the maker in scientific circles with the promotion of his business. In the remainder of this chapter I connect the decline of the instrument makers' scientific status more fully with the business pressures which he faced and which led him to play the roles in scientific activity with which this chapter has so far been concerned.

6. The Instrument Making Trade and its Pressures on the Artisan.

In the nineteenth century, instrument making became less affected by industrialisation than did most other trades, in that mechanisation was impractical in many of its precision branches, meaning that there was still a considerable amount of manual craftsmanship involved. However, the *market* was considerably affected so that the maker was presented with a new set of business pressures. Instruments still took a great deal of the craftsman's time to perfect; it has been suggested that a typical top-range Powell and Lealand microscope, for example, required some six hundred man-hours to complete,[145] and with the burgeoning of new observatories the astronomical instrument maker's time was often required at long stretches to equip these sites with the best telescopes. William Simms expressed the difficulties which the individual artisan faced (and how he felt he should be compensated for them) in a paper on a self-acting dividing engine, which he had developed from Troughton's original Copley Medal-winning method:

> Connected with the arts, no operation, perhaps, is attended with more difficulty, or to ensure success requires greater care and diligence on the part of the operator, than the original graduation of a circle for astronomical or the higher class of geodesical purposes; and, although the method invented by Mr.Troughton is a vast improvement upon every thing of the kind by which it was preceded, both as it lightens labour and ensures a high degree of accuracy in the result, yet, even by that method, the labour and application required for such a work are so considerable as not to admit of frequent repetition, without making serious inroads upon the constitution of the man engaged in such pursuits.
> Moreover, as graduation involves both the exercise of skill and the sacrifice of health on the part of the operator, its cost necessarily forms a considerable item in the price of an instrument...[146]

Simms' emphasis on the craftsman's sacrifice of his health for the sake of his art reinforces the argument that business pressures prevented the instrument maker from having the leisure to devote to scientific research, as was the case in the eighteenth century. The pressure on the individual artisan was also exerted on his firm. With the increasing demand for instruments not only from scientific customers, wishing to

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[145] H.M.Malies, "The Microscopes of Powell and Lealand", *The Microscope,* 1946-8 (6), pp.95-7, on p.96.

[146] Simms, *op.cit.* (note 18), p.83.

develop prototype instruments for their research, but also from professional customers, seeking instruments for surveying, navigation, and astronomy, firms had to expand their businesses in order to meet the demand and also to ensure that they kept pace with their rivals. New markets also developed in the nineteenth century, such as a market which demanded instruments to be used for teaching science, and these had to be catered for accordingly. In other words, the instrument maker, who in the earlier period had been an individual maker responding to a small number of orders, effectively became a manager of a workforce and business, meaning that a role in scientific development and innovation (even if his educational background had allowed it) became increasingly beyond his reach.

In chronometer making, the situation was even more acute in that very few developments were made to the chronometer after 1820 (with the exception of attempts, in which Airy played a part, to solve the error of compensation, i.e. to allow for temperature fluctuations),[147] so that the leading makers became little more than managers of workforces. Davies claims that Arnold and Dent, for example, employed 43 different workmen in the assembly of one item.[148]

In instrument making firms, owing to the scarcity of archive material, it is difficult to ascertain the exact sizes of workforce employed by the leading makers. In some areas, such as

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[147] A.C.Davies, "The Life and Death of a Scientific Instrument: The Marine Chronometer 1770-1920", *Annals of Science,* 1978 (35), pp.509-25, on p.521.

[148] *Ibid.,* p.514.

microscope manufacture, the staff remained small in number: Powell and Lealand never had a staff of more than five, and Hugh Powell and his son Thomas made all the objectives themselves.[149] In those departments of instrument making for whose products there was a large demand, workforces could be as high as those of a small factory - for example Mennim claims that by 1881 James Simms (son of William) had 78 men and 26 youths working for him.[150] Even with a relatively small firm such as that of Casella, who mainly made meteorological instruments, staff levels would have been well into double figures - by the early twentieth century they employed 24 workers and the First World War caused a further employment of around a dozen individuals, and an increase in working hours, to meet the extra demand.[151]

For those firms for whom output figures can be found, a similar story is told: workforces and output were in almost continual expansion throughout the nineteenth century in all aspects of instrument manufacture. Microscope makers, whose output and market remained relatively small, at least until the commencement of microscope use for medical and teaching purposes, increased production at a noticeable rate throughout the 1830s

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[149]Turner, *op.cit.* (note 27), p.124.

[150]Mennim, *op.cit.* (note 10), p.31. In Glasgow, the market for instruments designed by William Thomson and manufactured by James White burgeoned to such an extent in the latter part of the century that White's workforce rose from 18 men in 1871 to a total of 400 by the end of the century. By 1900 the work was carried out in a five-storey building and involved everyone from a large staff of electrical experts to a group of female mahogany polishers: Clarke, Morrison-Low and Simpson, *op.cit.* (note 63), p.258.

[151]Staff Time Book, C.F.Casella and Company Records D/B/CAS/54, Hackney Borough Archives, Rose Lipman Library, Hackney.

and 1840s. The three main metropolitan makers, Powell, Smith and Ross, made 724 microscopes between them in the years 1836-46.[152] This rate of production increased to 99 instruments in 1847 alone, by 1852 they made 179, and in 1857 they constructed 357 microscopes.[153]

The output in other areas of manufacture was even more strikingly extensive. Between 1868 and 1881 Louis Casella and his workforce made and divided the scales of over 48000 thermometers.[154] In the late 1860s they had been making barely 1500 a year, though by the 1880s this figure was over 5000 a year, and continued to rise.[155] A substantial portion of these thermometers had to be sent to Kew Observatory for verification, an indication that the philosophers in control of that establishment wished to emphasise their authority over the quality of the products supplied by artisans, in as much as this quality would affect work by men of science. In 1867, for example, a total of 70 items, including thermometers, barometers, and sympiesometers, was sent by the firm for verification by Balfour Stewart at Kew.[156] Clearly, the size of the demand for all types of instrument was in a very healthy state from a business point of view, but it necessitated an increasing devotion to business on the part of the maker. He could only survive if his products were up to the standard demanded by the market, and it was the philosopher who defined this market. In

[152] Turner, *op.cit.* (note 24), p.245.
[153] *Ibid.*
[154] Stock Book, C.F.Casella and Company Records, D/B/CAS/2, Hackney Borough Archives, Rose Lipman Library, Hackney.

[155] *Ibid.*
[156] *Ibid.*

the next section I would like to consider a way in which the maker could actively encourage the market to buy his products - by the production of advertising catalogues.

7. The Instrument Maker's Catalogue.

An almost universal method of response, on the part of the makers, to the pressures of business, was to issue catalogues of the instruments which it was possible to purchase from them. Casella produced particularly extensive material in this respect. In his 1861 catalogue,[157] the opening address made clear to the reader the quality of the instruments which could be bought from this experienced and knowledgeable maker:

> In presenting a new and extended catalogue to the public, my chief desire has been that it should fairly represent the various instruments connected with my establishment, including such recent additions as should efficiently meet the scientific and manufacturing wants of the day.
> The extensive alterations lately made in my premises, enable me to manufacture much more under my own care, and to carry out more efficiently many modifications and improvements in the various branches of my establishment, whilst gentlemen desiring to superintend or construct new arrangements of their own, can do so with perfect confidence, either personally or by forwarding drawings with instructions, and thus obtain the aid of practical workmen on most of the scientific subjects with which they may be engaged.[158]

Casella continued by claiming his superiority to other makers whom the reader might select:

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[157] L.P.Casella, *Illustrated and Descriptive Catalogue of Philosophical, Meteorological, Mathematical, Surveying, Optical and Photographic Instruments Manufactured by Louis P.Casella,* (London, 1861).

[158] *Ibid.,* p.v.

> To the METEOROLOGICAL DEPARTMENT I beg to direct particular attention, with the full belief that in many of its branches an excellence is attained unequalled by that of any other house in London... An extensive intercourse with the leading Opticians and Scientific Bodies enables me to introduce every novelty of interest as soon as it appears, and thus, though not made by myself, or in this country, to obtain it at once from the Continent...[159]

This drew attention to the fact that not all of the instruments in the catalogue were actually *made* at Casella's premises. Although it was clear that he did make most of the thermometers and other meteorological instruments which he sold (some 150 different types),[160] it was also apparent that Casella bought instruments such as the telescopes he supplied. For example, he ordered certain telescopes from Thomas Cooke of York, as Cooke's order books testify.[161] Indeed, Cooke engraved Casella's name on these instruments as the maker,[162] Casella presumably wishing to perpetuate the belief that he was able to make all types of scientific instrument.

The 1861 catalogue contained a total of 1399 different items which Casella was able to supply, including 130 items of electrical apparatus[163] and around 200 items related to the telescope and microscope,[164] only a small proportion of which he

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[159] *Ibid.*
[160] *Ibid.*, pp.6-30.
[161] Thomas Cooke and Sons Order Book, 1856-68, Vickers Collection AJB030, Borthwick Institute of Historical Research, York.

[162] *Ibid.*
[163] Casella, *op.cit.* (note 157), pp.76-84.
[164] *Ibid.*, pp.52-66.

could have made. In the 1871 edition of the catalogue,[165] the presentation was even more lavish, and the reader continued to be urged of the first-class quality of the work available from the maker:

> To self-registering instruments much of my attention is constantly given, and several of them are now described for which I was honoured with the only Prize Medal awarded to this class of instruments at the Great Exhibition of 1862, as well as the much extended patronage of the leading Governments and Observatories of the world, as shown on the title page...[166]

In the intervening decade Casella's business had expanded to the point at which he was now able to list almost 3000 items which he sold - this increase in his operation has already been noted with respect to his thermometers. The catalogue also was notable in that it contained short descriptions of the history of design of many of the instruments, especially the meteorological ones, presumably in order to create a market for such instruments among the casual and less well-informed section of the readership.

Similarly extensive catalogues to Casella's were issued during this period by Negretti and Zambra. Their third catalogue in 1859[167] included sections on standard meteorological instruments, photographic equipment, and stereoscopes and

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[165] L.P.Casella, *An Illustrated and Descriptive Catalogue of Surveying, Philosophical, Mathematical, Optical, Photographic and Standard Meteorological Instruments manufactured by Louis P.Casella,* (London, 1871).

[166] *Ibid.,* p.v.

[167] Negretti and Zambra, *An Illustrated and Descriptive Catalogue of Optical, Mathematical, Philosophical, Photographic and Standard Meteorological Instruments Manufactured and Sold by Negretti and Zambra,* (London, 1859).

stereoscopic views, which were also issued themselves as separate catalogues.[168] The firm made clear that they were not able to undertake the construction of all the instruments they listed in the catalogue, though their expertise with those instruments they did manufacture was certainly stressed to the reader:

> Owing to our having removed to more extensive premises, we are now enabled to undertake the manufacture of the greater portion of the instruments sold by us, and being practically acquainted with every department of our business, we feel confident of being enabled to give satisfaction with any orders we may be entrusted to execute. Our extensive and intimate connection with most of the first-class opticians on the continent, enables us to obtain early and correct information respecting any new instruments manufactured by them...[169]

The rhetoric here bears a remarkable similarity to Casella's in his catalogue of two years later - it is possible that Casella modelled his catalogue on those of Negretti and Zambra, for which by this stage they were quite famous. Their 1859 catalogue contained some 2135 entries, although, as they indicated, a proportion of these were manufactured elsewhere. Indeed, Cooke's order book shows that he supplied telescopes to Negretti and Zambra as well as to Casella.[170]

Later editions of the catalogue show that Negretti and Zambras' enterprise expanded even further with the increase of the market. Their 465-page catalogue of 1874 contained in excess

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[168] Negretti and Zambra, *Descriptive Catalogue of Standard Meteorological Instruments,* (London, 1859); Negretti and Zambra, *Descriptive Catalogue of Stereoscopes and Stereoscopic Views,* (London, 1859); Negretti and Zambra, *A Catalogue of Photographic Apparatus,* (London, 1859).

[169] Negretti and Zambra, *op.cit.* (note 167), p.v.

[170] Thomas Cooke and Sons Order Book, 1856-68, Vickers Collection AJB030, Borthwick Institute of Historical Research, York.

of 3000 items which one could purchase from the company.[171] The compiler of this and later catalogues was R.Willats, who managed the retail department at Holborn Viaduct, and who by 1874 had been with the firm some 18 years.[172] Undoubtedly this meant that Enrico Negretti himself considered that he had enough to do in managing a workforce, without having to spend time compiling an extensive catalogue. In the 1879 edition the purpose of the catalogue, to be a guide and encouragement to potential buyers, rather than a mere list, was emphasised explicitly:

> In this edition, as in all that have preceded it, our endeavour has been to make the work, not merely a list of prices, but in reality a guide for those who are purchasing Philosophical Instruments generally. All instruments are well described, some more fully than others, depending on the importance of the apparatus or article under consideration.[173]

Although few instrument makers produced catalogues on as lavish a scale as Negretti and Zambra or Casella, the intention in all cases was the same - to advertise oneself to those who purchased instruments, and to enhance the market for one's products among those who might not usually consider the purchase of philosophical instruments. With makers as eminent as Troughton and Simms, the dearth of written catalogues which they provided

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[171] Negretti and Zambra, *Encyclopaedic Illustrated and Descriptive Catalogue of Optical, Mathematical, Philosophical, Photographic and Standard Meteorological Instruments Manufactured and Sold by Negretti and Zambra,* (London, 1874).

[172] *Ibid.,* p.vii.

[173] Negretti and Zambra, *Encyclopaedic Illustrated and Descriptive Catalogue of Optical, Mathematical, Philosophical, Photographic and Standard Meteorological Instruments Manufactured and Sold by Negretti and Zambra,* (London, 1879), p.vii.

shows that they must have been sufficiently well known in the scientific world not to lack work, but the nature of those catalogues which they did produce suggests that they too were keen to promote a knowledge of, and desire to purchase, philosophical instruments generally. Chaldecott points out[174] that Troughton only produced one catalogue during his time in charge of the firm, in 1782. There is also evidence of a catalogue published in 1829, i.e. after Simms joined the firm, as well as those published as part of books by Simms[175] and by his brother, Frederick Walter.[176] Chaldecott only lists the firm as having produced one other separate catalogue before the 1880s (the one discussed in Chapter 5, produced around 1840, a copy of which exists in the Airy archive[177]). At least one other previously unknown catalogue was produced by the firm, however, in 1864.[178] The 1864 separate catalogue surprisingly only contained half the instruments which the 1852 version (the appendix to Simms' book) did. Both are certainly notable for being much less extensive than contemporary productions by

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174 J.A.Chaldecott, "Platinum and Palladium in Astronomy and Navigation. The Pioneer Work of Edward Troughton and William Hyde Wollaston", *Platinum Metals Review,* 1987 (31), pp.91-100, on pp.98-9.

175 Simms, *op.cit.* (note 130).

176 F.W.Simms, *A Treatise on the Principal Mathematical Instruments employed in Surveying, Levelling and Astronomy,* (London, 1834).

177 Catalogue of Instruments made by Troughton and Simms, c.1840, RGO6 160 f.147.

178 Catalogue of Instruments made by Troughton and Simms, 1864, Vickers Collection AJB110, Borthwick Institute of Historical Research, York.

Casella or Negretti and Zambra. It seems that Troughton and Simms were a sufficiently well known and respected business to be able to justify less frequent catalogues than these contemporaries, while still requiring to produce them to maintain their business, and even though their catalogues were much shorter than those of their competitors, this still did not mean that they actually *made* all the instruments therein, only that they felt able to be more selective about which instruments they chose to sell: the 1852 catalogue listed around 400 items and the 1864 edition less than 200 items. Other leading makers of the time were sometimes similarly selective. Thomas Cooke, for example, only listed 200 items in his catalogue of 1862,[179] and all these were related to astronomy, navigation, surveying, or engineering equipment.

The point which I wish to emphasise about the production of all these catalogues by instrument makers in the nineteenth century is that they were a necessary response to increased market competition. A catalogue which emphasised the expertise, skill, and extent of enterprise of a maker or firm helped in promoting a market for his products among casual readers, and among more professional customers, and gave him an advantage over his competitors. It was therefore to be expected that a maker would wish as far as possible to cultivate the impression among his readers that he was sufficiently organised to be able to manage the construction of all the instruments which were listed. With large lists, running to thousands of items, as in those of

[179] Catalogue of Astronomical, Surveying and Mathematical Instruments etc. manufactured by T.Cooke and Sons, 1862, Vickers Collection AJB110, Borthwick Institute of Historical Research, York.

Negretti and Zambra, many of the items were bought elsewhere to be sold in Negretti and Zambras' shop, and this was also the case with Casella, and to some extent with most of the other makers. Even so, a catalogue listing items which could be purchased from its producer increased his public profile and potential client base.

With such acute business pressures to which to respond in a rapidly expanding market, demonstrated by the increase in output, staff levels, and production of literature to promote one's firm, it was no surprise that the nineteenth century instrument maker was not able to assert his place in the scientific community as a contributor to the advance of scientific knowledge. The advent of the large exhibitions in the second half of the century provided the maker with another opportunity to advertise his business to large numbers of potential customers, and to demonstrate to scientific audiences wherein lay his originality and expertise. Although in the first aim success was assured, the British makers who exhibited at such events did not fare so well in the second regard. In the final section of this chapter I will consider the results of the Great Exhibition of 1851 bearing on the instrument making trade, and the significance these results had for the British scientific instrument maker in an expanding world market.

8. The Great Exhibition and the World Market for Instruments.

The Great Exhibition at the Crystal Palace in 1851 contained one class devoted to "Philosophical Instruments and Processes Dependent upon their Use", Class X (there were sub-classes Xa on Musical Instruments, Xb on Horological Instruments, and Xc on

Surgical Instruments).[180] The highest award in the Exhibition was a Council Medal of which 31 were awarded in Class X, 16 going to the United Kingdom makers who exhibited.[181] After this were awarded Prize Medals, and Honourable Mentions. Bennett shows that although the bare statistics suggest a successful performance from home products, a closer look at the results for British makers tells a different story.[182] Seven of the British medals were awarded for contributions outside the usual boundaries of instrument making - three of these for photographs or photographic processes and four for electric telegraph apparatus[183] (of which only the exhibits by Bain and W.T.Henley represented products of any originality by instrument makers, as opposed to philosophers). Of the remaining medals, those awarded to makers included one for Dollond's atmospheric recorder (described in Chapter 7), one to Chance Brothers for a flint glass disc, and one to Ludwig Oertling for his precision balances. This meant that the only indisputable merit found in traditional scientific instruments by British makers was for John Newmans's air pump and tide gauge, and in Andrew Ross and Smith and Beck, for their microscopes. The remaining medals were

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[180] Exhibition of the Works of Industry of all Nations, 1851, *Official Descriptive and Illustrative Catalogue of the Great Exhibition of the Works of Industry of all Nations 1851*, (London, 1851), pp.404ff.

[181] Exhibition of the Works of Industry of all Nations, 1851, *Reports of the Juries on the Subjects in the Thirty Classes into which the Exhibition was divided*, (London, 1852).

[182] J.A.Bennett, *Science at the Great Exhibition*, (Cambridge, 1983), pp.3-4.

[183] *Ibid.*, p.3, and *Reports of the Juries*, p.lxiii.

awarded to individuals unconnected with the industry.[184]

William Simms, as we saw in Chapter 5, was recommended unanimously by the Jury as deserving of a Council Medal, but the Council of Chairmen of the Exhibition chose to ignore this, and so Simms (who had only been persuaded to exhibit at the last minute) was only awarded a Prize Medal. Foreign makers, as Bennett shows from the Reports of the Juries, fared much better than their British counterparts, the French in particular being rewarded for excellence and originality within the traditional boundaries of instrument making, and for the superior quality of their workmanship.[185] Only the workmanship of the British microscopes as a class was, in the opinion of the jurors, without equal anywhere in the world.[186]

Although it has not been the aim in this thesis to consider the work of foreign instrument makers, their success in the Exhibition of 1851, and those subsequent to it, did undoubtedly have a deleterious effect upon the standing of the British makers in a world market, and it was increasingly becoming the case at this time that an international market for scientific instruments was appearing in parts of the world where astronomy, notably, had not before been cultivated. Large firms such as those of Grubb in Dublin and Cooke in York devoted a considerable part of their effort to major observatory work far from their home cities (where little demand existed for such work), and Simms equipped many observatories in different parts of the Empire. We have seen in Chapter 5 that he regarded his failure to win a Council Medal

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[184] *Ibid.*
[185] Bennett, *op.cit.* (note 182), p.4.
[186] *Ibid.*, p.7, and *Reports of the Juries*, p.243.

in 1851 as potentially of great damage to him abroad, whence he derived the greater part of his business.[187] We have also seen that Airy regarded the work of German and French makers as of equal quality to home makers,[188] but of course he used Simms because he was more convenient to the site of the work and to Airy's instructions, as well as being a personal friend.

The significance of the comparative failure of British instrument makers in an exhibition held on their own soil, then, was that it was damaging to them in a rapidly expanding world market for large instruments, and even to some extent for smaller commissions. William Thomson, for example, before the commencement of his relationship with James White, ordered a significant proportion of his research apparatus from Paris,[189] even though London would have been more convenient, suggesting that he considered the quality of the obtainable French products to be superior. Indeed, he had done so since before the results of the Great Exhibition, at which one of his favourite firms, Duboscq-Soleil, fared particularly well.[190]

For the most part, however, the home market remained very little affected by the results of the Exhibition, and the demand for scientific instruments from the British public increased steadily, providing makers with even less time to devote to

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[187] W.Simms to G.B.Airy, 20 October 1851, RGO6 441 f.269.

[188] Airy to J.Cranch, 26 May 1842, RGO6 159 f.106.

[189] C.W.Smith and M.N.Wise, *Energy and Empire. A biographical study of Lord Kelvin,* (Cambridge, 1989), pp.128-9. Thomson built up some of his Parisian connections while working there in Regnault's laboratory.

[190] *Reports of the Juries,* pp.263, 272; Bennett, *op.cit.* (note 182), p.6.

improvements, other than trifling practical ones, to their existing instruments. Original designs in instruments continued to be provided by the philosophers whose representatives had served on the juries in 1851 and later exhibitions, and the makers had simply to respond to these new designs and the increased demand which resulted. As a consequence of their inability to introduce grand new principles into their designs, the need to increase the efficiency of their businesses in an era of intense competition, and the tendency of the emerged philosophical elite to assert their exclusive position as the authoritative class in society, the standing of the scientific instrument maker in the scientific community declined.

CHAPTER NINE

CONCLUSION

Recent trends in history and sociology of science have been towards an analysis of the knowledge claims of scientists and their method of fact-construction. Bruno Latour, for example, has endeavoured, with some success, to show that all scientific "facts" are socially constructed.[1] This thesis has differed in approach from the work of Latour in that it has considered not the process of knowledge construction itself, but the claims men of science made of possessing knowledge which was valuable and worthy of praise. In other words, this work has not been concerned with whether or not science aims at or achieves "objective" truth, but with the ideology of the philosophers, who assumed that they did produce such truth and for whom it was only a matter of persuading everyone else that their activity was valuable and that they performed a vital social role.

Latour deals briefly with the nineteenth century in order to show that, until later in the century, doing science was not a "job", and although he refers only to Charles Lyell and the discipline of geology, his analysis is more widely applicable and indeed relevant to the argument here.[2] The aim in this thesis has been to account for the decline of the status of the instrument maker in the scientific community, and at the outset three possible contributory factors were postulated to account for this decline. Throughout the case studies, however, it has been apparent that the most significant factor in the process was the construction, by those in power, of a new ideology of what

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[1] B.Latour, *Science in Action. How to Follow Scientists and Engineers through Society,* (Milton Keynes, 1987).

[2] *Ibid.,* pp.146-50.

exactly a member of the scientific community should be, i.e. that an application of existing principles by, for example, an instrument maker, was not sufficient.

In this context Latour's analysis of Lyell's motives is most illuminating, for it shows that Lyell did not aim to "be a geologist". Rather, he merely wanted to be able to study the history of the earth.[3] More generally, the subjects of the case studies of this thesis, it should be realised, did not wish to become "scientists". The word *scientist* did not even exist until the mid-1830s.[4] Rather, they wished to create a society in which to be seen to practise science was a guarantee of being valued by one's country, in that science contributed to a nation's prosperity and well-being.

The nineteenth century, then, was a period in which there was an increasing sense of self-consciousness among men of science of their identity as a class and as a valuable group in terms of national wealth and prestige. Much of the rhetoric of individuals such as Babbage, Airy, Wheatstone and Faraday centred on this claim that the scientific man deserved a special recognition by society for his contribution to it. Chapter 7 has shown that this self-consciousness spread to British Association members with lower public profiles than the four individuals mentioned, and that the B.A.A.S. may be seen as the institutional manifestation of the motives of the emergent scientific elite. Indeed the men of science, with the Association as focus,

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[3] *Ibid.*, p.146.
[4] S.Ross, "Scientist: Story of a Word", *Annals of Science,* 1962 (18), pp.65-85.

believed that they had fully emerged as an elite group in society. At the same time, they considered that society itself had failed hitherto to realise this.

It was this ideology of the men of science as to what constituted status in the scientific community and in society that tended to exclude from the class other participants in the scientific enterprise such as instrument makers. The rhetoric of the philosophical class emphasised their importance to national life, particularly (as it had wide appeal) in financial terms. In an age of expansion in the commercial realm, the fact that men of science were able to stress that scientific discoveries, if successfully applied, had great economic potential, served as an important political tool in their quest for recognition of their activity by society.[5] It is important to note, however, that the man of science, while accepting such justifications to be worth emphasising, did not necessarily have a full commitment to utilitarian doctrines. The scientific elite was too heterogeneous for that to have been the case. But demonstrating that their activity aided in the fulfillment of objects of political economy such as providing a plentiful revenue or subsistence for the people, and supplying the State with a revenue sufficient to maintain the public services[6] - gave them an argument for their worth.

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[5] R.A.Stafford, *Scientist of Empire. Sir Roderick Murchison, Scientific Exploration and Victorian Imperialism,* (Cambridge, 1989), esp.pp.189, 207.

[6] These objects are stated in A.Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations,* (London, 1776). New edition, edited by R.H.Campbell, A.S.Skinner and W.B.Todd, (Oxford, 1976), p.428.

Individual men of science, such as Babbage and Airy, stressed the importance of the application of scientific discoveries and principles in order to transform society. As Chapman points out in the case of Airy, the *profit* which the man of science gained from his work was in terms of scientific prestige and public utility, not financial,[7] and a concern of the analyses of Rudwick,[8] and Morrell and Thackray[9] has been to emphasise a desire amongst gentlemen for personal aggrandisement as the primary motive for pursuing activities such as science in the early nineteenth century. This set the philosopher apart from the other links in the chain of application - the industrialists, engineers and indeed instrument makers - whose main goal, as has been shown, became not scientific status but financial profit. The individuals in power in the scientific community in this period were not motivated by financial reward, and thus these gentlemen were likely to give short shrift to anyone who deliberately tried to accumulate wealth directly through science. Even the behaviour of Wheatstone in taking out patents could be viewed, by some of the more conservative elements in the philosophical elite, as dubious

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[7] A.Chapman, "Sir George Airy and the Concept of International Standards in Science, Timekeeping and Navigation", *Vistas in Astronomy,* 1985 (28), pp.321-8, on p.321.

[8] M.J.S.Rudwick, *The Great Devonian Controversy. The Shaping of Scientific Knowledge among Gentlemanly Specialists,* (Chicago, 1985), esp.pp.17-18.

[9] J.B.Morrell and A.Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science,* (Oxford, 1981), esp.pp.423-5.

conduct for a gentlemanly philosopher.[10]

Although those who constructed the ideology of the elite eschewed financial motives, and sought only to consolidate the prestige of their class, it would not be correct to think that all those philosophers doing science in this period were so noble in their motives. If we consider groups peripheral to the elite, such as the electrical practitioners who formed the short-lived Electrical Society of London in 1837, the interest in self-promotion for financial gain becomes more direct. In this sense, where an elite philosopher did not grant scientific status to a mere tradesman out to do business, but not to contribute directly to national wealth and prestige, with an electrical practitioner such a gulf did not exist. Therefore one would expect the instrument maker to be able to play a somewhat greater role in the Electrical Society than in the higher echelons of the scientific community. Practitioners such as William Sturgeon, for example, as Morus shows, can be seen to have been tradesmen in much the same way as instrument makers such as Edward Montague Clarke, in that their trade was the giving of lecture-demonstrations to paying customers.[11] Sturgeon's motives for performing electrical research, then, did not involve a wish to contribute usefully applicable principles, nor to increase

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[10] William Thomson was also subject to criticism for his entrepreneurial activities: C.W.Smith and M.N.Wise, *Energy and Empire. A biographical study of Lord Kelvin,* (Cambridge, 1989), p.807n. The general distaste of the dons at Cambridge for "economic man" is noted in *ibid.,* pp.116-20.

[11] I.R.Morus, *The Politics of Power. Reform and Regulation in the work of William Robert Grove,* (Ph.D. Thesis, University of Cambridge, 1989), pp.13-18.

his status and that of men of science as a class, but were aimed at ways of enhancing his marketability as a lecturer. Therefore his work centred on areas such as increasing the power and efficiency of batteries and producing more impressive display phenomena, so that if he were successful in his research, the attendances at his lectures would rise.[12]

It can thus be seen that Sturgeon's enterprise was very little different from Clarke's: both made money from electricity, Clarke constructing electrical machines and developing them practically, and Sturgeon concentrating on the more philosophical aspects but with the same end in view. It was therefore to be expected that the ideology of the metropolitan electrical practitioners would not be damaging to instrument makers such as Clarke, in terms at least of the status one was able to hold in the electrical community. Indeed, Clarke was able to play a significant part in the life and work of the Electrical Society, and meetings were even held at his premises in Lowther Arcade.[13] However, there was still a sense in which the philosopher, Sturgeon, did something *prior* to the instrument maker, Clarke, as the former was able to make *discoveries,* which the artisan was unable to do. That Sturgeon considered himself a philosopher, and therefore of higher status than an artisan, and that he was keen to protect intellectual property could be seen in a postscript of his to a paper of Clarke's in which the maker,

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[12] *Ibid.*
[13] *Ibid.,* pp.26-7.

as with the electrical machine discussed in Chapter 3, seemed to have claimed an invention as his own in whose genesis he had little part:

> It must at all times be gratifying to philosophers to observe that their discoveries and inventions are of sufficient importance to demand the attention of others, and to inspire, in instrument makers, a spirit of emulation to excel in their attempts to improve even the most trifling mechanical arrangement. On the other hand, it must be allowed that there is a respect and gratitude due to philosophers, which those who profit by their discoveries are but too seldom disposed to acknowledge. Notwithstanding the trivial alterations he may think proper to make, the fame of the instrument maker could not possibly suffer by a candid acknowledgement of the source of his information, on any point whatever, relative to the original invention: and the *least* degree of courtesy that could be expected from him would be to permit the discoverer, or inventor, to have a priority of publication. Mr.Clarke will observe, however, that we have not hesitated to give publicity to his paper without the slightest alteration, but it is our duty to state, that Mr.Clarke's method of opening and shutting the battery circuit is similar to that employed by Mr.Barker... and that the original employment of the spur-wheel and mercury for this purpose was by Mr.Page.[14]

This shows that the ideology of the philosopher as to what constituted *real* innovation in science was fairly universal, and that it did not encompass the technical refinements made by instrument makers. Thus, although the instrument maker had an important part to play in the enterprise of the metropolitan electrical practitioners, there was a sense in which those such as Sturgeon, though promoting a trade themselves, saw the

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[14] W.Sturgeon's editorial postscript to E.M.Clarke, "Description of an arrangement of a series of the Sustaining Voltaic batteries, so as to obtain quantity or intensity. Also of an apparatus to give shocks by a single voltaic pair", *Annals of Electricity,* 1836-7 (1), pp.499-501.

instrument maker as of lesser status because of the limited nature of the work he was capable of performing. The notion of what determined *real* scientific work, i.e. the work of a philosopher, while held implicitly by those who saw themselves as members of the scientific elite, had also been explicitly stated by Adam Smith. In referring to the discovery of scientific principles, Smith stressed:

> When an artist makes any such discovery he showes himself to be not a meer artist but a real philosopher, whatever may be his nominal profession. It was a real philosopher only who could invent the fire-engine, and first form the idea of producing so great an effect by a power in nature which had never before been thought of. Many inferior artists, employed in the fabric of this wonderful machine, may afterwards discover more happy methods of applying that power than those made use of by its illustrious inventer...[15]

This passage, though left out of the final version of Smith's *Wealth of Nations,* can be seen as an encapsulation of the ideology of the philosopher with regard to his work, and of his attitudes to the labour of "meer" artists. Even so, as has been emphasised earlier in this thesis, the instrument maker at the time the *Wealth of Nations* was written was able to show himself to be a real philosopher, and in individuals such as Edward Nairne and John Dollond the discovery of new principles went hand in hand with construction of instruments. By the nineteenth century, however, the instrument maker no longer made the kinds of advance which pressed his claims to be seen as a real philosopher, and owing to the carefully constructed

[15] Adam Smith's Early Draft of the *Wealth of Nations,* cited in W.R.Scott, *Adam Smith as Student and Professor,* (Glasgow, 1937), p.338.

ideology of those who did consider themselves to be such, was denied full membership of their community. The dominant role in the development of instruments came to be played by those men of science who had occasion to use them, and who were therefore more fully aware of what was needed in a new instrument than those who constructed it.

In this context, the interest in instrumental development on the part of philosophers had several different facets. Morrison-Low, for example, has divided David Brewster's concerns with optical instruments into four areas.[16] Firstly, he was able to *improve* various instruments, such as micrometers, telescopes, and microscopes. Secondly, he *invented* new devices like the kaleidoscope. Thirdly, his interest led him to work in areas such as patent reform, whereby those who provided the intellectual labour of invention and development would be adequately rewarded, and he also served on Exhibition juries, thus controlling the perceptions of the merits of invention and workmanship attributed to individual philosophers and artisans. Finally, Brewster was able to encourage makers such as Alexander Adie by employing them to develop prototypes of his new instruments and, by his patronage, enhancing their business.[17]

These four areas of activity concerning instruments and their makers typify the sense of control which was exerted by members of the philosophical elite over instruments and their

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[16] A.D.Morrison-Low, "Brewster and Scientific Instruments", in A.D.Morrison-Low and J.R.R.Christie (eds.), *"Martyr of Science": Sir David Brewster 1781-1868,* (Edinburgh, 1984), pp.59-65, on p.59.

[17] *Ibid.*

makers in the nineteenth century, an extension of the belief of the man of science in his own expertise and authority. Although, as has been shown, in nineteenth-century Britain an increased division of labour and an expanded market caused the maker to have less time to devote to invention and development, there was undoubtedly a sense in which the attempts at control made by philosophers such as Brewster, Babbage and Airy prevented such a role being taken by the artisan. In a society in which the emergent philosophical elite emphasised their authority and pre-eminence, with an increasing sense of their identity, the artisan tended to be excluded not only from their community, but from being perceived as the key link in the chain of the production of instruments as well.

This process could be very easily explained by claiming that an industrial society is of necessity a professionalising society,[18] and that therefore in nineteenth-century Britain the rise of a professional class devoted to advancing scientific principles and their applications was inevitable. In this framework the division of labour would ensure that an instrument maker became unable to contribute to the work of the new professional class of scientists, and thus could not possess status in their community. However, not only is such a deterministic approach not helpful, but it is also the case that the professionalisation model is inaccurate. The aim of men of science in this period was not (as shown particularly in Chapter 7) to professionalise their activity. As Morrell

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[18] A.M.Carr-Saunders and P.A.Wilson, *The Professions,* (Oxford, 1933), p.297.

suggests, the British Association, which has been considered here as the institutional manifestation of the aspirations of the men of science as a group, deliberately ignored matters concerning professionalisation, such as the creation of full-time paid posts by government.[19] His argument maintains that the motives of the philosophers in the early existence of the B.A.A.S. centred on career interests, intellectual property, and collective exploitation, and not on ambitions to make a profession of their activity.[20] The argument in this thesis has supported this view. In the first half of the nineteenth century, although a growing sense of identity among the men of science caused criteria for membership of their community to be refined to exclude groups peripheral to their enterprise, this did not entail a general wish to create the profession of "scientist".

Although this rise of self-consciousness was linked to societal forces and thus to the Industrial Revolution, it is not true to say that industrialisation and concomitant division of labour *caused* the increase of class identity, though it did have a similar *effect* to the latter in reducing the status of the artisan in the scientific community. By the time a profession of "scientist" could be argued to exist, a different relationship between philosopher and artisan subsisted, and it was no longer on the agenda that an instrument maker be a member of the scientific community. Thus a study of the nature of practical

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[19] J.B.Morrell, "Professionalisation", in R.C.Olby, G.N.Cantor, J.R.R.Christie and M.J.S.Hodge (eds.), *Companion to the History of Modern Science,* (London, 1990), pp.980-9, on p.987.

[20] *Ibid.,* p.988.

collaborations between professional men of science such as William Thomson and instrument makers such as James White would form a suitable subject for further research, in the light of the analysis offered here. This work, however, has been concerned with the claims to knowledge and authority made prior to the establishment of science as a profession, and as such provides a necessary foundation for future work on the man of science/instrument maker relationship.

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